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Philosophical Magazine

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THE

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AND

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CONDUCTED BY

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AND

WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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VIII. Illustrative of Mr. H. Wilde's Paper on some Improvements in Electromagnetic Induction Machines.

ERRATA.

Vol. 44, page 522, line 5 from bottom, *for* therefore *read* therefor.

Vol. 45, page 160, *for* paragraph 3 of Mr. Hudson's paper commencing "This point" &c.) *substitute*:—

This point can be easily tested experimentally as regards sound. Thus a tense string with amplitude of vibration = 2 ought to become inaudible at twice the distance at which it ceases to be heard with amplitude = 1, if the *square* of the amplitude be the correct assumption. The relative distances at which the sounds should be inaudible ought to be as 1 to 0·70715, if the simple amplitude (not its *square*) be correct.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
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JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JANUARY 1873.

1. *On Manometric Flames.* *By* Dr. RUDOLPH KÖNIG (*of Paris*)*.

[With Two Plates.]

IN the beginning of 1862 I invented a new method of observation, which had for its object to make apparent the sounding air-waves, or, what is the same thing, the changing density of the atmosphere while penetrated by sounding vibrations, or while itself in a state of vibration, in the same way as acoustic experiments were able to show clearly the vibration of bodies which produce the vibration of the atmosphere.

The first apparatus founded on this method was shown in the London Exhibition of 1862; and since that period I have invented a whole series of apparatus on the same principles: a short description of some has appeared in Poggendorff's *Annalen* for 1864; and others are briefly sketched in my Catalogue of 1865.

The following pages are designed to explain all these apparatus, as well those which have been added since the publication of my Catalogue, as also the experiments in connexion with them.

The small instrument, on the use of which my method is founded, and to which I have given the name of *Manometric Capsule,* consists of a cavity in a wooden plate, whose orifice is closed by a thin membrane. Illuminating gas may be introduced into this cavity through a pipe—a second pipe, terminating in a gas-burner, giving means for exit and ignition.

Now, if the air before the membrane be rendered suddenly of a greater density, the membrane will of course be driven inwards, and thus expel the gas and cause the flame to rise quickly. If, on the contrary, the air be suddenly rarefied, the membrane becomes drawn outwards, the space within momentarily increased, the gas expanded, and the flame lowered.

---

* Translation, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlvi. p. 161.

A membrane is known to possess, like every other elastic body, only a definite series of notes; and thus we should suppose that the manometric capsule would only show an effect when the note acting upon it agreed with one of the notes of its membrane.

But this is not the case; for besides the vibration which a body makes under the influence of its elasticity, any motion whatever can be forced upon it if only the active force be much greater than the resistance which it can offer.

For example, let us take a long thin string, tuned to the fundamental note of 100 vibrations, and place its centre in firm connexion with the prong of a strong massive tuning-fork of 110 vibrations; it will then clearly move to and fro 110 times in unison with the vibrations of the tuning-fork, although in accordance with its nature it could only execute 100, 200, 300, &c. vibrations. In point of fact it does not truly vibrate, but is only mechanically drawn to and fro. This is also the case with the manometric capsule, as it is so constructed that the resistance offered to the condensation and rarefaction of the atmosphere must be considered very trifling, indeed almost nil. One and the same capsule is thus equally effective for every note; also different capsules, whose membranes have *not* been tuned in unison, nevertheless give the same results under the influence of the same note.

If out of several capsules which are fed by the same gas-reservoir you set *one* in activity, the flames in all the others are set in motion. Thus, if the membrane be pressed into the capsule, the pressure will not only drive the flame higher from the exit-pipe, but will also spread its influence through the entrance-pipe to the general reservoir, and thence to the other capsules, the flames of which become prolonged, although in a less degree. Of course, a pressure in the contrary direction produces an opposite effect. If, therefore, several capsules are to be employed at the same time, this mutual influence must be annulled.

I at first sought to attain this end by placing between the reservoir and the capsules long thin india-rubber tubes; but this did not act quite satisfactorily.

I attained my object, however, by the use of accessory capsules, through which I permitted the gas to pass before I conducted it into the manometric capsule; they are constructed like the others, each consisting of a cavity closed by a thin membrane.

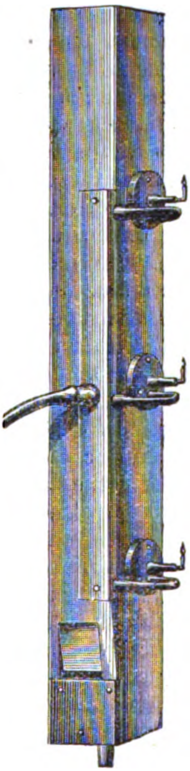
If the pressure derived from the manometric capsule pass through the entrance-tube towards the gas-reservoir, it will be annulled when entering the accessory capsule by the yielding of the membrane.

Practice shows that we may put into the strongest motion one of several flames isolated in the foregoing manner without in any way affecting the rest.

Proof of the different condition of the Air in the Nodes and Ventral Segments of a sounding Air-column.

In order to show the changing density of the air in the nodes and its fixed condition in the ventral segments of a sounding air-column generally, I make use of an open organ-pipe, which is so constructed that either its fundamental tone or its first harmonic or overtone, the octave, can be sounded at will (fig. 1). At the node of the fundamental and the two nodes of the octave are three orifices in one side of the pipe; over these three manometric capsules are so placed that the orifices are exactly closed by the membranes, being of the same diameter; a common reservoir, provided with accessory capsules, feeds the three flames, the length of which can be regulated by cocks.

Fig. 1.



If, now, we give to the three flames an equal height of 15 to 20 millimetres, and sound the octave, then the two exterior flames will be put into such violent motion that they will appear prolonged, narrow, quite blue, and without illuminating power, on account of the considerable amount of air which they draw with them in their flickering up and down, whilst the middle flame will remain almost still and bright, being placed at the centre of a ventral segment, where the air is only gliding to and fro.

At the sounding of the fundamental the middle flame is at the node, and therefore violently agitated; the two exterior ones, which are then between the node and the centres of the ventral segments at the ends of the pipe, show only a weaker motion. As in this case it is only a question of different intensity of motion in the individual flames, it is better here to make use of smaller flames, when the middle one becomes quite blue, while the exterior ones remain bright. If we give the flames the length of only 8 to 10 millims., on sounding the fundamental the middle flame will be extinguished, on sounding the octave the exterior ones will disappear.

These experiments may also be made with a closed organ-pipe which can be sounded on its fundamental and its first overtone. One of the flames must then be at the end of the pipe, where the node of the fundamental, as well as one of the nodes of the overtone, are found.

If the flame be shortened, on sounding the fundamental the end flame will be the first to go out, and then the middle one, because the latter is nearer to the node than to the ventral segment in the mouth of the pipe.

But on sounding the first overtone, the 12th of the fundamental, the middle flame remains unchanged, while the two exterior ones become extinguished.

Comparison and Combination of several Tones.

These experiments have only shown the general working of whole series of consecutive vibrations; if, however, we allow the flame to be reflected by a rotating mirror, we see all phases of their motion side by side, and we can then not only examine the number of vibrations and the ratios of different tones, but also observe the images made by the combination of several tones.

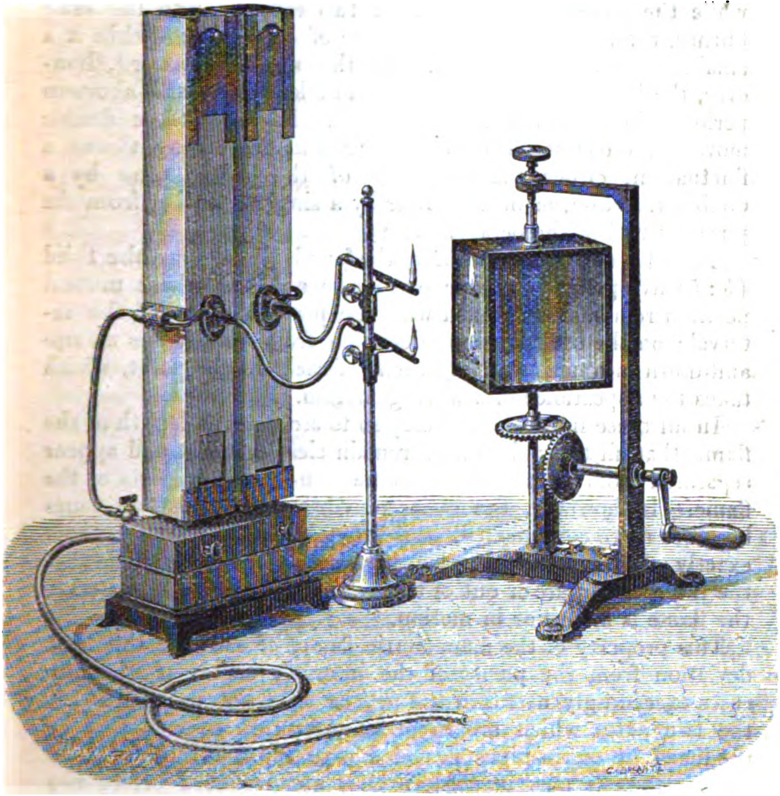
The apparatus which serves for these investigations consists of a set of organ-pipes, each of which is provided at the node of its fundamental with a manometric capsule. This can be connected by means of an india-rubber tube with gas-burners, which are placed on a special stand (fig. 2).

Before these gas-burners there is placed a revolving mirror, made of four glass plates coated with platinum. The platinum surfaces are turned outwards, in order to avoid the confusing double images of the common mirrors, which is caused by reflection from the two surfaces of the glass plates. A small wind-chest, for the reception of two organ-pipes, has two mouth-pieces, the larger of which serves to conduct the air from a bellows. Through the smaller one the gas is conducted to a common receiver, provided with two cocks, which are joined by means of india-rubber tubing to the capsules of the organ-pipes.

The reflection of a flame at rest shows in the rotating mirror a band of light of the width of the height of the flame. If we, however, sound the organ-pipe in connexion with it, there appears in place of the band of light a series of regularly consecutive flame-pictures, the tips of which are bent in a direction opposite to that in which the mirror is moving.

If we place two burners in such a position that their reflections give two bands of light, one above another, and connect them with two organ-pipes which together give the interval of the octave, the series corresponding to the higher tone gives double the number of flames that the other one does, by which the vibrations are shown to be in the ratio of 1 : 2 (Pl. I. fig. 3). If we take organ-pipes of other intervals, we get with the fifth three flames above two, with the fourth four above three, and so on.

Fig. 2.



The rapidity of the motion of the flames allows their reflections in the mirror to be very sharply defined; but as they are of very short duration, it would be difficult in this experiment to observe trifling deviations from the purity of the intervals; for although in point of fact it is easy to recognize that in one of the series there are almost always two flames when there is one in the other, yet it would be difficult to discover that about 200 in the one series coincide with about 101 in the other. These exact observations can be made with the greatest facility, however, if we make the two capsules of the two corresponding organ-pipes act on the same flame.

If we sound two organ-pipes, exactly tuned to an octave, while the gas streams from their two capsules into the same burner, the flame has the appearance of containing within it a smaller one without motion. By the slightest discord, however, the latter becomes flickering, and lengthens and shortens periodically within the greater one. Each of these double movements composed of ascending and descending shows a fluctuation, either the deviation of the upper tone by a double vibration, or of the lower by a single vibration from the pure interval of the octave.

The fifth (2 : 3) shows three, the fourth (3 : 4) four, the third (4 : 5) five points of flame one above another, whose mutual position remains unchanged with the perfect purity of the interval; on the contrary, any deviation from this causes an up-and-down movement among them of each single point, which takes the appearance of a waving motion.

In all these intervals it is easy so to arrange the length of the flame that all the points may remain clearly bright and appear separated from each other by blue non-luminous parts of the flame. If, however, the ratios of vibration of the two tones becomes more complicated, it is often difficult to observe them exactly; but even in this case the flame shows plainly whether the interval be pure or out of tune, as we have but to see whether the flame is at rest or in motion.

This property of the manometric flame, of showing the least deviation from the purity of the interval, makes it in many cases exceedingly useful in tuning, as it is not necessary that the two notes which are to be brought into tune should be produced by organ-pipes provided with capsules: the notes of any instrument may be used, if they are produced before two resonators in relation with them, which act on two manometric capsules whose gas-pipes end in the same burner.

The ratio 1 : 2 is the most convenient, on account of its easy examination; so that if we want to tune a series of tuning-forks to the same note, it is better to choose the fork for comparison an octave lower or higher.

If we wish to observe the whole process of vibration in the above-mentioned flames on which two notes act at the same time, we must again employ the rotating mirror. The pure octave shows in it a series of flames, in which a shorter always follows a longer, and the shorter ones have all, like the longer ones, equal heights (fig. 4, Pl. I.). If any *beats* occur, the summits of the smaller as well as of the larger flames move up and down. However, these motions are opposed; so that in those positions where the long flames are at their longest, the short flames are at their shortest, and *vice versâ*.

In fig. 4, Pl. I. the picture of the seventh (8 : 15, or 8 : 16 − 1) shows this process, although in a very short period. The fifth (2 : 3) shows a period of three, the fourth (3 : 4) of four, the third (4 : 5) of five, and the second (8 : 9) of nine in the range of the increasing and then decreasing flames.

If the proportion is not of the form $n : n \pm 1$, then there takes place in the whole period not only a rise and fall of the flame-summits, but the curve connecting them shows as many elevations and depressions as the difference between the two ratios. For example, see the picture of the sixth (3 : 5) (fig. 4, Pl. I.).

The more complicated the interval of the two notes, the more carefully we must bring it into perfect purity of tune, until no further movement whatever can be discerned in the flame, because otherwise the recurring periods of the flame-pictures in the mirror suffer continual change by the change of phase, and in that case it becomes difficult to recognize them. But this exact tune becomes still more imperative if we wish to combine more than two notes while making them act on one flame. It will be remarked, besides, in these experiments, how difficult it is to retain absolutely constant notes with organ-pipes, even when we make use of a well-regulated bellows.

Coexistence of two Tones in the same Air-column.

The investigation of the combination of two related tones in one flame-picture is especially useful, because it teaches us from the flame-picture of a combination of tones, of which the components are unknown, to find the single tones of which it is composed.

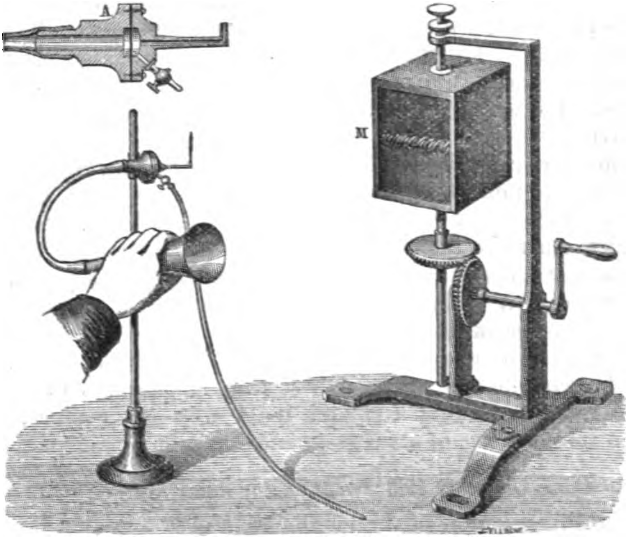
A passage to the trial of such a combination of tones as, *e. g.*, each sound offers, is the combination formed by a fundamental with a known overtone in the same air-column. Very suitable for such an experiment is the above described closed organ-pipe with three flames, since the node of the fundamental as well as one node of the first overtone are situated at their ends.

If we blow the fundamental (1) very gently, the flame-picture in the mirror shows the vibrations of this tone; if now we blow the overtone (3) strongly, each single vibration will be replaced by three flames. With a rather weaker blast both tones are produced together, and we always see three flame-summits over every fundamental flame (fig. 5, Plate I.). Therefore several tones present at the same time in the air-column give exactly the same flame-picture as the combination of the same tones when each is produced by its own particular organ-pipe.

Representation of Sounds.

The apparatus which is used for the representation of sounds consists simply of a manometric capsule, before the membrane of which there is a small cavity terminating in a short tube (fig. 6). The sounds to be represented must be conducted into this cavity with the smallest possible loss of their intensity and without undergoing any change in their passage.

Fig. 6.



The sound-pictures of the combined tones of the same instrument are never all alike, but the deepest tones always show much larger and more complicated flame-groups for each single vibration of the fundamental than the higher ones, because the high harmonic tones, which are to be heard in the sound of the deeper tones of the instrument, disappear more and more as the fundamental ascends. Thus the higher the tone the smaller in comparison are the dimensions of the means which produce it. The vibrations of all resounding instruments, however, take a simpler form if the dimensions of the latter are very small, because the different bodies lose their capacity of forming subdivisions in vibration, by which the accessory tones, if not exclusively, yet in many cases are chiefly produced.

A second reason, however, and that a very potent one, is this. If the tones are produced not so much by the elastic vibrations of a body as rather by gusts of air, as in the siren and pandean pipes, the upper notes which are contained in the sound of a lower note have so high a place in the scale for a high note that they produce no effect, either on the ear or on an artificial membrane.

The lowest note of the violin, for example, is *g* (192 vibrations), and its 8th harmonic $\overline{\overline{\overline{g}}}$ (1536 vibrations) is within the range of the instrument. It is produced on the G-string by a length of 4, and on the E-string by about 13½ centimetres. Nevertheless, if we take this very $\overline{\overline{\overline{g}}}$ as fundamental tone, the length of string of its eighth harmonic on the E-string would be about 17 millimetres; and besides, with 12,288 vibrations, it would be already nearly two octaves above the highest notes used in music, which sufficiently explains why it is not heard in the sound of $\overline{\overline{\overline{g}}}$.

My success was but partial in the representation of violin sounds, owing to the high position of the notes of the instrument, since, with the exception of the notes from *g* to $\bar{c}$ on the G-string, I obtained only the fundamental vibrations for all the rest. In my endeavours to conduct the notes as loud as possible to the membrane I tried two methods. First, I connected the interior air of the violin with the small apparatus, by means of an india-rubber tube which I introduced into one of the *f*-shaped apertures of the violin; and secondly, I pressed my stethoscope with its concave membrane on the bottom-piece of the violin, precisely under the sounding-post, and attached the india-rubber tube to the flame-apparatus. The results in the latter case were as follows.

On the G-string *g* showed the figure of the octave in weak wave-formed flames, which, as far as *b*, rose to sharply defined clearly cut flames. With $\bar{c}$ the latter fell quite suddenly into one single broad, short, and faint flame, in which I could perceive only the smallest trace of the octave when played forcibly. Already the D-string only showed simple flame series, which for $\bar{d}$ $\bar{e}$ $\bar{f}$ $\bar{g}$ were rounded, wave-like, and weak, but on playing $\bar{a}$ became again stronger. The $\bar{a}$ on the A-string gave very high and deeply cut flames, $\bar{b}$ still stronger ones, which fell, however, at $\overline{\overline{c}}$ and became quite weak. Up to $\overline{\overline{g}}$ and $\overline{\overline{a}}$ on the E-string every trace was lost of the small flame-points which had appeared at the last overtones.

On the connexion of the interior air with the apparatus, the insensibly graduated picture of the octave from *g* changed into a single sharply defined flame at *b*; this attained such an extraordinary height at $\bar{c}$, as though it had been produced by the vibrations of an organ-pipe provided with a capsule at the node.

The note $\bar{d}$ also showed a series of high and sharply defined flames, which, however, quite disappeared at $\bar{e}$ to give place to the weak rounded-off waving lines as far as $\bar{a}$.

This sudden appearance of very high flames in the region of $\bar{c}$ is explained by the circumstance that the lowest proper note of the interior air of the violin is precisely $\bar{c}$. For the upper notes I obtained the same result as with the stethoscope; that is to say, the notes $\bar{a}$ and $\bar{b}$ again gave much stronger vibrations than $\bar{e}\ \bar{f}\ \bar{g}$, and than the upper $\bar{\bar{c}}\ \bar{\bar{d}}\ \bar{\bar{e}}$ &c.; so that the second peculiar note of the interior air, or rather of the whole system formed by the violin, seems to be in the region of $\bar{a}$ and $\bar{b}$.

With regard to sound, we have in this case certainly been able only to make evident the transition from the figure of the octave to that of the simple note. The siren shows much better the gradual disappearance of the higher upper note from the musical sounds when their fundamental tone is raised. To this end I intercept the impulse above the open perforated plate by means of an arched aperture which expands into a small tube, and is placed immediately above a part of the apertures so as to permit them to affect the flame, while I cause the rotation of the plate to increase from its lowest to its highest swiftness by increased pressure of the air. The mirror then shows at the lowest notes very large and dense flame-groups; these change towards the middle of the great octave into more clearly defined and deep-slit waves, with at first five, then towards *c* and *d* with four flame-points. At *g* and *a* the number of the points falls to three, at $\bar{c}$ and $\bar{d}$ to two; and at $\bar{a}$ the last trace of the octave disappears from the sound; after this all the still higher notes only show single flame-pictures.

But the result of this experiment is essentially different if when a sounding-chest is fastened over the perforated plate. It first intensifies the upper harmonics of the sound, then the lower, and lastly the fundamental itself: this causes the flame-groups no longer to become simpler gradually and in accordance with the height of the notes, but to show rather sudden changes alternately rising and disappearing. Thus the sound of a siren, over the perforated plate of which a resonance-box giving the note $\bar{\bar{c}}$ was placed, after showing a few complicated and faint pictures when the plate was slowly rotated, produced on reaching the pitch *c* clearly a large flame in agreement with the fundamental tone: this flame had four summits, derived from overtone 4, which coincided with the proper note of the sounding-chest. On turning the plate more rapidly, the flame-picture became simpler, until at *f* it became one single flame, so that the overtone 3 must be quite wanting in this sound of the siren. The ascending scale had scarcely passed *f* when there appeared between each two large flames a small but sharply defined flame, which quickly increased in size, and towards $\overline{c}$ reached nearly the height of the chief flames, where the effect of the resonance-box confirmed the fact of its being the overtone 2 of the sound of the siren. Above $\overline{c}$ the smaller flame leant always more towards the larger one, until at $\overline{a}$ it completely disappeared in it: after this again only single flames appeared (Pl. I. fig. 7).

In order to make the sound in these experiments act strongly on the capsule, I provided the resonance-box with a tube, and placed its interior in direct connexion with the flame-apparatus. These experiments, in which the air-impulses of the siren are prevented from passing immediately into the atmosphere, being compelled to pass through a resonator which remains unchanged for all the fundamental tones of sound, give a visible picture of the process of the formation of vocal sounds; for it is known that the air contained in the cavity of the mouth, when speaking or singing the same vowel in different tones, is always tuned to the same note, so that the mouth must act on the air-waves produced in the larynx in the same way as the sounding-chest on the air-impulses of the siren. Nevertheless the series of flame-pictures of the same vowel, sung in the tones of two octaves, does not show such sudden changes as might have been expected without closer research.

In order to produce the pictures of the vowels, I sing them into a small funnel-shaped mouthpiece which is connected with the cavity before the membrane by a short india-rubber tube; thus they reach the capsule with great intensity (fig. 6).

I had already in 1867 sketched and had painted the pictures of the vowels *u, o, a, e, i* sung to the notes of the two octaves from C to $\overline{c}$. I proceeded in the following manner. In order to be sure that I had not changed the character of the vowel in the transition from one tone to another, I first verified the proper note of the mouth with the tuning-fork; then, while I sang into the apparatus, an artist drew the picture which he saw in the mirror. I also drew the same picture independently: and if both our drawings were identical they were looked upon as correct; if, however, there were discrepancies, I repeated the experiment until the error was discovered.

The five finished drawings (Pl. II. fig. 8) were unfortunately too late for the Exhibition; but I was able to exhibit them at the Meeting of the Association of Natural Philosophers at Dresden in 1868. I delayed their publication until now because I wished to revise them with precision, but was always prevented by the delicate state of my throat, which did not permit me such fatiguing experiments. But now, since I can no longer hope to recover, I have used my best endeavours to make the pictures correct, and give them forth, not indeed as absolutely perfect, but as nearly so as it was possible for me to make them.

The drawings themselves are much more difficult than might be supposed, particularly of the large flame-groups of the deeper notes, not only on account of the evanescence of the pictures, but also because the flame-summits do not always follow each other, but are partly situated beneath one another, so that it appears as though different flame-groups were intermingled, or rather pushed partly one before another. But these flames, whose background, so to say, is formed by other flames, easily escape observation, particularly if the back ones are not so high, nor the front ones so low, that the bright summits of the latter stand out upon the blue lower parts of the former. We can indeed, by a rapid rotation of the mirror, separate all the summits from each other; but then the whole group becomes difficult to observe, on account of its great length and the great bending of the flames.

However imperfect the drawings may be on account of the absence of some details, yet they give true pictures of the general outline as portrayed in the mirror. If, for example, the vowel A be sung on the note C, the picture shows a group from which a tall bright flame rises near a smaller very blue one; after these come a whole mountain of regularly toothed flames. Now it is quite possible that this ridge has really 9 summits, whilst I have only drawn 8; for it has sometimes appeared to me that there were more than that number on days when I produced this very low note stronger and purer than usual; but this does not change the character of the whole group, which could never be mistaken for that of U, O, E, or I sung in the same note. In any case, therefore, these pictures appear to me sufficient for the representation of the great difference in the appearance of the sound of the five vowels, sung on the same note, as well as to show the manner of the change of the flame-pictures of the same vowel from one note to another. But this is the chief point, and indeed all that can be attained with certainty by the apparatus; for just on account of its great sensitiveness we must not hope for absolutely correct pictures. The details in the group change most remarkably, not only when the same vowel is sung in the same note by different voices, but also when the same voice gives vowel and note with different intensity. A very slight change in the condition of the voice is sufficient to effect great changes in the flame-pictures. For instance, when my throat is weary, instead of obtaining the picture as drawn of U sung on *e*, I get only a small flame and two tall broad ones, the last in place of two and two in the picture; and similar simplifications are made in all the flame-groups.

In order to see first what influence may be expected from the fixed notes of the mouth-cavity on the flame-pictures, I will give a general view by drawing for each vowel, sung in each note of the two octaves from C to $\bar{c}$, the harmonic overtone to which the characteristic note approximates, and the number of vibrations by which they differ.

For O, A, and E, I take the characteristic notes (given by Helmholtz) $\bar{b}$, $\bar{\bar{b}}$, and $\bar{\bar{\bar{b}}}$; but, differing from the former opinion of Donders and Helmholtz, I have found for U and I the notes *b* and $\bar{\bar{\bar{\bar{b}}}}$; so that the five chief vowels are all an octave distant from each other, and the characteristic note of the lowest vowel, viz. U, unites with the lowest note which it is possible for the mouth to intensify by resonance.

In the definition of these notes there is no question of an absolutely exact number of vibrations. If, for example, I find the most powerful resonance of the mouthpiece for U giving between 220 and 230 vibrations, I may take equally 224 or 225 vibrations as the characteristic note of U. I make this remark here particularly because, in a short address to the Paris Academy (April 25, 1870) on the before-mentioned definitions of the characteristic notes of U and I, I gave as the average simple vibrations for U, O, A, E, I, 225, 450, 900, 1800, and 3600—but after a subsequent revision, the equally correct numbers 224, 448, 896, 1792, and 3584. The former are indeed easier to retain, but they refer to no note in use; whereas the latter numbers show the vibrations of the seventh overtone of $\underline{C_1}$, C, *c*, $\bar{c}$, and $\bar{\bar{c}}$ ($\bar{c}$=256 vibrations.).

In the following Table the first column contains the vowel, the second the note sung, and the third and fourth the two overtones of the sound of that note between which the characteristic sound of the vowel falls, together with the number of vibrations by which one of these notes is lower and the other higher than the proper note of the mouth-cavity.

U 448	C	$3(g)$	− 64	+ 64	$4\overline{(c)}$	O 896	C				
	D	$3(a)$	− 16	+128	$4\overline{(d)}$		D	$6\overline{(a)}$	− 32	+112	$7\overline{(d)}$
	E	$2(e)$	−128	+ 32	$3\overline{(b)}$		E	$5\overline{(gis)}$	− 96	+ 64	$6\overline{(b)}$
	F	$2(f)$	−107	+ 64	$3\overline{(c)}$		F	$5\overline{(a)}$	− 42·6	+128	$6\overline{\overline{(c)}}$
	G	$2(g)$	− 64	+128	$3\overline{(d)}$		G	$4\overline{(g)}$	−128	+ 64	$5\overline{(b)}$
	A	$2(a)$	− 22	+192	$3\overline{(e)}$		A	$4\overline{(a)}$	− 42·6	+170·6	$5\overline{\overline{(cis)}}$
	B	1(B)	−208	+ 32	$2\overline{(b)}$		B	$3\overline{(fis)}$	−176	+ 64	$4\overline{(b)}$
	c	$1(c)$	−192	+ 64	$2\overline{(c)}$		*c*	$3\overline{(g)}$	−128	+128	$4\overline{\overline{(c)}}$
	d	$1(d)$	−160	+128	$2\overline{(d)}$		*d*	$3\overline{(a)}$	− 32	+256	$4\overline{\overline{(d)}}$
	e	$1(e)$	−128	+192	$2\overline{(e)}$		*e*	$2\overline{(e)}$	−256	+ 64	$3\overline{(b)}$
	f	$1(f)$	−106·7	+234·6	$2\overline{(f)}$		*f*	$2\overline{(f)}$	−213·4	+128	$3\overline{\overline{(c)}}$
	g	$1(g)$	− 64	+320	$2\overline{(g)}$		*g*	$2\overline{(g)}$	−128	+256	$3\overline{\overline{(d)}}$
	a	$1(a)$	− 21·4	+405·2	$2\overline{(a)}$		*a*	$2\overline{(a)}$	− 42·6	+384	$3\overline{\overline{(e)}}$
	b			+ 32	$1\overline{(b)}$		*b*	$1\overline{(b)}$	−416	+ 64	$2\overline{(b)}$
	$\overline{c}$			+ 64	$1\overline{(c)}$		$\overline{c}$	$1\overline{(c)}$	−384	+128	$2\overline{\overline{(c)}}$

A 1792	C			14			B 3584	C	28
	D	$12(\overline{\overline{\overline{a}}})$	− 64		+ 80	$13(\overline{\overline{\overline{\overline{c}}}})$		*c*	14
	E	11	− 32		+128	$12(\overline{\overline{\overline{b}}})$		$\bar{c}$	7
	F	$10(\overline{\overline{\overline{a}}})$	− 86		+ 85	11			
	G	$9(\overline{\overline{\overline{a}}})$	− 64		+128	$10(\overline{\overline{\overline{b}}})$	I 7168	C	56
	A	$8(\overline{\overline{\overline{a}}})$	− 86		+128	$9(\overline{\overline{\overline{b}}})$		*c*	28
	H	7	−112		+128	$8(\overline{\overline{\overline{b}}})$		$\bar{c}$	14
	c			7					
	d	$6(\overline{\overline{\overline{a}}})$	− 64		+224	7			
	e	$5(\overline{\overline{\overline{gis}}})$	−192		+128	$6(\overline{\overline{\overline{b}}})$			
	f	$5(\overline{\overline{\overline{a}}})$	− 85		+256	$6(\overline{\overline{\overline{\overline{c}}}})$			
	g	$4(\overline{\overline{\overline{g}}})$	−256		+128	$5(\overline{\overline{\overline{b}}})$			
	a	$4(\overline{\overline{\overline{a}}})$	− 86		+341	$5(\overline{\overline{\overline{\overline{cis}}}})$			
	h	$3(\overline{\overline{\overline{fis}}})$	−352		+128	$4(\overline{\overline{\overline{b}}})$			
	$\bar{c}$	$3(\overline{\overline{\overline{g}}})$	−256		+256	$4(\overline{\overline{\overline{\overline{c}}}})$			

The characteristic tone of U approaches therefore to the third overtone of D and E, to the second of A and B, and to the fundamental *a* and *b*; and in point of fact in the flame-groups of D and E we can perceive a forking into three, in those of A and B into two chief divisions; while the flame-pictures of *a* and *b* show a great preponderance in intensity of the fundamental over the accessory tones.

The characteristic tone of O does not approach any of the overtones of the sounds that are sung (except C) nearer than about half a tone; therefore it has but little effect on the flame-pictures. At *a*, where it approaches the second, and at *d*, where it approaches the third overtone, we perceive clearly the forking into two and three parts respectively; but the more complicated groups of A, F, and D do not show any particular prominence of the tones 4, 5, and 6. This was to be expected, as the air in the mouth cannot strongly vibrate if its pitch, as in this case, differs half a tone from the note already weak when sounded.

The characteristic tone of A approaches no overtone nearer than 16 vibrations, except at C and *c*, where it coincides with the 14th and 7th overtones. Nevertheless the pictures of C and *c* do not show the existence of the 14th and 7th overtones, probably because these notes are so high and weak in the larynx that they cannot vibrate the atmosphere in the mouth sufficiently to act on the flame.

The characteristic tones of the vowels E and I are too high to have any effect on the flame; and thus E sung on $\overline{c}$ shows but such a picture as does a fundamental distantly accompanied by its octave, instead of a group of seven summits.

I, sung on the same note, shows only a series of simple flames, which seem to indicate a simple tone. This simplicity of the flame-picture here, however, is only apparent, as in all the pictures of I.

The wide, large, and almost forkless flames shown by the different groups are really mostly whole tufts of flame, which appear somewhat confused when the note is weakly given; but when, on the contrary, it is blown with force, and particularly at the mouthpiece, numerous bright points may be clearly seen, which indicate the presence of very high accessory notes. It is very fatiguing to sing I, and when pitched low is so difficult that I was compelled to omit the sounds from C to F in the drawings.

I made an experiment to see whether the flame-picture would take any different form if I placed the tube, instead of before the mouth, at the back part of it, and then sang A on *f*; but, with the exception of increased intensity, the result in both cases was the same.

The whispered vowels had but a slight effect on the flame.

The bands of light in the mirror appeared under their influence like an alternately darker and lighter ribbon with irregular small teeth; and the whole was so cloudy and undefined that I could even discover no difference between the different vowels. The semivowels *m* and *n* gave such similar pictures that I could not distinguish between them. I have sketched them for the notes $\bar{c}$, *g*, *e*, *c* (fig. 9, Pl. I.); deeper notes showed longer, but still misty and undefined periods. Of course in these experiments I was obliged to put my nose instead of my mouth to the instrument. The quivering R, silently pronounced, shows a series of flame-summits of different elevations pretty regularly forked or toothed. In the small rotating mirror, with a plate 15 centims. wide, of which I generally make use, these summits appeared to me to follow each other irregularly. But when I employed a larger one, 40 centims. wide, I perceived the regular periodicity of the whole group, which was repeated four or five times in the width of the mirror. The teeth, which are spread over the whole of the flame-summits, are simply caused by the air-current. Of this we can easily convince ourselves by placing the tongue at a little distance from the gums instead of permitting it to vibrate on them, and then expelling the air violently through the narrow aperture. The band of flame then appears serrated, without any individual flame-summits rising above it. But if we intone the R, the picture of the note unites with that of the letter, and there ensues such a confused series of single flames and whole groups of dissimilar height and form, that in the evanescence of the picture it is impossible to decipher them. I have sought to give the character of the voiceless R in fig. 10, Pl. I.

The different characteristics of the voiceless explosives P, T, and K are easily recognized. At P the flame suddenly rises straight up high above the average line, then shows two or three similar *élancements*, which are followed by a few rapidly decreasing ones. Both the high and low chief movements show as at R the indentations caused by the air-current.

The rise of the flame is less sudden at T, neither is it so high; and the deep incisions are wanting, which at P in the commencement show two or three rapid *élancements*. At K, which is articulated further back in the mouth, there is still less a sudden rise of the flame; but the picture begins with a regular rising and falling wave, followed by a few rapidly diminishing ones of the same form. The indentation of the whole picture is the same as in P and T.

If we utter one of these consonants many times in succession while continually turning the mirror, we rarely see the picture well; it is therefore better to place the mirror so that the flame shows in one corner, and with a slight turn must pass over its whole surface. If we utter the consonant only at the moment of making this movement with the hand, we generally succeed in observing the most interesting part, namely the commencement of the picture.

To pursue these experiments further, perhaps it would be advisable to employ a mirror placed in an oblique axis on which it would be turned, and would then show the flame-picture in a continuous circle instead of broken bands.

The voiceless sibilants F, S, and CH give the same unsatisfactory result as the whispered vowels. I could see nothing defined in the confused dim light-bands.

[To be continued.]

II. *On an Acoustic Pyrometer. By* ALFRED M. MAYER, *Ph.D., Member of the National Academy of Sciences, Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U. S. A.**

[With a Plate.]

HAVING recently devised an arrangement of apparatus (Pl. III. fig. 7)—which is an instrumental simplification of the method first practised by Zoch (Pogg. *Ann*. vol. cxxviii.)—for measuring the number of acoustic wave-lengths contained in a given tube†, the idea occurred to me that I could use the method for the determination of the variation in the number of wave-lengths contained in this tube caused by a change in the temperature of the air which it contains, and thus succeed in readily determining any temperature to which the tube might be exposed.

The accuracy of this (as far as I know) entirely new method of pyrometry, and the facility of its application, can be judged of by the following discussion.

The formula $V=\sqrt{\frac{gh\Delta}{d}(1+at)\frac{c'}{c}}$ gives the velocity of sound in air of a known temperature. This formula, as is well known, is reduced numerically to $V=333\sqrt{1+\cdot 00367t}$; in which V = the velocity of sound at the temperature t Centigrade, 333 = the velocity of sound, in metres, at 0° C., and ·00367 is the coefficient of expansion of air under a constant pressure. We will suppose that we have outside of the furnace whose temperature we would determine, an UT_4 organ-pipe, and that we have placed opposite its mouth an UT_4 resonator, and that tubes from the nodal capsule of the pipe and from the resonator lead to contiguous gas-jets placed before the revolving mirror. We will also assume that the air in and around the organ-pipe is at 0° C., and that the serrations of the flames of pipe and resonator are, by means of the manometric flame-micrometer, brought to coincidence when 13 metres of metal tube, connecting the resonator and its manometric capsule, are placed in a furnace which also has the temperature of 0° C. Therefore the length of a wave in the furnace-tube is $\frac{333}{512}=0{\cdot}65$ metre, and it will contain twenty wave-lengths. Now gradually raise the temperature of the furnace to 820° C. As the temperature rises, we shall see the serrations of the resonator-flame gradually slide over those of the organ-pipe flame; and when the temperature has reached 820° C., we shall have observed that the serrations of the resonator-flame have glided over ten times the distance separating the centres of two contiguous serrations of the flame of the organ-pipe; for at 820° C. the air in the furnace-tube will have expanded to four times its volume at 0° C., and therefore

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* Communicated by the Author.

† See my previous paper in the Philosophical Magazine (November 1872), "On a Method of detecting the Phases of Vibration in the Air surrounding a Sounding Body, and thereby measuring directly in the vibrating air the length of its Waves and exploring the form of its Wave-surface."

$$\left(\lambda=\frac{333\sqrt{1+{\cdot}00367\times 820}}{512}\right);$$

it will contain half the number of wave-lengths it did when at 0° C.; and the length of one of these waves in the tube will be 1·3 metre.

We will now determine the limit of accuracy of the method by elevating the temperature of the furnace 100°, or to 920° C. At this temperature the velocity of the pulses in the furnace-tube will equal 696·63 metres; and the length of the wave at this velocity will be 1·36 metre. But 1·36−1·3=0·06 metre, the difference in wave-length produced by the increase in temperature from 820° to 920°, and sufficient to cause the serrations to be displaced 0·46 of the distance separating the centres of two contiguous serrations of the organ-pipe flame. But by means of the manometric-flame micrometer* one tenth of this displacement can be measured; therefore we can measure an increase of 10° in temperature above 820°.

From an examination of the well-established formula for the determination of the velocity of sound, it will be seen that the accuracy of our determinations of furnace-temperature will depend only on the precision of the coefficient ·00367, which is the number arrived at by Magnus and Regnault for the expansion of air under a constant pressure; and this is one of the most reliable constants we have in physics. Hence the accuracy of our measures to 10° C. will be equal to those of the air-thermometer, whose indications at present are necessarily received as our standards of thermometric determinations.

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* See my previous paper in the Philosophical Magazine, "On a Method of detecting the Phases of Vibration" &c. In this paper I give the credit of the suggestion on which I founded my micrometer to M. Radan, but I find that it is due to Zoch (Pogg. *Ann.* vol. cxxviii.). Radan mentions it in his *l'Acoustique* without giving credit to the inventor.

We will now examine the relation existing between temperatures and wave-lengths. I here give two Tables: the first contains the velocities of sound and the wave-lengths of the note UT$_4$ corresponding to temperatures between 0° C. and 2000° C.; the second, those corresponding to temperatures between 0° C. and −272°·48 C.

Temperatures.	Velocities.	Wave-lengths.
°	metres.	metre.
0 C.	333	0·650
100	389·34	0·760
200	438·53	0·856
300	482·72	0·942
400	523·14	1·021
500	560·74	1·095
600	596·03	1·164
700	629·04	1·228
800	660·67	1·290
900	690·77	1·349
1000	719·64	1·405
1100	747·38	1·458
1200	774·19	1·512
1300	799·96	1·562
1400	824·94	1·611
1500	849·35	1·658
1600	872·96	1·705
1700	895·97	1·748
1800	918·41	1·793
1900	940·26	1·836
2000	961·70	1·878
0	333	0·650
− 50	300·86	0·587
−100	265·00	0·517
−150	223·14	0·435
−200	171·79	0·335
−250	95·60	0·186
−272·48	00·00	0·000

These related numbers I have projected into the accompanying curve (Pl. III. fig. 8), whose abscissæ are the temperatures, and whose ordinates are the wave-lengths. This curve, which is the graphical expression of $y=\frac{333\sqrt{1+\cdot00367x}}{512}$, is evidently a parabola, since it has the form $y^2=ax$; and y will equal 0 when x has receded to the point on the axis of abscissæ equal to $-272°{\cdot}48$ C., which is "the absolute zero" of temperature.

It is evident that this curve will give the numerical relations between temperatures and the wave-lengths of any note or the velocities of sound in any gas by merely giving different numerical values to the divisions on the axis of ordinates.

It only remains to give the simplest formula for determining the temperature of the furnace in terms of the observed displacement of the resonator-serrations, and of the known number of wave-lengths in the furnace-tube at the temperature t.

Let t = temp. C. of the air in and around the organ-pipe,
t' = „ „ the furnace-tube,
v = velocity of sound at temperature t,
v' = „ „ „ t',
l = number of wave-lengths in furnace-tube at temp. t,
d = observed displacement of resonator-serrations by an elevation of temperature $t'-t$;

then $l-d$ will equal the number of wave-lengths in the furnace-tube (allowance made for elongation of tube by heat) at temperature t'. As the velocity of sound in the furnace-tube will be inversely as the number of waves it contains, it follows that

$$v' : v :: l : l-d;$$

hence

$$v'=\frac{vl}{l-d};$$

but

$$v \quad =333\sqrt{1+{\cdot}00367t}, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

and

$$v' \quad =333\sqrt{1+{\cdot}00367t'}; \quad . \quad . \quad . \quad . \quad (2)$$

hence

$$\frac{vl}{l-d}=333\sqrt{1+{\cdot}00367t'}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Reducing equation (3), we obtain

$$t'=\left(\frac{vl}{20{\cdot}16(l-d)}\right)^2-272{\cdot}48, \quad . \quad . \quad . \quad . \quad (4)$$

which gives t' in terms of v, l, and d. Combining equations (1) and (3), we obtain

$$t'=\frac{272{\cdot}48(2l-d)d+tl^2}{(l-d)^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which gives t' in terms of l, d, and t. But as v has to be calculated in order to obtain l in equation (5), it follows that equation (4) is the simpler and the more readily worked numerically.

If we call T *the absolute temperature* Centigrade, then

$$T=t'+272\cdot48,$$

and equation (4) becomes

$$T=\left(\frac{vl}{20\cdot16(l-d)}\right)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

in which equation the origin of coordinates is at the vertex of the parabola.

This paper is intended only to give a general account of the new method of pyrometry; and therefore I have not touched on the details of apparatus and experiment. These I will present in a subsequent communication, in which accounts of actual applications of the method will be given. I may, however, here remark that if the tube in the furnace is 13 metres long and 0·015 metre in diameter, it can be coiled into a spiral 0·5 metre in diameter, or into two spirals each of 0·25 metre diameter; or the tube can be bent on itself several times and thus form a compact fascicle of tubing. Also the tubing should *ascend* from the resonator to the furnace-tube and *descend* from the latter to the manometric capsule, so that the rarefied air in the hot tube cannot enter the tubing outside of the furnace. If the serrations of the manometric flames are too dim to be readily observed, they can be rendered distinctly visible even in broad daylight by passing the gas through a glass tube containing tow saturated with benzole. I have thus been able to exhibit these phenomena at night to an audience in a room brightly illuminated with many gas-jets. We can also make the vibrating flames brilliant by sifting into them the fine scrapings of lead-pencil.

In ascertaining the number of displacements produced by any elevation of temperature, the furnace-tube is slowly moved into the furnace, so that the displacements of the resonator-serrations can be counted as the tube gradually attains the highest temperature, when the serrations become stationary.

October 12, 1872.

The reader is requested to correct the following errata occurring in my paper "On a Method of detecting the Phases of Vibration" &c., Phil. Mag. Nov. 1872:—

Page 323, line 10 from bottom, *for* ventral *read* nodal.
" " 4 " *for* in front of *read* behind.

III. *The Chemistry of Sulphuric Acid-manufacture.*
By H. A. SMITH[*].

IT is astonishing, considering the importance of the subject, how little the chemistry of vitriol-manufacture has been inquired into. If we look over the records of its progress for the last hundred years, as embodied in the specifications of patents, we will find that the fundamental atoms of the structure remain the same, and that it is merely some particular point in the building itself which has been improved or embellished. I propose in this and succeeding papers to make a minute inquiry into the chemistry of this manufacture, and to elucidate, as far as I possibly can, the various laws which determine the combinations and decompositions occurring in the actions, and the causes which prevent these actions taking place.

I shall not in these papers attempt to give, even on a general scale, the various methods which have been used for the production of sulphurous acid, but shall take as the starting-point the use of sulphur for this purpose, the earliest substance from which this gas was obtained, and one which still exists as the purest source of all our vitriol-manufacture.

The subjects treated of in this paper are:—

Sect. 1. *An experimental examination into the causes which determine the action,* inter se, *of the gases in the lead chamber.*

Sect. 2. *The distribution of the gases in the lead chamber*; and following from this,

An inquiry into the best form of chamber to be used in the manufacture of sulphuric acid.

I hope to treat in another paper of the distribution of heat in the lead chamber, and also of one or two subjects intimately connected with this manufacture.

SECTION I.—*An experimental examination of the circumstances which determine the action,* inter se, *of the gases in the lead chamber.*

This inquiry was entered into in the hopes of being able to throw some light on the interior economy of the lead chamber, as at present used in the manufacture of sulphuric acid. Although the method ordinarily employed to show the theory of the formation of this acid is a very good one, yet there are numerous points which cannot be shown, and which can only be pointed out through the agency of chemical analysis.

No one has yet attempted to do this minutely; and our knowledge of the phenomena of alkali-manufacture is in many respects very much behindhand. Experiments have certainly been made, and great successes have been achieved on all sides; but these have tended more to broad generalizations than to exact chemical facts—to the manufacturing, not to the scientific side of the question. The interior of the lead chamber is comparatively an unknown land to us. Lowthian Bell has lately traced the actions occurring in his blast furnace through every stage, from the bottom to the top, in a series of most laborious experiments; but no one has yet done this with the sulphuric acid-chamber. I now venture to hope that this very limited attempt to at least *commence* such an investigation may be of interest, not only to those engaged in the manufacture, but to those who only look at it from a scientific point of view.

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[*] Communicated by the Author.

In observing the theory of the manufacture of sulphuric acid, there were many points which it struck me would well repay a closer examination. It is well known that when sulphurous acid comes into contact with one of the high oxides of nitrogen, it deprives it of its oxygen, provided the contact takes place in the presence of steam. But there are many causes which prevent this action; and it is not an uncommon thing to see this experiment fail.

If the heat of the vessel in which the combination is to take place be too high, or if it be not high enough, the result will be failure. And so in practice: every manufacturer knows how careful he must be in regulating the amount of steam he throws into his chamber; otherwise he finds a great amount of sulphurous acid escaping into the atmosphere, a larger amount of nitrous fumes in the acid from his Gay-Lussac Tower, and a smaller yield of vitriol.

It is into the laws which regulate the combination of those gases that I wish to inquire; and I have tried to do so, first, by individual experiment, and then by a careful examination of the lead chamber in which the action takes place.

On the Action of Sulphurous Acid Gas upon Nitric Acid Gas.

Although we are indebted to the labours of Clément, Désormes, Davy, De la Provostaye, and others for the light which has been thrown upon the theory of this action, so far as it relates to sulphuric acid-manufacture, yet I trust I may be excused for bringing forward results which differ in some degree from those of the above-mentioned workers.

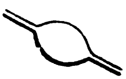
It is generally understood at the present day that no action can take place between *dry* sulphurous acid and nitric acid gases when brought together in the same vessel; and in all chemical treatises this fact is distinctly stated. Thus Miller, in his 'Elements of Chemistry,' speaking of the theory of the manufacture of sulphuric acid, says:—"*Direct combination, however, cannot be produced between the two gases* (oxygen and sulphurous acid); *the intervention of a third substance becomes necessary, and if water be presented to them a very gradual process of oxidation occurs.*"

Gmelin also, in his 'Handbook of Chemistry,' observes:—"A *dry* mixture of two measures of sulphurous acid gas and one measure of oxygen *remains unaltered*; but if water be present, a very gradual condensation takes place, and sulphuric acid is produced." The results arrived at by different observers may be summed up in the deduction from the above paragraphs—namely, *That no action can take place between these two gases without the intervention of water, either in the liquid or gaseous state.*

It was with the intention of inquiring more closely into this that the following experiments were made; and the conclusion I come to, and which I hope to clearly demonstrate in this paper, is the very reverse of that generally admitted—*That action does take place between the dry gases under certain conditions.*

As I was anxious to have the gases in as similar a condition as possible to those in the lead chamber, the sulphurous acid was made from burning sulphur, the nitric acid being prepared from nitrate of soda by the action of sulphuric acid, whilst the air employed was first carefully dried by passing through sulphuric acid and caustic potash, every care being taken to prevent the presence of even the smallest amount of moisture.

Exp. I. When dry sulphurous acid and nitric acid in the gaseous form are brought into contact in a perfectly dry glass vessel, which is then hermetically sealed, there is *apparently* no action (for this experiment I used a vessel of the following shape); but if this mixture, after being allowed to stand for ten or twelve days, be then opened, and the remaining gases expelled, it is found that a decided, though small amount of sulphuric acid has been formed over the sides of the glass vessel, and may be seen in the condition of white crystals, soluble in water, and behaving in all respects as sulphuric acid. There are many things which will prevent this formation, and which I intend noticing further on, such as temperature &c. This result, however, is greatly hastened by the addition of *a single drop* of water on the end of a fine platinum wire.



The conclusion one was apt to arrive at was that these crystals were merely the ordinary chamber crystals; but I was led to doubt this from various circumstances. In the first place, they seemed to differ from the chamber crystals in *form*, these partaking more of the needle-shaped form of the crystals of sulphuric acid; and also, when exposed to the air, they remained a long time (several days in fact) without change. This shows they are not the same as the chamber crystals. Again, when they were brought into contact with water, they dissolved without the evolution of nitrous fumes, from which we may fairly conclude that they were crystals of sulphuric anhydride.

I had long been inclined to take a different view of the cause of this action from that generally accepted. It seemed to me, in all its phases, to resemble the action of a small piece of leaven in a loaf of unleavened bread, or the action of a minute crystal dropped into a supersaturated solution of a salt, which immediately causes the solidification of the whole liquid. The action only requires to be commenced, and it then continues till the whole of the attainable oxygen has been made use of. This action, if quickened at all, is only slightly so, by the further addition of steam.

Exp. II. If, again, instead of inserting a drop of water into the vessel in which the gases are confined, it be surrounded with a coating of ice, the same effect takes place, a much longer time, however, being required.

Exp. III. The same end can also be attained by the *sudden* application of heat.

These results then led me to judge of the action as I have said, showing that the sulphurous acid is able to deprive the nitric acid of some of its oxygen without the intervention of steam, a medium which has hitherto been considered necessary.

It has been shown that a very small amount of water can cause the action to commence. The next experiment tried was to find what effect water present in a large quantity had upon the formation of the acid.

Exp. IV. A mixture of two volumes of steam to one of the mixed gases was put into a glass vessel and allowed to stand twenty-four hours (the gases being mixed in requisite proportion). The same amount

(*Exp.* V.) of the mixed gases was passed into a similar vessel, but into which *no air* was allowed to enter, to see if any action could take place in its absence; and the water was presented to the gases in the liquid condition on a platinum wire, and allowed to stand the same length of time. At the end of the time the results were:—

	Acid produced, calculated to percentage.
Exp. IV. . . .	66 per cent.
Exp. V.. . . .	93 „

a certain amount of nitric acid still remaining untouched. In both cases, however, the result, *in appearance*, was a complete combination. Still, on examination, a large amount of nitric acid was found in solution in water in No. 1 (Exp. IV).

Exp. VI. Equal volumes of steam and mixed gases were then tried, and the result on examination gave—

	Acid produced, calculated to percentage.
Exp. VI. . . .	74 per cent.

From these experiments, then, I considered myself justified in adopting the theory of the leavening nature of the action taking place.

It is also evident that the volume of steam introduced should be less than the combined volumes of the two gases. But let us take another case. Suppose the temperature of the vessel in which the experiment is to take place be raised to 100° C., or kept in boiling water, the results are found to differ in every case.

Exp. VII., VIII., IX. Taking the same volumes as above, I find the different yields of acid to be thus:—

Exp.	Acid produced, calculated to percentage.		Exp. above.
VII.. . . .	86·7	corresponding to	IV.
VIII. . . .	24·5	„ „	V.
IX.	80·2	„ „	VI.

Here, then, is quite another phase opened up to us: temperature has a great deal to do with the action taking place; and, as the result of many experiments, I find that its influence may be embodied in the general rule, that "*The higher the temperature the more steam required.*"

The foregoing results thus show the action occurring when nitric acid gas is brought into contact with sulphurous acid gas, both with and without the presence of steam. The next question to be solved is, In what part of the vessel does most action take place?

Most persons who have attended chemical lectures are familiar with the method employed to illustrate the formation of sulphuric acid. The mixed gases of sulphurous and nitric acids are brought together in a large globe, and steam is then introduced. Ruddy fumes are first formed; then a crystalline deposit takes place; and soon the atmosphere in the globe becomes white in colour. But on close examination another peculiarity is seen in the vessel; and that is, that near to the deposit, and long after the ruddiness has apparently disappeared, a very small narrow band of red fumes is noticed wherever the crystallization has taken place. This, then, led to the question being asked, Does not a greater formation of acid take place when some acid previously formed is present? and this question was attempted to be answered by the following experiments.

Exp. X. A layer of sulphuric acid from which all moisture had been expelled by long boiling, and which had been previously carefully weighed, was laid in the bottom of the vessel, and the gases allowed to enter, steam being excluded. The gas at the upper part of the vessel became almost immediately nearly white; but a strong and long-continued action seemed to be taking place at the bottom, near the surface of the sulphuric acid, no apparent action being noticed towards the top. On examination no sulphurous acid was found, whilst the weight of acid originally present had greatly increased. (In this experiment, as in the former, the vessel was allowed to stand twenty-four hours.) This seemed to answer the question, especially as on many repetitions the same results were obtained.

I felt these perfectly satisfactory as laboratory experiments. Some had now to be attempted on a large scale; and through the kindness of an acid-manufacturer I was enabled to make the desired trials; but as in every case I sustained signal defeat, being neither able to cool the chamber sufficiently nor raise it to the required temperature, I found I must, after many disappointments, rest satisfied (as many have found themselves obliged to do) with a knowledge of the fact that what appears perfect in the laboratory will not bear the crucial test of manufacture!

I still felt satisfied that I had proved the drawback to this to be want of control over the unwieldiness of the chamber. But one fact came out very strongly—that if I wished a good yield of acid, *the increase of steam must be in proportion to the increase of temperature.*

The point which next claimed my attention on the manufacturing scale was the question, In what part of the chamber does the greatest formation of acid take place?

This investigation resolves itself into two separate ones:—

1. The distribution of gases in the lead chamber.
2. The distribution of heat in the lead chamber.

The answer to the first of these embodies almost necessarily the much vexed question regarding the best form of chamber for use in the manufacture of sulphuric acid.

SECTION II. *On the Distribution of Gases in the Lead Chamber, and also an inquiry into the best form of chamber to be used in the manufacture of Sulphuric Acid.*

In the preceding page the following observation is made when speaking of the laboratory method of showing the theory of sulphuric acid-manufacture:—"Near to the deposit, and long after the ruddiness has apparently disappeared, a very small narrow band of red fumes is noticed wherever the crystallization has taken place." This, then, when taken into consideration along with exp. X. in the same section, tended to lead me to the belief that the greatest amount of condensation takes place at the bottom of the chamber, near the surface of any sulphuric acid which has been already formed, and that the upper portion of the chamber is of use principally as a reservoir for the gases; so that if, instead of having a long *high* chamber, one that was long but of low height were to be used, the same purpose would be answered to a greater degree, and the expense of chamber-building greatly reduced. It was with the intention of proving or disproving the truth of this theory that the following investigation was undertaken. The size of chamber used was about 140 feet in length by 30 high and 25 wide. The gases were introduced at the end of the chamber through an iron pipe 12 feet long by 3½ in diameter—the chamber-draft very moderate—steam injected at three points in the side and along with the gases at the end. In order to have a definite plan of proceeding, I took specimens of chamber air at every 10 feet along its length, 15 feet from the bottom—and also specimens at the same distances at 3 feet from the bottom,—thus having

14 analyses, chamber air at 15 feet

and

14 „ „ 3 „

the first being in reality a bisection of the chamber along its length.

The gases of which the percentages were obtained were sulphurous, sulphuric, and nitric acids.

Before giving my own results, I should be glad if I could quote those of any other observer; but I am unaware of any work having been done on this subject; so that the following observations may be interesting from their newness.

Sulphurous Acid.

Taking, then, in the first place, sulphurous acid, I will show first its distribution in the chamber, then take the two others in the order given above.

As is naturally expected, the largest amount of sulphurous acid is present at the entrance to the chamber; but dispersion takes place very rapidly indeed, so that each 10 feet makes a decided change in the percentage of acid present.

The amount of sulphurous acid present at a distance of 10 feet from end of chamber is equal to 72 per cent., continuing the same till about 20 feet from end. There is then a rapid fall to 46 per cent. at 30 feet, falling rapidly still, till at 40 feet from end the amount only equals 31 to 33 per cent. After this the variations are not so remarkable; the amount of sulphurous acid, however, becomes gradually less, and less till its lowest point is reached at 120 feet from entrance, when 13 per cent. is the amount present. These analyses were made at 15 feet from bottom of chamber. In those made at 3 feet from bottom the variations are not so sudden. At 10 feet from entrance the amount present is 3 per cent.; this rises rapidly, till at 40 feet from end it attains its maximum and 29 per cent. of acid. It is now at its highest point; and from this it begins to descend very gradually, till at 130 feet it falls to 8 per cent., and near its exit rises to 16 per cent.

The following diagrams will show this variation more plainly. The numbers along the top indicate the length of the chamber divided into distances of 10 feet each; those down the side of Diagram I*a* the percentage of acid present.

Diagram I*b*.

Length of chamber in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
15 feet in height	p. c. 72	p. c. 70 to 72	p. c. 46	p. c. 31 to 33	p. c. 25	p. c. 26	p. c. 30	p. c. 22	p. c. 29 to 30	p. c. 22	p. c. 23	p. c. 13	p. c. 18	p. c. 18	15 feet in height.
3 feet in height	p. c. 3	p. c. 8	p. c. 16	p. c. 29	p. c. 28	p. c. 18	p. c. 19	p. c. 20	p. c. 17	p. c. 17	p. c. 14	p. c. 13	p. c. 8	p. c. 16	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

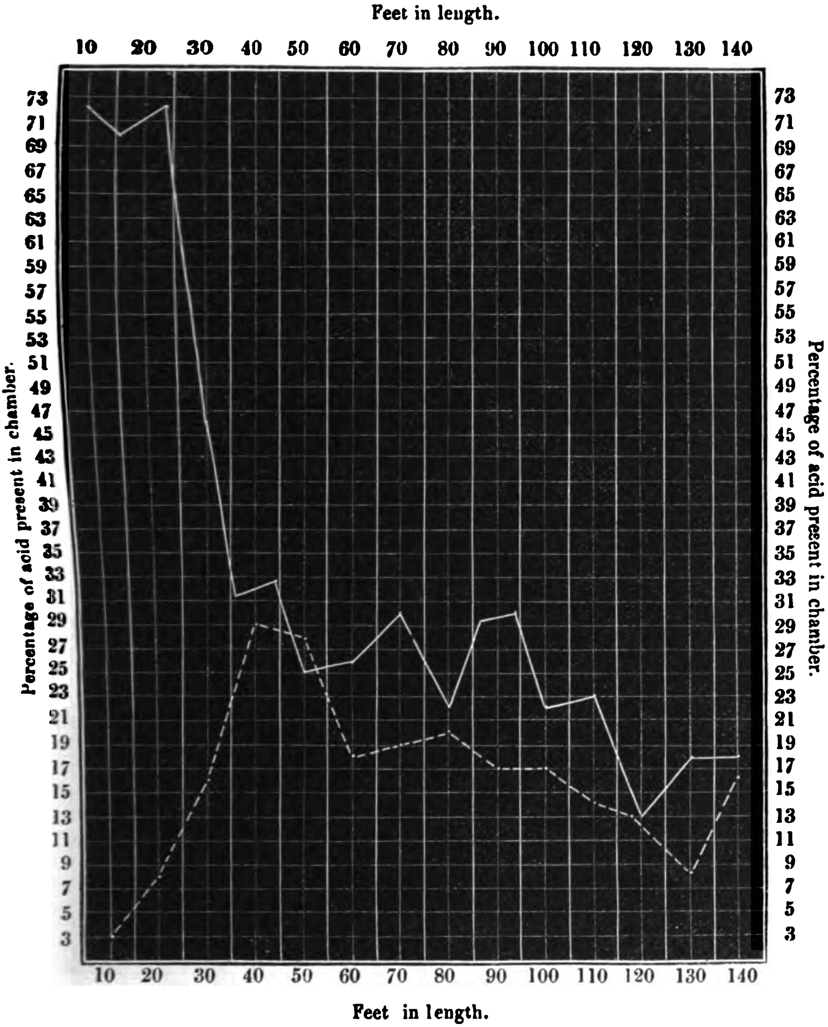
Length of chamber in feet.

The figures in the divisions of the above diagram represent the percentages of sulphurous acid corresponding to diagram I*a*.

The numbers "3 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

The numbers "72 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

Diagram I*a*.



In this diagram the dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

Or if we take the following Proportional Tables, we shall see the variations (numerical) more plainly.

No. I. At 15 feet height.

Feet from end of chamber.	
120	=1
130	=1·4
140	
80	=1·7
100	
110	=1·8
50	=1·9
60	=2
70	=2·3
90	
40	=2·5
30	=3·5
20	=5·5
10	

No. II. At 3 feet height.

Feet from end of chamber.	
10	=1
20	=2·6
130	
120	=4·3
110	=4·6
30	=5·3
140	
90	=5·6
100	
60	=6
70	=6·3
80	=6·6
50	=9·3
40	=9·6

In looking over Diagram I *a*, it may be noticed that there are three distinct falls in the percentage of acid, and after each there is again a slight rise. It may be interesting to remark that almost exactly at those parts were the points at which steam was thrown into the chamber. The falls are at 20 feet, 70 feet, and 110 feet respectively, steam being injected at 20 feet, 65 feet, and 110 feet.

The tremendous fall occurring from 20 feet to 40 feet may be accounted for by the great amount of steam entering the chamber at this point—as not only was it entering at 20 feet, but also, along with the gases, at a little below the large iron pipe at the end of the chamber; so that the steam absorbs a large amount of the hot sulphurous acid.

In the analyses at 3 feet these falls are not so noticeable, are indeed not so great, the acid here being out of the immediate action of the steam. In these experiments the temperature of the chamber was kept as low as possible, and the amount of steam allowed to go into the chamber was, as far as could be determined, almost one quarter the volume of the mixed gases. This, then, tends so far to show, what I previously imagined, that the upper portion of the chamber was of use principally as a reservoir for the sulphurous acid, allowing it to descend as it was required to the lower or working portion. This was also tried in another way.

The ordinary funnel-shaped collector usually placed in the chamber, and which communicates with a small leaden jar on the outside, by which a manufacturer gets an idea of the strength and make of his acid, was brought into use. Instead of being, as usual, placed about 8 feet from the bottom of the chamber, it was in this case placed about 16 feet high, and the amount of sulphuric acid formed was carefully observed.

In this case, after standing nine days, only $\frac{1}{18}$ of an inch of acid had formed, whilst at the height of 4 feet the make of acid was regular and fairly large.

In the former case (at 16 feet) sulphurous acid was continuously escaping, whilst the amount in the latter was merely trifling. This, then, was a very fair proof of the truth of my theory.

Sulphuric Acid.

Passing from the sulphurous to the sulphuric acid, I find the diagram of percentage in the latter a very strange one. At 10 feet from the point of entrance, where 0 is the percentage of acid, to 140 feet the variation at 15 feet in height is very trifling. The highest amount is reached at 50 feet, showing there only 23 per cent. But at 3 feet the analyses present a more extraordinary difference than that of Diagram I.

Beginning again at 10 feet from entrance, I find the amount of acid to be equal to 81 per cent.; then a sudden rise brings it to 89 per cent., this being the maximum; then comes a most rapid and continuous fall, till at 100 feet the amount is 30 per cent., and remains nearly at this to the end of the chamber.

The following diagrams will show the variations.

[Diagram II*a*, see p. 34.]

Diagram II*b*.

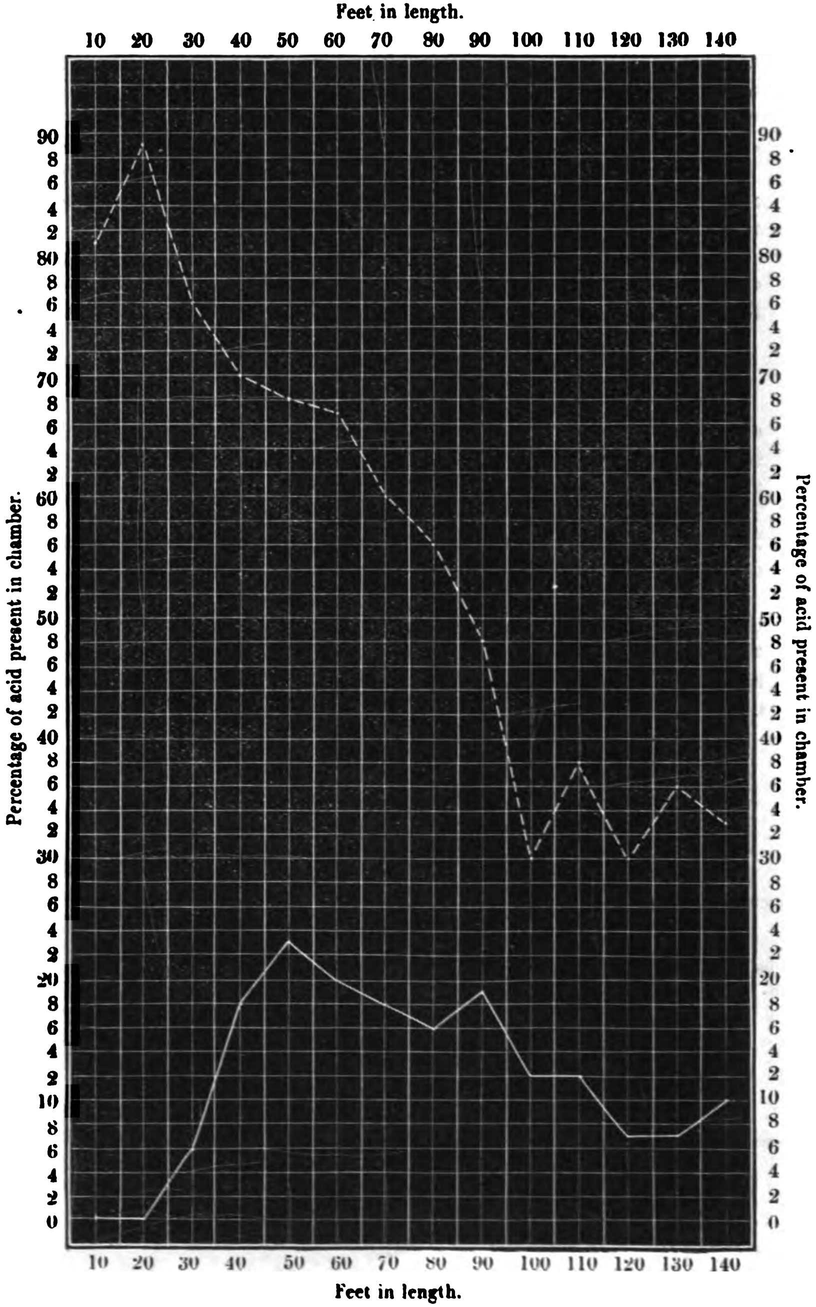
Length of chamber, in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
15 feet in height.	p. c. 0	p. c. 0	p. c. 6	p. c. 18	p. c. 23	p. c. 20	p. c. 18	p. c. 16	p. c. 19	p. c. 12	p. c. 12	p. c. 7	p. c. 7	p. c. 10	15 feet in height.
3 feet in height.	p. c. 81	p. c. 89	p. c. 76	p. c. 70	p. c. 68	p. c. 67	p. c. 60	p. c. 56	p. c. 48	p. c. 30	p. c. 38	p. c. 30	p. c. 36	p. c. 33	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

Length of chamber, in feet.

The figures in the divisions of the above diagram represent

Diagram II*a*.


Feet in length.
10 20 30 40 50 60 70 80 90 100 110 120 130 140
Percentage of acid present in chamber.
90 8 6 4 2 80 8 6 4 2 70 8 6 4 2 60 8 6 4 2 50 8 6 4 2 40 8 6 4 2 30 8 6 4 2 20 8 6 4 2 10 8 6 4 2 0
Percentage of acid present in chamber.
10 20 30 40 50 60 70 80 90 100 110 120 130 140
Feet in length.

the percentages of sulphuric acid, corresponding to the lines in Diagram II *a*.

The numbers "0 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

The numbers "81 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

In Diagram II*a*, as in Diagram I*a*, the top figures denote the length of the chamber divided into spaces of 10 feet each, whilst the side figures denote the percentage of acid present.

The dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

On comparing this with Diagram I *a*, the similarity is very striking. It is only required to place the white line in Diagram I. on the dotted line in Diagram II., and *vice versâ*. In No. I. the greatest amount of sulphurous acid is at the top of the chamber, and the smallest amount at the bottom. In No. II. the largest amount of sulphuric acid is at the bottom of the chamber, whilst the smallest amount is at the top.

Proportional Tables for Diagrams II.

No. I. At 15 feet height.

Feet from end of chamber.	
30	=1
120 130	=1·2
140	=1·7
100 110	=2
80	=2·7
40 70	=3
90	=3·2
60	=3·3
50	=3·7
10 20	=0

No. II. At 3 feet height.

Feet from end of chamber.	
100 120	=1
140	=1·1
110 130	=1·2
90	=1·6
80	=1·8
70	=2
60 50	=2·2
40	=2·3
30	=2·5
10	=2·7
20	=2·9

Nitric Acid.

The variations in the percentage of nitric acid are not very great, being only between 3 per cent. and 26 per cent., these being the maximum and minimum amounts. It attains its greatest height at 100 and 110 feet from end of chamber, and then sinks very rapidly down to 3 per cent. at 140 feet.

The figures and lines are similar to those in the preceding diagrams.

The dotted line represents the amount of acid at 3 feet from bottom of chamber.

The white line represents the amount of acid at 15 feet from bottom of chamber.

Diagram III *a*.

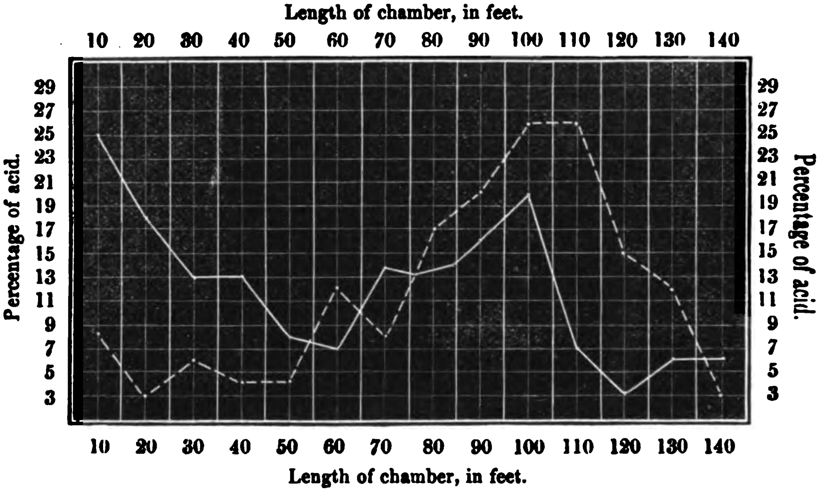


Diagram III *b*.

Length of chamber, in feet.

(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)
15 feet in height.	p. c. 25	p. c. 18	p. c. 13	p. c. 13	p. c. 8	p. c. 7	p. c. 14	p. c. 13 to 14	p. c. 16	p. c. 20	p. c. 7	p. c. 3	p. c. 6	p. c. 6	15 feet in height
3 feet in height.	p. c. 8	p. c. 3	p. c. 6	p. c. 4	p. c. 4	p. c. 12	p. c. 8	p. c. 17	p. c. 20	p. c. 26	p. c. 26	p. c. 15	p. c. 12	p. c. 3	3 feet in height.
(Entrance.)	10	20	30	40	50	60	70	80	90	100	110	120	130	140	(Exit.)

Length of chamber, in feet.

The figures in the divisions of the above diagram represent the percentages of nitric acid, corresponding to the lines in Diagram III*a*.

The numbers "8 per cent." &c. represent the percentage of acid at 3 feet from bottom of chamber.

The numbers "25 per cent." &c. represent the percentage of acid at 15 feet from bottom of chamber.

I also give the following Proportional Tables for Diagrams III.

No. I. At 15 feet height.

120	=1
130	=2
140	
60	=2·3
110	
50	=2·7
30	=4·3
40	
70	=4·7
80	
90	=5·3
20	=6
100	=6·7
10	=8·3

No. II. At 3 feet height.

20	=1
140	
40	=1·3
50	
30	=2
10	=2·7
70	
60	=4
130	
120	=5
80	=5·7
90	=6·7
100	=8·7
110	

We have now before us results which may assist us in coming to some conclusion regarding the most useful form of chamber. We have seen that the chamber must be divided into two parts —the working portion, and the reservoir (so to speak) for the gases. If, then, instead of employing this reservoir we lowered the height of the chamber and extended its length, we should have a greater condensing surface, as we have seen that the greater amount of acid condenses near the surface of already formed sulphuric acid. We should also have a form of chamber better adapted for getting a good draught. It would not be at all difficult for manufacturers to have a Table drawn up for the manager of their chambers, showing him the amounts of steam necessary to be thrown into the chamber according to the increase or diminution of temperature; they would thus have a great saving, both in the amount of gas escaping useless to the chimney, and also in the amount of acid manufactured. I hope soon to show the temperature at which the greatest amount of action takes place between the gases in the chamber, but would merely mention that it is of the greatest consequence to the manufacturer to take particular notice of the temperatures of his chamber, as upon the successful management of this depends in a very great degree the yield of acid, and also that trouble to all manufacturers, the flowering of the sulphur in the acid.

[To be continued.]

IV. *On the Definition of Intensity in the Theories of Light and Sound. By* ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*[*].

MR. BOSANQUET appears to think[†] that in my paper in the Philosophical Magazine for October last, I have fallen into an error in estimating the *vis viva.*

In point of fact I never attempted to estimate the *vis viva.* Adopting the definition of intensity propounded by three out of the five writers to whom I referred, viz. that the intensity is measured by the square of the amplitude, I pointed out that, admitting the square of the amplitude properly to represent the effect on the eye or ear of a single undulation, we must divide that quantity by the time—or, as Mr. Bosanquet would express it, we must multiply it by the number of undulations incident on the organ in a unit of time—in order to arrive at the true measure of intensity of the ray or note.

I must plead guilty, however, to having overlooked the contrariety exhibited by the definitions cited in my paper,—Sir John Herschel, Dr. Lloyd, and Mr. Airy taking the square of the amplitude as the measure of intensity, while Prof. Tyndall and Dr. Helmholtz (herein following Fresnel) adhere to the square of the maximum velocity as the measure; which latter, as Mr. Bosanquet points out, will have the square of the periodic time in the denominator, assuming the vibration to be correctly represented by the formula ordinarily employed for that purpose[‡]. Having always worked with the former definition, and never having heard that there was any dispute about the matter, I took for granted without inquiry the identity of the definitions, although the slightest examination would have shown them to be irreconcilable[§].

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[*] Communicated by the Author.

[†] See Phil. Mag. S. 4. vol. xliv. p. 386.

[‡] That in some important particulars the formula completely misrepresents the vibration is certain. I do not dwell upon this, however, as the want of correspondence between the two definitions is abundantly obvious.

[§] Sir John Herschel adopts both definitions, apparently without any consciousness of their incongruity; for, while in the passage I have quoted from his 'Treatise on Light' (No. 563) he speaks of the amplitude as determining the intensity both of light and sound, in his 'Treatise on Sound' (No. 126) he gives the following:—

"In the theory of sound, as in that of light, the intensity of the impression made on our organs is estimated by the shock, impetus, or *vis viva* of the impinging molecules, which is as the square of their velocity—and not by their inertia, which is as the velocity simply."

I may remark that the notion of the intensity of our sensations being measured by the "shock" of the "impinging molecules," which Sir John Herschel here adopts from Fresnel, is founded on a complete misappre-

But this alternative definition, just as much as the former, stands in need of correction. For, any claim which the maximum velocity can have to be regarded as the test of the intensity of a note or ray must rest on the assumption of its correctly representing the effect of a single undulation on the organ operated upon; and, admitting this to be the case, the true measure of intensity must be the square of the maximum velocity divided by the periodic time; so that instead of the measure of intensity being some constant multiple of the ratio of the squares of the amplitude and periodic time, *it must be a constant multiple of the ratio of the square of the amplitude to the cube of the periodic time.*

My reason for considering that the *square* of the amplitude cannot enter as a factor into the expression for the intensity is very simple.

If we have two series of waves superposed, each of which is represented by

$$y=a\sin\frac{2\pi}{\lambda}(vt-x),$$

the resultant vibration will be represented by

$$y=2a\sin\frac{2\pi}{\lambda}(vt-x);$$

from which it follows, if the square of the amplitude enters into the expression for the intensity, that the two systems of vibrations combined will produce *four* times the amount of illumination (supposing light to be referred to) which either would produce separately—a conclusion which appears absolutely fatal to this mode of estimating the intensity*.

That the data upon which Mr. Bosanquet founds his experimental determination of the measure of intensity are precarious, must, I conceive, strike every one. That the result he obtains is inadmissible, appears to follow from the following considerations.

Let a, a_1 be the amplitudes of two notes at opposite extremities of the musical scale; separated, say, by *seven* octaves. Then, if τ be the periodic time of the one, $2^7.\tau$ will be that of

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hension, inasmuch as the particles of air in contact with the tympanal membrane must necessarily have the same velocity as the latter.

If I remember rightly, Fresnel distinguishes between the intensity of the vibration itself and that of the sensation resulting from it—expressing the former by the simple power, the latter by the square of the maximum velocity.

* The only attempt to prove that the square of the amplitude and not its simple power should be taken, with which I am acquainted, assumes as a postulate that two candles will give *twice* the illumination of one! See Airy's 'Tract on the Undulatory Theory' *in loco.*

the other. Suppose, now, that the intensities of the two notes are equal; then, according to Mr. Bosanquet's measure of intensity, we shall have

$$\frac{a^2}{\tau^4}=\frac{a_1^2}{2^{28}\,.\,\tau^4},$$

$$\therefore\ a_1^2=2^{28}\,.\,a^2$$

and

$$a_1=2^{14}\,.\,a;$$

i. e. the amplitude of excursion of the lower note will be upwards of *sixteen thousand times* as great as that of the higher—a conclusion which appears incredible.

In reference to the principle on which the definition of intensity must be determined, I may observe that we have no direct consciousness of the amount of a velocity or force impressed alike upon all parts of our frame; it is only when the velocity or force produces relative displacement of different parts of the system that we become conscious of its existence, and are enabled to measure its effects. The amount of displacement of the nerves of the eye or ear is therefore necessarily one element in the expression for the intensity of the ray or note. The only other element remaining to be taken into account is the time within which such displacement is effected. Till the contrary is shown by indisputable experimental evidence, I must contend that the simple ratio of one of these elements to the other is the only measure of the intensity of a light or sound which can be regarded as admissible.

6 New Square, Lincoln's Inn,
December 6, 1872.

V. *On the Magnetizing-Function of Soft Iron, especially with weaker decomposing-powers. By* Dr. A. Stoletow, *of the University of Moscow*[*].

[With a Plate.]

In Kirchhoff's generalization[†] of Poisson's theory of the magnetization of soft iron, the knowledge of a certain empiric function is of the greatest importance. This we will name the *magnetizing-function* of iron, and denote it by k.

In order to render palpable to ourselves the physical signification of this quantity, we have to imagine an infinitely long and thin iron cylinder in a homogeneous magnetic field; the magnetic force is directed along the axis of the cylinder, and its magnitude is R. The iron is then magnetized uniformly throughout its whole length; that is, the magnetic moment m, referred to the unit of volume, is the same in every point of the cylinder. The ratio $\frac{m}{R}$ we designate as the value of the magnetizing-function for the argument R. If M, L, and T represent the units of mass, length, and time, R as a *magnetic force* is a quantity of the dimensions $M^{\frac{1}{2}} L^{-\frac{1}{2}} T^{-1}$; the quantity $k(R)$ is a *pure number*.

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[*] Translated from a separate copy, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlvi. pp. 439–463, having been laid before the Moscow Mathematical Society on Nov. 20 (December 2), 1871.

[†] Crelle's *Journal*, vol. xlviii. p. 370.

If, now, k is known for every value of R, we have all that is necessary to enable us to determine theoretically the magnetization of any mass we please of isotropic iron of which the form and dimensions are known and which is in a given magnetic field, so far as the coërcive force of the iron can be neglected. It is true that only a few special cases can be considered from this point of view; but this is, perhaps, not owing to the indeterminateness of the question, but solely to the analytical difficulties of the solution.

On the course of k when R becomes greater or less, as well as on the question how far it turns out different for different sorts of iron, the information at present existing is still rather unsatisfactory. Most observers have experimented with cylindrical rods—a case in which a strict theory can only be carried out on the assumption that the rod is of infinite length and thinness. On the other hand, the magnetizing-force made use of, and the magnetic moment it produces in the iron, have for the most part not been given in *absolute measure*, which makes the calculation of k impossible. So far as I know, such absolute measurements have only been made by Weber and Von Quintus Icilius.

The latter had to do with iron ellipsoids (in a homogeneous magnetic field); while Weber used cylindrical rods, which could only be considered by way of approximation, as very extended ellipsoids, and to that extent admit a theoretical treatment.

Neither of the two physicists mentioned has calculated the values of k from his experiments; they contented themselves with the consideration of the magnetic moment of the mass of iron. This, however, is not adapted to show clearly the universal dependence of the magnetization upon the magnetizing-force, since the magnetic moment of a cylinder of unlimited length, or an ellipsoid, is conditioned not only by this force, but also by the *form* of the iron.

Kirchhoff* first, from Weber's measurements†, calculated the values of the function k for certain values of its argument, in which for the cylindrical form of the iron an ellipsoidal one approximating to it as nearly as possible was substituted. The following numbers resulted:—

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* Crelle's *Journal*, vol. xlviii. p. 374.

† *Electrodyn. Maassbest.* iii. art. 26.

R.	k.	R.	k.
296	25·0	1512	8·4
301	23·5	1583	8·1
612	16·9	1773	7·4
823	13·5	1975	6·7
967	12·0	2080	6·4
1184	10·2	2397	5·7
1297	9·5	2484	5·6

As unit for R, $\frac{\text{mgr.}^{\frac{1}{2}}}{\text{mm.}^{\frac{1}{2}}\,.\,\text{sec.}}$ was taken, after Gauss.

From this we see that, with rising values of the argument, the function k diminishes, at first rapidly, then more slowly, and approaches asymptotically either zero or infinity—a fact already indicated previously by the observations of Joule and Müller.

The same method of calculation can with greater right be applied to the more recent experiments of Von Quintus Icilius*, because the form of the iron was really ellipsoidal. Some of these experiments were also performed with feebler magnetizing forces, as, instead of the direct magnetic action of the iron, the induction currents excited in a spiral wrapped round the ellipsoid, on the reversal of the magnetizing current, were measured.

For the calculation of k from these experiments, however, we must use only the most elongated ellipsoids, because with others the influence of k on the quantity of the magnetic moment is inconsiderable, and almost vanishes in comparison with the influence of the *form* of the ellipsoid. We will therefore calculate the experiments with both ellipsoids ($l=199$, $d=1·97$, and $l=350$, $d=2·12$; l is the polar axis, d the equatorial axis, both expressed in millimetres). If m is the magnetic moment of an extended ellipsoid of rotation which is magnetized by a constant force X acting parallel to the polar axis, we have

$$m=k\mathrm{R}=\frac{k\mathrm{X}}{1+k\mathrm{S}},$$

where S represents a number to be calculated from the ratio of the axes of the ellipsoid, viz.

$$\mathrm{S}=4\pi\sigma(\sigma^2-1)\left(\frac{1}{2}\log\operatorname{nat}\frac{\sigma+1}{\sigma-1}-\frac{1}{\sigma}\right),$$

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* Pogg. *Ann.* vol. cxxi. pp. 134 & 137.

if $\sigma=\frac{1}{\sqrt{l^2-d^2}}$*. The application of these formulæ to the two ellipsoids above mentioned gives the following Tables:—

TABLE I.

R.	*k*.	R.	*k*.	R.	*k*.
2·40	30·5	33·1	119·0	53·3	110·9
5·20	40·8	33·9	118·7	59·2	113·0
12·0	72·5	38·6	120·2	98·4	89·3
21·1	99·1	45·6	120·4	176·2	62·9
24·1	113·4	51·9	119·1	300·7	39·7

TABLE II.

R.	*k*.	R.	*k*.	R.	*k*.
5·18	20·1	116·5	76·8	1722	7·11
8·71	22·6	148	64·9	2034	6·06
10·30	23·1	213	47·1	2044	6·05
14·30	28·4	240	41·9	2449	5·37
22·2	45·3	250	40·7	2981	4·28
26·9	54·3	379	27·9	3013	4·23
34·4	83·4	455	23·8	3464	3·73
38·5	94·5	495	21·9	3864	3·36
47·0	98·1	610	18·1	3971	3·25
49·2	107·5	749	14·9	4229	3·05
64·9	107·3	935	12·3	4541	2·86
97·2	87·0	1339	8·88		

Hence becomes evident the remarkable fact which, it seems to me, has not yet been duly recognized, that with the lower values of R the magnetizing-function has an ascending course, and with a certain value of R reaches a maximum. We see further from these Tables that with a very long and thin rod, if it is magnetized by a force not too great, the magnetic moment increases not as is usually assumed, nearly proportionally, but much more rapidly, and between certain limits of the force is nearly proportional to the cube of it.

This is to be seen even from some of Joule's experiments†, whose attention, however, was chiefly directed to the *permanent* magnetism of the rod. Further, Wiedemann‡ remarked that, with rods of moderate thickness, the magnetic moment increases *a little more quickly* than the magnetizing-force. From the series of experiments No. 1 of Von Quintus Icilius the fact comes out very strikingly and at once; prominence was given to it by the observer himself: he seems, however, to be surprised that the augmentation of $\frac{m}{X}$ does not occur in the same degree with all ellipsoids, and thinks that "it may be too soon, in the present state of our knowledge, to expect to deduce a determinate law in relation to this"*. And yet this dissimilar behaviour of different ellipsoids is a direct consequence of the theory.

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* Neumann, Crelle's *Journal*, vol. xxxvii. p. 44.

† Phil. Trans. 1856, p. 287.

‡ *Galvanismus*, vol. ii. p. 297.

Indeed, let us first contemplate the two less-elongated ellipsoids numbered 2 and 3 in v. Quintus Icilius, and which were cut out of the same piece of iron as No. 1 ($l=199$, $d=1{\cdot}97$). For No. 2, l was $=200$, $d=20{\cdot}41$; for No. 3, $l=51$, $d=19{\cdot}84$. We may therefore, in the expression

$$\frac{m}{X}=\frac{1}{\frac{1}{k}+S},$$

neglect $\frac{1}{k}$ in comparison with S, or put $\frac{m}{X}=\frac{1}{S}$. We then get $\frac{m}{X}=3{\cdot}80$ for No. 2, and $0{\cdot}608$ for No. 3.

From the experiments of v. Quintus Icilius there result the means 4·34 for No. 2, and 0·596 for No. 3.

M. v. Quintus Icilius† has, further, investigated also a more elongated ellipsoid ($l=100{\cdot}5$, $d=5{\cdot}24$), with which the approximative calculation just used would be inadmissible. In order to test the theory in this case also, let us proceed as follows. First, for every X given we seek the corresponding R, using Table II. (as the more extended): that is, we first put $k=0$, therefore $R=X$; we find for this value of R the corresponding k from the Table, calculate again $R=\frac{X}{1+kS}$, &c. until two consecutive values of R come out nearly equal. Then we have found k, and can calculate $\frac{m}{X}=\frac{1}{\frac{1}{k}+S}$ and compare it with the result of experiment.

In this way I find, for example,

X.	$\frac{m}{X}$ calculated.	$\frac{m}{X}$ observed.
48·2	7·6	7·09
275	10·0	9·67
553	10·0	9·99
1701	6·5	7·51
2851	4·5	5·00
4436	2·9	3·52

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* Pogg. *Ann.* vol. cxxi. p. 135.

† *Ibid.* pp. 132 & 138.

We see that here also observation and calculation do not diverge too widely.

Before I leave the experiments with ellipsoids and pass to my own investigation, I must mention a still more recent work (1870), that of M. Riecke*. He has observed, according to Weber's method, the remagnetizing of various ellipsoids by the vertical component of terrestrial magnetism. The more lengthened the ellipsoid, the greater was, in general, the number resulting for *k*. In conformity with the foregoing, this was to be expected—since, X remaining equal, the quantity R increases simultaneously with $\frac{1}{S}$,—although M. Riecke is more inclined to seek another reason for it. The magnetizing-force was not measured directly; but if we assume, with Weber, that the vertical component of the earth's magnetism (at Göttingen) was $=4{\cdot}228$†, then it follows that, for Riecke's experiments, $R=0{\cdot}31-0{\cdot}72$. Corresponding to this, *k* increased from 13·5 to 25·4.

It seemed to me not without interest to ascertain the magnetizing-function by another method, recently proposed by Kirchhoff‡. Therein I experimented especially with feebler decomposing forces, in order once more to establish and place beyond doubt the ascending course of *k* with such forces. The experiments which I will communicate appear also to have a further interest. The only case of magnetizing theoretically solved completely, and which at the same time can be carried out in practice, was till quite recently that of an ellipsoid (inclusive of the sphere). In the present experiments, I believe, the theory is for the first time tested on a body of another form, namely a *ring*.

We imagine a ring of iron—that is, a solid of rotation which is not touched by the rotation-axis. Let this ring, in its whole periphery, be wrapped round with wire (the *primary wire*); and let another wire (the *secondary wire*) be wound round it once or more times. If a constant current is passed through the first wire, and if the second is closed upon itself, a momentary current is induced in the latter as soon as the direction of the primary current is suddenly reversed§. The integral value of the induced electromotive force, expressed in absolute electromagnetic measure, is, according to the theory given by Kirchhoff:—

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* *Die Magnetisirungszahl des Eisens für schwache magnetisirende Kräfte*: Göttingen, 1871. Abstract in Pogg. *Ann.* vol. cxli. p. 453.

† This number belongs properly to the *middle* of the year 1870. See Weber, "Bestimmung der erdmagnetischen Kraft in Göttingen," p. 30 (*Abhandlungen d. k. Gesellschaft d. Wiss. zu Göttingen*, vol. vi.).

‡ Pogg. *Ann.* Ergzbd. v. p. 1.

§ I always employed the *reversal of the current*, because thereby the results are less vitiated by residual magnetism than with the *closing and opening* of the circuit. The same method was made use of by Weber and v. Qu. Icilius. The employment of *both* methods would enable us to measure the residual magnetism of the iron.

$$E=4nn'i\{4\pi kM+P\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The first term of the expression proceeds from the currents induced by the remagnetizing of the iron; the second, from the direct voltaic induction of the two wires. Herein signify:—

n and n', the numbers of the windings of the primary and secondary wires respectively (if the latter is wound round the ring ν times in one direction, ν' times in the other, we have to understand by n' the difference $\nu-\nu'$);

i, the intensity of the primary current, in absolute electromagnetic measure;

M, the integral, extended to the cross section of the ring, of the form $\int\frac{dS}{\rho}$, in which dS is an element of the surface of that section, ρ the distance of this element from the rotation-axis of the ring;

P, a similar integral, referred to the surface of a primary winding.

k is the magnetizing-function of the iron; and the argument R, to which k is referred, is the mean value of the magnetizing-force. This is $=\frac{2ni}{\rho}$ for a point (ρ) of the ring. Consequently

$$R=\frac{2niM}{S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

S denoting the entire surface of the cross section of the iron.

If, then, we know the form and dimensions of the ring and the primary turns, as well as the number of these and the secondary rounds, the function k can be calculated for every given R, to be expressed in absolute measure, as soon as the ratio $\frac{E}{i}$ is likewise measured in absolute measure.

This is the fundamental idea of the method recommended by Kirchhoff and which I have followed. To what extent it required to be modified for different values of the magnetizing-force R, will be gathered from the following.

I had such a ring made of soft iron, in the workshop of Dr. Meyerstein, at Göttingen. It was kept for twelve hours at a red heat, and then cooled by gradually covering the fire. The cross section of the ring is a rectangle; the extreme diameter I found equal to 200·025 millims., the internal $=180·37$ millims.; the height $=14·75$ millims. From this the quantity denoted above by M is calculated $=1·526$ millim.

To this ring two rings of wood, circularly rounded off on the outer side, were cemented; upon these a covered copper wire (without its covering, 0·45 millim. thick; with it, 0·67) was wound as close and uniformly as possible. This wire, mostly used as primary closing, had 800 turns; it was connected with a galvanic series. The mean contour of a winding is pretty accurately represented by the combination of a rectangle 11·1 millims. wide, and 24·5 high, with two semicircles of 11·1 diameter (Pl. III. fig. 1). Accordingly the quantity denoted by P (upon the exact knowledge of which much less depends than upon the determination of M) is found by calculation to be 3·87 millims.

Equations (1) and (2), applied to the ring described, give us the following formulæ for the calculation of R and *k* :—

$$k=\frac{\frac{1}{320\,n'}\cdot\frac{E}{i}-3{\cdot}87}{19{\cdot}172};\quad R=16{\cdot}84\,i.$$

Upon the first layer of wire-turns, which mostly served alone for the primary wire, 750 rounds of the same piece of wire were wound, and likewise filled up the entire periphery of the ring; in this second layer, however, the wire was divided into five separate portions, of which the number of circumvolutions were respectively 50, 100, 150, 200, and 250. As required, one or another of these divisions, or different combinations of them were connected with a multiplier and used as a secondary wire. The number of the secondary circumvolutions might thus be increased, step by step, to 100, 150, ... 700, 750; and, with the same *effective* number of turns, I could vary the resistance of the secondary wire, by using, for example, at one time the 50 division alone, at another combining the 250 and 200 divisions in opposite directions. All these wires were wound in such wise that with each the *longitudinal* current, according to Ampère, was compensated by a returning round of wire. (Fig. 2 is a sketch of the inner wire.)

With greater decomposing forces I could make use of a smaller number of secondary turns. Then I had no need of the divisions above mentioned for the secondary closing, and merely caused the conducting wire of the multiplier to run round the ring in ten turns. Those divisions, however, I could now connect with each other, all in one direction, and with the first layer of wire; whereby the number of the primary circumvolutions became 1550, and with the same galvanic series the decomposing force was considerably greater. With this arrangement, taking into account the alteration of *n* and P (with the outer turns the length of the oval was 37·75 millims., the breadth

11·75), the following equations became valid:—

$$k=\frac{\frac{1}{6200n'}\cdot\frac{E}{i}-4{\cdot}11}{19{\cdot}172};\quad R=32{\cdot}626i.$$

Before passing to the proper measurements, I will give an account of two preliminary experiments, which I made for the purpose of ascertaining the regular course of the phenomenon and to test its accordance with theory.

If we leave the magnetizing current i unchanged, from equation (1) it follows:—1, that the induced electromotive force increases proportionally with the number of the secondary turns; 2, that it depends *only* on the number, and not on the quality, of the turns.

Lenz found similar laws by experiment when investigating the induced currents excited in a spiral wire enclosing an iron cylinder as soon as the cylinder was pulled away from the pole of a powerful steel magnet*. Theoretically, however, these laws can only be derived under the assumption that the cylinder is infinitely thin and long†.

I conducted the current of a Daniell's series through the primary wire of the ring. Divisions 100, 150, 200, and 250 of the secondary wire were connected with each other and with a galvanometer of great deadening force. The connexion was made in various ways, so that the numbers of effective turns were respectively 100, 200, 300, 400, 500, and 700. As the resistance of the secondary circuit remained unaltered, the deflections observed on the galvanometer when the primary current was reversed were proportional to the induced electromotive forces. The mean numbers of these deflections, each from eight observations, were:—

with 100 turns,	200,	300,	400,	500,	700,
= 47·2 scale-divisions;	94·4;	140·4;	189;	236·4;	329·9.

Starting from the number 329·9, the others are found by calculation to be

47·13, 94·26, 141·4, 188·5, 235·6.

When the divisions were so combined that the number of turns running in one direction was equal to the number in the opposite direction, I obtained on the reversal of the current the small deflections ±1·5, which are probably to be attributed to the imperfect homogeneousness of the iron mass or of the primary turns‡. When 150−100−50 turns were employed, the magnet remained perfectly at rest.

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* Wiedemann, *Galvanismus*, vol. ii. p. 634.

† Kirchhoff, Crelle's *Journal*, vol. xlviii. p. 368.

‡ According to theory, the action of the ring upon external magnets

Similarly it was proved that the form and other conditions of the secondary turns are without influence. When I caused a thicker wire to run, in 50 wide and irregular turns, round the ring, and formed a secondary closing out of this wire, the oppositely directed division of 50 turns, and the multiplier, I obtained a sensible deflection on reversing the primary current.

Generally the consecutive induction-impulses are very constant, if only the precaution is taken to turn the commutator several times after each change of the magnetizing-force before commencing the observations, as the first deflections are influenced by residual magnetism.

I pass now to the proper measurements of k and R.

As we have seen, it depends upon ascertaining the ratio $\frac{E}{i}$ of the induced electromotive force to the intensity of the inducing current. For this purpose, according to the strength of the latter, various methods were employed.

With stronger primary currents, the arrangement shown in fig. 3 (Pl. III.) was adopted. The current of a galvanic apparatus K (mostly 4–12 Daniell's elements, for more powerful currents 12–14 Bunsen) was conducted through two commutators (C_1 and C_2), the primary wire P of the ring, and a circular roll of wire (R) of known dimensions. This roll, intended for the measurement of the primary current, is placed perpendicular to the magnetic meridian, eastward of a magnetometer (M); the axis produced of the roll meets the middle point of the magnetic bar. The distance of the latter from the centre of the roll was mostly fixed at 1000 or 1250 millims.

Moreover the magnet M is now surrounded by a multiplier with close windings. Through this multiplier, provided with a damper, the induced currents are conducted. That is to say, the conducting wire of the multiplier is either wound in several turns round the ring, or connected with the above-mentioned divisions of the second layer of wire upon the ring. Lastly, W is a Siemens's resistance-scale, by which the primary current can be weakened. The observation was effected with scale and telescope.

The constant current in the roll R imparts to the magnetometer a certain deviation from the magnetic meridian. By turning the commutator C_1, the current in the primary wire P only is reversed. Thereby an induction-shock is produced in the mul-

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should have been altogether independent of whether a current passed through the turns and in which direction. This, however, was far from being verified when I placed the ring close beside the galvanometer—which again intimated the before-mentioned want of homogeneousness. At all events the direct action of the ring upon the galvanometer was quite imperceptible when it held its usual place.

tiplier, and imparts a deflection to the magnet. The position of equilibrium conditioned by the primary current is observed; as well as the induction-deflection. By a second turning of C_1 the magnet, after coming to rest, receives an impulse in the opposite direction. Finally, the same observations are made after the commutator C_2 has been turned and thereby the current in the entire primary circuit reversed. Let

a be half the difference between the two positions of equilibrium of the magnet with the two positions of the commutator C_2,

A the elongation of the magnet from the then position of equilibrium by the induction-shock,

T the time of an oscillation of the undamped magnetometer,

λ the logarithmic decrement of the oscillations when the secondary wire is closed,

m the ratio of the torsion-moment exerted by the multiplier upon the magnet to that proceeding from the roll, the same current passing through both;

then the ratio of the induction-shock J to the primary current i, both taken in the same measure, is

$$\frac{J}{i}=\frac{A}{ma}\cdot\frac{T}{\pi}\cdot e^{\frac{\lambda}{\pi\mu}\arctan\frac{\pi\mu}{\lambda}},$$

where μ is the modulus of the Briggian logarithms.

If, instead of observing the first deviations, we wish to employ the multiplication method, A and a are to be calculated, in the known manner, from the consecutive readings and the damping λ.

Therefore, m and T being known (T varies a little, and must be determined afresh from time to time), the ratio $\frac{J}{i}$ can be calculated from determinations each time of A, a, λ. If, further, the resistance W of the secondary closing is known in absolute measure, we have also the ratio $\frac{E}{i}=\frac{J.W}{i}$, which is required for the calculation of k.

The resistances of all the wires of which, in the various observations, the secondary circuit consisted—that is, of the multiplier M (w_m) and all the sections of the second layer of wire (w_{50}, w_{100}, ...)—were determined in absolute measure by comparison according to Wheatstone's method with a British Association unit. They were (reduced to 20°·4 C.):—

$w_m = 2{\cdot}4758\times10^{10}\frac{\text{mm.}}{\text{sec.}}$ $\quad$ $w_{150}=2{\cdot}1150\times10^{10}\frac{\text{mm.}}{\text{sec.}}$

$w_{50} = 0{\cdot}74502$,, ,, $\quad$ $w_{200}=2{\cdot}6829$,, ,,

$w_{100}=1{\cdot}4046$,, ,, $\quad$ $w_{250}=3{\cdot}4253$,, ,, In every observation, the temperature of the air close to the multiplier was noted; and a second thermometer was applied to the second layer of the ring, which with more powerful primary currents was considerably heated by contact with the primary wire. The resistances were reduced to the readings of the two thermometers; and herein the increase of a resistance w (of copper) with a heating of 1°C. could be supposed equal to $0{\cdot}00387\,w$*.

The number above denoted by m, which specifies in what ratio the action of the multiplier was more powerful than that of the roll (the currents in both being equal), was measured once for all. For this purpose the current from a galvanic apparatus was conducted through the roll; a known portion of the same current was sent through the multiplier by means of a derivation of small, accurately measured resistance. There was found $m = 2414$ for the case in which the roll was 1000 millims. distant from the suspending thread of the magnet. From the constants of the roll to be given below, the corresponding number m could be calculated for the case in which the distance was different.

We have now all that is necessary for the calculation of k from the observed A, a, λ, and T. In order, however, that we may be able to calculate also the argument R (the magnetizing-force) to which this k refers, the intensity of the current i must be determined in absolute measure. Let

H be the horizontal direction-force of the magnet (proceeding from the magnetic field of the place of observation, in a small part also from the torsion of the suspension-thread),

u the angle of deflection of the magnet, produced by a current of absolute value i which is passing through the roll,

F the superficial extent of the roll,

r the distance of the roll from the thread of the magnet;

then we have, as a first approximation:—

$$\frac{\mathrm{H}\tan u}{i} = \frac{2\mathrm{F}}{r^3}.$$

More exactly, the right-hand part of this equation is a series which proceeds by descending powers of r. Presupposing that both magnet and roll, in their distant magnetic action, are symmetrical in relation to their axes, only odd powers of r can occur in that series. Limiting ourselves to the first two terms, we obtain

$$\frac{\mathrm{H}\tan u}{i} = \frac{2\mathrm{F}}{r^3}\left(1 + \frac{\beta}{r^2}\right),$$

where β is a constant† which depends on the dimensions of the

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[*] Matthiessen and v. Bose; see Wiedemann, *Galvanismus*, vol. ii. p. 1060.

[†] The influence of the angle u on β may be neglected when u is small.

roll and the magnet, as well as on the distribution of the magnetism in the latter.

From the dimensions of the roll and the numbers of the convolutions of its four layers of wire, there was found

$$F=6075500 \text{ sq. millims.}$$

The second constant β could now be ascertained by experiment. To that end the angles of deflection u_1 and u_2 of the magnet were observed, which were occasioned by the roll when, traversed by a constant current, it was placed at two different distances r_1 and r_2. Then evidently

$$\beta=-\frac{\frac{\tan u_1}{\tan u_2}-\left(\frac{r_2}{r_1}\right)^3}{\frac{\tan u_1}{\tan u_2}-\left(\frac{r_2}{r_1}\right)^5}\cdot r_2^2.$$

In this determination, the most advantageous ratio $\frac{r_2}{r_1}$ is found by the rules of the calculation of probabilities to be $=1\cdot336$. Accordingly with $r_1=1000$ millims. r_2 was taken as $=1335$ millims. In this way I obtained $\beta=-26301$, and therefore

$$\frac{H \tan u}{i}=0\cdot011831 \quad \text{for } r=1000 \text{ millims.,}$$
$$=0\cdot0061169 \text{ for } r=1250 \quad ,,$$

The absolute intensity of the current can be calculated according to these formulæ, provided that the quantity H is known. This was measured thrice in the course of the investigation, by Gauss's method. In order to take account of the variations of H in the intervals, the deflection-bar used in the measurement of H was daily placed upon the board which carried the roll, at the distance of 1250 millims. from the latter, and the deflection of the suspended magnet was observed; from this the alteration of H since the last measurement could be ascertained. It is true that the magnetic moment of the bar was not altogether constant; the variation, however, was caused chiefly, perhaps, by the temperature, and could still be allowed for. I will now call attention to some sources of error in the method described. If we so form the secondary closing that the number of effective turns is $=0$, small deflections of the magnet are to be expected, even independently of the unhomogeneousness of the iron and of the primary convolutions, as soon as the position of the commutator C_1 is changed. These arise partly from the interruption of the primary current, but partly from the extra currents which are induced in the primary wire of the ring as well as in the roll. The first and third of these disturbances act, as may easily be seen, in opposition to the primary current; while the action of the extra currents induced in P changes its direction both with the initial placing of C_1 and with the placing of C_2. Hence arise small movements of the magnet, the direction of which changes with the placing of C_2, but their magnitude with the nature of the change of position of C_1. This was actually observed. It is not difficult to estimate the influence of this source of error both upon the calculation of A and on the values of a (when these are determined from multiplication-readings), and to apply the correction. Moreover the correction of A can be dispensed with, if the secondary wire be inserted in the multiplier now in one direction and then in the opposite.

As an example of the method here discussed, I cite, abridged, the record of one measurement:—

19*th October* 1871; Series, 12 Daniell's elements weakened by 10 Siemens's resistance. Roll at 1000 millims.

Number of primary turns $n'=250-200+150-100=100$.

Temperature at the ring, $t_r=16^\circ{\cdot}7$ C.; temperature at the multiplier, $t_m=9^\circ{\cdot}7$. Hence $W=11{\cdot}874\times10^{10}\frac{\text{millims.}}{\text{second}}$.

Distance of the scale from the mirror of the magnet $=2125{\cdot}3$ scale-divisions.

Logarithmic decrement $\lambda=0{\cdot}1410$; consequently

$$\log e^{\frac{\lambda}{\pi\mu}\operatorname{arc\,tan}\frac{\pi\mu}{\lambda}}=0{\cdot}06588.$$

Duration of an oscillation of the magnetometer undamped, $T=20{\cdot}419$ seconds.

Direction-force of the same, $H=1{\cdot}9820$ milligr.$^{\frac{1}{2}}$ millim.$^{-\frac{1}{2}}$ sec.$^{-1}$.

The eight positions of equilibrium of the magnet (with three combinations of C_1 and C_2 and both ways of connexion of the secondary wire with the multiplier) were, corrected for the final distance of the scale from the mirror:—

	621·5	404·8
	622·2	406·2
	623·3	405·9
	621·7	405·4
Means . .	622·17	405·57

The corresponding movements of the magnet through induction-shocks were:—

243·4	244·4
241·7	244·5
241·9	244·4
243·7	244·4

Consequently $a=108\cdot23$ sc.; $A=243\cdot46$ sc.
Hence we obtain

$$\frac{E}{i}=8\cdot3700\,.\,10^{6}\,\frac{\text{millims.}}{\text{second}},\quad i=4\cdot2656\,\frac{\text{mm.}^{\frac{1}{2}}\,\text{mgr.}^{\frac{1}{2}}}{\text{sec.}},$$

and, lastly,

$$k=136\cdot22;\qquad R=71\cdot830\,\frac{\text{mgr.}^{\frac{1}{2}}}{\text{mm.}^{\frac{1}{2}}\,\text{sec.}}.$$

With feebler currents this method required a modification, as the measurement of such currents by removing the roll would be too inexact. This could be remedied by inserting a bridge before the wire P, so that only the current in P would be weakened. Mostly, however, for such cases the arrangement was adopted which Kirchhoff employed in his measurement of the induction-constant. The series K (Pl. III. fig. 4), the primary wire P of the ring, the secondary wire S, and the multiplier M of the magnetometer formed a circuit, which was divided into two branches by means of a bridge B of small resistance. By the commutator C_1 the current in P was reversed, and by the commutator C_2 the disposition of the wire of the multiplier was changed.

The currents induced in S when C_1 is shifted pass almost exclusively in the circuit SBM; while the current from the galvanic apparatus passes for the most part in K B P, only a small portion of it going through M and being used for the measurement. As the reduction-constant of the multiplier, which serves for the determination of absolute current-intensities from observed readings, had been previously ascertained, the current i_m in the multiplier could be calculated in absolute measure. Finally, from the known resistances of M, S, and B the whole magnetizing current i in P was obtained.

Now, with respect to the quantity $\frac{E}{i}$ required for the calculation of k, it is easily seen that it becomes $=w_b\frac{J_m}{i_m}$, if w_b denotes the absolute resistance of the bridge B, and J_m the integral value of the induction-current in the multiplier. (It is here presupposed that w_b^2 may be neglected in comparison with the product of the two resistances K B P and S B M, which was admissible in all the experiments.)

As a bridge B strong copper wires were used, each with its ends soldered to two small forks of thicker wire, which were amalgamated beneath and dipped into mercury-cups (fig. 5). The resistance of the proper wire 1B2 (from the place of soldering, 1, to the other, 2) is to be regarded as the quantity denoted by w_b. This resistance was determined by Thomson's method*, with only a slight modification, which was required by the circumstance that I had not to measure off two resistances according to a given ratio, but to compare each two already definitively measured and prepared resistances. In this manner, for the four bridges used, the resistances w_b at 20°·4 C. were found to be respectively equal to

$$1{\cdot}1626 \times 10^8 \frac{\text{mm.}}{\text{sec.}}, \quad 2{\cdot}2589 \times 10^8 \frac{\text{mm.}}{\text{sec.}},$$
$$1{\cdot}1683 \times \;,, \;\; ,, \quad 2{\cdot}2811 \times \;,, \;\; ,, \;;$$

so that by using the two small wires in juxtaposition a resistance is obtained which amounts to less than 0·002 of the least value of S M B.

In all other respects the method of measuring was identical with the one first described: now the first deflections of the magnet were observed; then the multiplication method was employed. The disturbing influence of the extra currents (which now proceeded only from the wire P), as well as of the interruption of the current, tends always to diminish the deflection of the magnet, which is occasioned by the primary current and has to be calculated from multiplied readings. This influence it is easy to eliminate; the mean value, however, of the induction-deflections is not affected by it, if we always carry out the two shiftings of the commutator C_1.

I give also an example of this second method:—

September 3, 1871. One Daniell; $n=800$; secondary wire $=$division of 100 turns. Consequently $\frac{w_s+w_b}{w_b}=668{\cdot}30$ (w_s is the resistance of the branch B M S B, fig. 4).

In B the two smaller bridges were inserted in juxtaposition; temperature$=21°{\cdot}2$; consequently $w_b=5{\cdot}8687 \times 10^7 \frac{\text{mm.}}{\text{sec.}}$.

Further

$T=20{\cdot}640, \quad \lambda=0{\cdot}16571, \quad H=2{\cdot}0018, \quad D=2261$ sc.

The multiplications (10 readings each) gave as positions of equilibrium of the magnet,

409·80, 584·67, 584·78, 409·68,—

and as the values of the induction-deflection,

124·23, 122·83, 122·64, 123·90.

Correction for extra currents $=1{\cdot}84$.

From this we find

$a=89{\cdot}33$ sc., $A=123{\cdot}40$ sc., and, finally,

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* Phil. Mag. S. 4. vol. xxiv. p. 149; Wiedemann, *Galvanismus*, vol. ii. p. 1046.

$$k=103{\cdot}33, \quad \mathrm{R}=15{\cdot}57.$$

The results of my measurements are collected in the following Table:—

R.	k.	R.	k.	R.	k.	R.	k.
4·302	21·54	16·47	113·5	83·26	120·0	195·7	61·93
5·497	23·78	23·21	157·0	91·40	112·2	205·9	59·22
7·017	26·44	32·12	174·2	100·35	108·1	217·0	56·47
9·220	40·95	35·62	172·3	105·03	104·2	228·0	53·92
10·53	51·10	38·14	170·7	111·18	97·12	235·8	52·88
11·51	59·76	40·38	168·9	119·6	93·97	252·2	49·68
12·60	68·70	52·47	161·6	132·6	87·70	272·7	47·29
13·67	76·53	67·89	141·7	140·1	82·08	288·2	44·04
14·94	84·53	71·83	136·2	156·0	75·43	296·1	43·65
15·60	104·48	75·55	132·1	179·3	66·87	307·3	42·13

From these data the continuous curved line in fig. 6 is drawn; its abscissæ represent the values of R, and its ordinates the corresponding values of k. The two dotted lines are drawn after the above-calculated series of experiments of Von Quintus Icilius. We see that at the higher values of R the course of all three curves is very similar, while at the lower values the curve obtained by me has a much steeper ascent.

As is well known, the number k varies when the temperature of the iron undergoes considerable variations. This temperature, in the case of my ring, could not be stated exactly. The temperature at the place of observation sank considerably during the time the investigation was proceeding (September and October 1871); but this was partly compensated by my using gradually more and more powerful decomposing forces, so that the ring was more and more heated by the primary current (partly also, perhaps, by the remagnetizing itself); therefore most of the measurements refer to 15°–20° C. A considerable augmentation of k appeared only when once, towards the close of the investigation, I intentionally caused the current from 14 zinc-carbon elements to pass through the primary wire until it was heated from 10° to 40°; the results of such experiments are not comprised in the Table.

With certain arrangements for the positive determination of the temperature of the iron, similar methods could be employed for the investigation of the influence of temperature on the magnetizing-function. It would moreover be desirable to extend such measurements to various sorts of iron, in which the course of k might exhibit considerable deviations. It would also be to the purpose to make use of thinner rings for the measurements than the one above described, because they must furnish more accurate results, provided that the form of the ring be verified with equal exactness*. It has not yet been possible for me to complete my work in the directions mentioned.

The closer investigation of this subject might present many and various points of interest. On the one hand, a deeper insight would be thereby gained into the essence of that molecular process which we designate as the magnetization of a substance. From the facts here considered it seems to follow that the hypothesis of the existence in iron of convertible molecular magnets, in the form in which it is developed by Weber†, does not correspond to the course of the phenomenon with feebler decomposing forces. On the other hand, more exact knowledge of the function k might be of practical utility, especially in the construction both of electromagnetic motors and of those magneto-electrical machines of a newer kind (those of Wilde, Siemens, Ladd, &c.) in which the temporary magnetizing of iron plays so important a part.

In conclusion, to Geh. Hofrath Kirchhoff, in whose laboratory this research was carried on, and who kindly assisted me with his advice, I most heartily express my gratitude.

Heidelberg, October 1871.

VI. *Experiments on Fluorescence.* *By* Eduard Hagenbach‡.

It had long been observed that a certain number of solutions —for example, of nephritic wood (*lignum nephriticum*)— possessed the property of giving, in incident light, a peculiar reflection, quite different from the colour presented by the same bodies in transmitted light. Brewster and Herschel were the first to recognize that this property extended to a great number of substances; they thought to explain it, the one by internal dispersion, the other by a peculiar reflection at the surface. It is to Stokes that we are indebted for having established that the change thus produced in the composition of the incident light does not result solely from reflection or absorption, but from the substance itself becoming luminous under the influence of incident light and emitting radiations whose refrangibility is different from that of the exciting rays. It was, again, this savant who introduced into science the word "fluorescence" to designate this phenomenon, on account of its being particularly noticeable on certain varieties of fluor spar.

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* Equation (1) for example, is strictly correct only under the assumption that in a cross section of the ring the number k can always be regarded as a *linear function* of ρ. The Table of results proves that even with my ring (in which the quantity ρ, or also the decomposing force, proportional to $\frac{1}{\rho}$, varies 10 per cent. within the cross section) this was almost everywhere admissible.

† *Electrodyn. Maassbest.* vol. iii. art. 26.

‡ Abstract, by the Author, of the complete Memoir in Poggendorff's *Annalen*, vol. cxlvi. pp. 65, 232, 375, & 508.

Stokes studied a great number of substances with respect to their fluorescence, and in this way arrived at the important law that the refrangibility of the light of fluorescence is never greater than that of the exciting rays. The results obtained by Stokes have since been confirmed and extended by the labours of other physicists, particularly M. V. Pierre.

I was induced to undertake in my turn the study of this subject, because from different quarters there were sent to me, to be submitted to a more searching examination, substances possessing in a high degree this interesting property. I soon perceived that a theory of the singular phenomenon would not be possible until we possessed more exact observations than those which had hitherto been made. I extended my researches not only to these new substances, but also to those previously studied. I turned my attention more particularly to three points, namely:—

1st. Determination of the limits and the maxima of fluorescence: the question here was, to ascertain in what parts of the spectrum fluorescence begins and ends—and to see in each particular case if there was one or several maxima, always fixing their exact position.

The method which I made use of for this purpose consisted in the direct projection of the solar spectrum on the surface of the liquid.

2nd. Study of the absorption-spectrum of the fluorescent substances, and verification of the relation, first clearly established by Stokes, between absorption and fluorescence—consisting in this, that in all portions of the spectrum where there is fluorescence there is also absorption of the exciting rays.

3rd. Spectral analysis of the light emitted by fluorescence. Here the object was, to know the limits between which the spectrum produced by fluorescence extends, and to fix the position of the maximum or maxima, according as the spectrum presented only one or several luminous bands separated by dark portions. In this part of my work I applied myself to find out especially whether modifications in the composition of the incident light (that is to say, that which excites the fluorescence) involve modifications in the light emitted by the fluorescent substance: with this aim I employed different sources of light for its production.

The method employed in these researches was, to throw the light of fluorescence, by a mirror properly arranged for the purpose, on the slit of a large spectroscope, with the aid of which it was analyzed.

I studied, in all, thirty-six substances. I shall mention here only some which are perhaps less known.

Solution of Morine Alum.—This liquid, which gives a fine fluorescence, is obtained (as M. Goppelsröder has shown) by dissolving precipitated morine alum (Cuba lac) in alcohol acidulated with chlorhydric acid.

Naphthaline-rose (called in England Magdala red) dissolved in alcohol.—This substance, one of the colouring-matters obtained from tar, was discovered by M. Schiendl, of Vienna, and was afterwards studied by M. A. W. Hofmann. It, too, exhibits a remarkable fluorescence, intense and beautiful, of yellow light.

Thiomelic Acid.—Whoever has made any elementary studies in chemistry knows that after heating for some time a mixture of alcohol and sulphuric acid, as is done in the preparation of olefiant gas, we obtain a thick greenish black residuum. M. Erdmann has given to this substance the name of thiomelic acid. M. Goppelsröder drew my attention to the remarkable fluorescence of this liquid.

Amide of Phthalic and *Amide of Terephthalic Acids.*—Under this double denomination I designate two substances, the solutions of which in alcohol and ether give a beautiful green and blue fluorescence, and which M. Hugo Müller, of London, has described as amides obtained by the reduction of nitrophthalic and nitroterephthalic acids.

Phthaleine of Resorcine or *Fluoresceine.*—This substance, which gives a magnificent green fluorescence, was sent me by M. Ad. Bæyer.

I shall not here give in detail the results of my observations; they will be found in full in my complete memoir in Poggendorff's *Annalen*; but I will come at once to the general conclusions set forth by my labours. And first I must remark that the property in question presents itself under the most varied forms. There can be no doubt that here we have to do with a phenomenon which, conformably to what takes place with most physical effects, depends essentially on molecular constitution and chemical composition—each substance appearing as an isolated individual with special characteristic properties, in such manner that it is very difficult to lay down general laws sufficiently exact. In the sequel I shall endeavour to demonstrate this view.

Fluorescence in the Spectrum or *Fluorescent Spectrum.*—To the question whether all the radiations of the spectrum are capable of exciting fluorescence, we can answer in the affirmative. Only the rays beyond B can be excluded from the number of those which produce fluorescence; at least I know of no substance which is fluorescent in that portion of the spectrum. But when we consider that the light emitted by fluorescence is less refrangible than the exciting rays, we can well understand how it is that fluorescence excited in the extreme red is not visible to our eyes.

As regards the greater or less extent of fluorescence in the spectrum, great differences are found between different substances. There are cases in which fluorescence commences in the violet, after the line G—for example, fluor spar, and a solution which I first studied and which must contain bisulphanthrachinon; while in other cases it extends nearly through the spectrum, as takes place with ethereal and alcoholic solution of chlorophyl, alcoholic solution of naphthaline-rose and thiomelic acid. In the direction of the extreme violet, fluorescence always extends beyond the group H.

In respect of intensity in the different parts of the spectrum this remarkable fact presents itself—that in a great number of cases several different maxima of fluorescence are observed, separated by bands of relative minimum. These maxima are not all equally marked; the difference of brightness between them and the minima likewise varies.

The number of these maxima also is very variable. Thus, for example, seven are observed with fresh solution of chlorophyl, five with solution of lampblack in alcohol or turpentine, three with naphthaline-rose, alcoholic solution of turnsole, purpurine in alum, and uranium glass, two with the alcoholic solution of guaiacum resin, and only one with solution of morine alum and thiomelic acid and with the solutions of sulphate of quinine, æsculine, fraxine, photene or anthracene, petroleum, and nitrate of uranium.

Correlation of Fluorescence and Absorption.—The fact that wherever incident light produces fluorescence, absorption exists at the same time, is a consequence of the law of the conservation of *vis viva* which it was easy to foresee. In my observations I continually saw absorption accompany fluorescence; often the absorption-spectrum could even serve to determine more precisely the maxima of fluorescence. What is still more remarkable is the inverse fact recognized by Stokes, that, in fluorescent substances in general, absorption is always accompanied by fluorescence, which was not necessarily to be foreseen; for in other coloured liquids there is often absorption of light without fluorescence. On this last point in particular, although my researches have fully confirmed the connexion already known between the two, yet I have found cases in which the substance studied exhibited in certain places a peculiar absorption in addition to that corresponding to the fluorescence. It is so, for example, with the aqueous solution of turnsol, solution of purpurine in soda, and nitrate of uranium either solid or dissolved.

Spectra of the Light emitted by Fluorescence, or *Fluorescence-Spectra.*—I have given particular attention to the study and description of fluorescence-spectra, which are presented under the most varied forms. I shall here notice only those which appear to me of special interest.

The different kinds of light already known to be emitted by fluorescence present the greatest variety in their colours, according as they are red, orange, yellow, green, blue, or violet; but it is only by spectral analysis that an exact idea can be obtained of their composition.

It is moreover established that there are very great differences in the breadth of the fluorescence-spectrum. That of chlorophyl has the least breadth; it is so narrow that we might almost suppose that the light thus emitted is a homogeneous red. Those of thiomelic acid, fluor spar, and others, on the contrary, exhibit a very great breadth; the fluorescence-spectra of these substances include rays belonging to the different portions of the spectrum from the red to the violet.

An interesting point to note is, that in a great number of cases the fluorescence-spectrum offers a very unequal distribution of luminous intensity: we have in them a series of brighter bands, maxima of luminous intensity, separated from one another by other bands more or less obscure; it is right, however, to remark that these bands are not sharply defined, but pass gradually into one another; and the dark bands are not absolutely black, but more or less shaded zones. I have observed, for example, eight maxima in the fluorescence-spectrum of nitrate of uranium, six maxima in that of photene or anthracene and in that of petroleum, five maxima with uranium glass and the extract of lampblack, three very distinct with solution of guaiacum, three less decided with fluoresceine, two equally well-marked maxima with alcoholic solution of turnsol, solution of orchil, and fresh solution of chlorophyl in ether, two maxima also, but less clear, with oxide of brasiline, sulphate of quinine, æsculine, and tincture of curcuma, and only one maximum (consequently no intermission in the intensity of the light) with solution of morine alum, naphthaline-rose, thiomelic acid, and fluor spar.

The difference in luminous intensity between the maxima and minima in the fluorescence-spectra varies a great deal: the maxima in the case of nitrate of uranium form very neat bands with sharply defined edges; while with fluoresceine, which gives a fluorescence identical in appearance with that of the former solution, similar intermissions are hardly to be distinguished.

It might be supposed that the interruptions in the spectrum projected upon the surface of a fluorescent substance correspond to the intermissions of its fluorescence-spectrum, the one being the determining cause of the other: there are cases in which we might believe such a coincidence to exist; solutions of lampblack, for instance, give five maxima in the spectrum thrown on their surface, and likewise five maxima in the fluorescence-spectrum. But what is observed in a great number of other cases demonstrates that in no instance have we here to do with a simple relation. Fresh solution of chlorophyl gives seven maxima in the spectrum projected upon its surface, but only two in its fluorescence-spectrum; naphthaline-rose gives three very pronounced maxima in the first case, and no intermission in the second; on the contrary, nitrate of uranium, which gives no interruption in the spectrum thrown on its surface, shows two very marked maxima in the spectrum of its fluorescence.

Intermission in the projected spectrum, as well as in that of fluorescence, may in some cases, as those of the solutions of guaiacum, purpurine, orchil, and turnsol, be explained by the fact that they are mixtures of several colouring-matters. Nevertheless in other cases, where the colouring substance is a pure crystallizable body, one can no longer see in mixture the cause of the intermission.

Stokes's Law.

We have already mentioned, at the opening of this article, the law of Stokes, which states that the light emitted by fluorescence is never more refrangible than the exciting rays.

M. Lommel has recently contested the accuracy of this law, on the ground of a series of observations made by him on naphthaline-rose and chlorophyl. My own experiments have contrarily led me to recognize the perfect correctness of Stokes's law in all possible cases. The other laws which it has been thought might be introduced into the domain of fluorescence have not found their verification in these researches.

Influence of the Solvent.

The liquid in which the fluorescent substance is dissolved exerts sometimes an influence on the nature of the light emitted; but here, again, no precise rule can be established. The influence of the solvent manifests itself in many cases by displacement of the bands of maximum in the projected spectrum; thus the solutions of the amide of phthalic acid, of chlorophyl, purpurine, &c. are distinguished from the others by the bands being nearer to the violet. The colour of the fluorescence and the position of the maxima in the fluorescence-spectrum also change in some cases with the nature of the solvent; this is seen, for instance, with the amide of phthalic acid and with solutions of lamp-black. On the other hand, there are cases in which the solvent appears to exert no influence, either on the spectrum thrown on the surface of the liquid or on the fluorescence-spectrum.

Influence of the State of Aggregation.

When a substance is fluorescent in the solid state, does it necessarily follow that it must also be so in the liquid state, and *vice versâ*? The answer to this question differs according to the different substances.

There are substances, as the double cyanide of platinum and barium, which are fluorescent in the solid and not at all in the liquid state. There are others, such as nitrate of uranium, which as solids give a very intense fluorescence, and as solutions give only a very faint one. There are also substances which are very fluorescent in both states of aggregation; it is so with photene, sugar of malt, and tincture of curcuma. Moreover some substances, feebly fluorescent in the solid state, are very strongly so in solution—*e. g.* æsculine, sulphate of quinine, chlorophyl, and the amides of phthalic and terephthalic acids. There are, lastly, substances which are not at all fluorescent in the solid state, while they are so in the liquid condition—for example, naphthaline-rose.

Relation between Phosphorescence and Fluorescence.

It is at present impossible to decide whether phosphorescence and fluorescence are two altogether distinct phenomena, or whether there is an insensible transition from one to the other; the presumptions, however, are in favour of the latter view.

This question can only be satisfactorily answered when we have succeeded in ascertaining in some substances the persistence of fluorescence, even if for only a very short time. I have tried to obtain this result, but have not succeeded; nevertheless I do not pretend to give this fact as decisive, because my apparatus could only make perceptible a persistence of $\frac{1}{1300}$ of a second or more, while it would be indispensable, for this class of experiments, to operate with apparatus very much more delicate.

I remark further that the fluorescence-spectra with intermission which we have demonstrated for a great number of substances are evidently analogous to the spectra of phosphorescent substances studied and described by M. Edm. Becquerel. This fact would also tend to connect the two classes of phenomena.

Theory of Fluorescence.

I do not intend here to launch into a criticism of the different theories by which it has been attempted to explain fluorescence. I will merely affirm that none of those theories is adequate to account for the immense variety of the phenomena, and that it will be long before any future theory will be so.

The ideas expressed by Stokes are still at present the best basis for an attempt at a theory of fluorescence. Thus we must admit with him that the undulations of the æther which strike the fluorescent body set its molecules in motion and cause it to become self-luminous. In this there is a certain analogy with the acoustic phenomenon of bodies vibrating in unison. In one point, however, the difference is very great; spectral analysis of the light of fluorescence excited by homogeneous light does not give homogeneous light, but an infinity of radiations of different wave-lengths. In this respect fluorescence-spectra approach the spectra of incandescent solids. If the light emanating from a homogeneous substance thus presents an infinite variety of wave-length, this can only be explained, as Stokes has already shown, by the action of forces which are not merely proportional to the first power of the amplitude, and in this way produce undulations for which the period of an oscillation is a function of the amplitude. Undulations of this kind must be admitted in the case of an incandescent solid; for without that we cannot explain to ourselves the continuity of the spectrum. The incandescence of a solid body constitutes, in my opinion, a relatively simple problem, the solution of which it is indispensable to have before we pretend to explain the emission of light by fluorescence. The light emitted by incandescent solids of utterly different natures is, as we know, identical; it is therefore independent of their intimate molecular constitution, which, on the contrary, has a notable influence in the case of fluorescence, and adds to the complication of the phenomenon. It is clear that, besides the theoretical considerations which Stokes was obliged to use in order to justify the law which bears his name, there are more than one point which it is important equally to take account of—not only the molecular constitution of the substances, but also the more or less great mass of the material molecule compared with that of the ether atom which puts it in motion. It is, besides, only by means of a complete theory of fluorescence that any one will arrive at a satisfactory explanation of Stokes's law. What has hitherto been attempted of the kind is only a number of more or less venturesome hypotheses. I affirm only that theories deviating from that law would deserve no credence.

VII. *Notices respecting New Books.*

The Earth a Great Magnet. By ALFRED MARSHALL MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology.* London: Trübner and Co.

THIS is the report of a lecture delivered before the Yale Scientific Club on February 14, 1872, in which the lecturer proposed to present to his audience "*one prominent truth* in simple and striking experiments." The truth which is kept steadily before the mind throughout the lecture is, "that the earth is a great magnet;" and this truth is developed, step by step, by experiments of the most conclusive kind, each having been rendered distinctly visible to the audience by means of the *vertical* lantern, so that the processes of magnetizing and demagnetizing, with all the interesting motions of the needles, were seen projected on a luminous screen of eighteen feet diameter.

The lecture itself is a masterly production, and exhibits the result of much close reading as well as experimental research. Quotations are given from earlier writers on magnetism, illustrative of the sound knowledge which they possessed; and as each experiment illustrative of the lecture is described as well as the apparatus employed in manipulation, the reader is conducted from a consideration of the most ordinary magnetic phenomena presented by bar and electro magnets, to that of the same phenomena evolved from terrestrial magnetism. A paragraph selected from the closing portion of the lecture will fully substantiate this statement.

"Now we have finished our experiments; and what have they shown? I have temporarily magnetized a bar of soft iron, by pointing it towards a pole of our large magnet. I did the same with the bar and the earth. I permanently magnetized an iron bar, by directing its length towards the pole of the magnet, and vibrating it with a blow of a hammer. I did the same with a bar, struck when pointed towards the earth's magnetic pole. I have shown you the action of a small magnetic disk on iron filings placed above and around it. You saw that the earth produced the same action on the beams of the aurora. I showed you the action of this disk on a freely suspended magnetic needle, and pointed out to you the earth's similar action on a dipping-needle carried over its surface. I have evolved a current of electricity from a magnet, by cutting with a closed conductor across those lines in which a magnetic needle freely suspended places its length. I did the same with the earth by cutting across those lines which are marked out by the pointing of the dipping-needle. Therefore, what am I authorized to infer? When the effects are the same, the causes must be the same; for according to all the principles of philosophy, and conformably to that universal experience which we call common sense, like causes produce like effects."

To those who are desirous of possessing in a compressed form the leading facts of terrestrial magnetism, we strongly recommend a perusal of the lecture.

War Department Weather Maps. Signal-Service, United-States Army. Friday, November 22, 1872.

We have received the three maps of the above date, 7.35 A.M. 4.35 P.M., and 11 P.M. Washington time respectively. An improvement has been introduced, particularly in the insertion of the Oceanic Currents with their velocities per hour. The land surface is coloured green, the principal mountain-ranges shown, and elevations above 8000 feet left white. A distinction is made between surfaces above and below 800 feet, the former being darkly shaded, the latter lightly. The ordinary isobars, isotherms, state of weather, &c. are shown as formerly.

One of the latest results of the operations of the American Signal-Office, as reported in the 'New York Herald,' Nov. 16, is the discovery of a vast aërial wave which passed over the States in November. It originated in the Pacific Ocean, and was traced distinctly over the whole breadth of the continent; so that America as well as Western Europe has a "great November wave." The American storm-warnings in connexion with this wave were issued from two to three days in advance.

The issue of three maps daily is of great advantage, as it contributes so much more efficiently to an acquaintance with atmospheric changes, than by an issue of one map daily. The weather maps of the English Meteorological Office are of great utility; but this utility would be greatly increased by two additional maps being issued daily, for 4 P.M. and midnight. Probabilities, as on the American plan, could then be deduced with more or less success, either officially or privately by subscribers.

The Atmosphere of the Sun. The Rede Lecture, 1871. *By* J. NORMAN LOCKYER, *F.R.S.* London: Macmillan and Co.

This Lecture contains a *résumé* of the work effected by Mr. Lockyer in connexion with Dr. Frankland, relative to their joint researches on Solar Physics, an account of which has for some time been before the public. The lecture, which is well conceived, is based upon Newton's query of the sun and fixed stars being great earths vehemently hot, from which vapours and exhalations arise and are compressed and condensed by the vast weight and density of their atmosphere.

The remarks of Mr. Lockyer, bearing on the reply to Newton's query, are of somewhat a desponding character, inasmuch as they allude to the more than century and half century that the world has had to wait for the "outcome" of the splendid generalization by which the proof has been obtained, that the sun and fixed stars are undoubtedly "vehemently hot." Nor is his tone more encouraging towards the close of his lecture; for in speaking of our witnessing the birth of a new science, he says it is one which requires costly apparatus and appliances above the means of most individuals, with thoroughly organized work extending over centuries. How then is the science of the sun to progress?

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xliv. p. 541.]

June 20, 1872.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

THE following communication was read:—

"On Supersaturated Saline Solutions." By Archibald Liversidge, Assoc. R.S. Mines.

There is, perhaps, no necessity to describe in detail the ordinary phenomena presented by supersaturated saline solutions, since they must now be well known to all.

The following series of experiments have chiefly been made upon sodic sulphate; but before citing them, it may, however, not be out of place to briefly allude, *en passant*, to the conclusions drawn by the numerous writers and experimenters upon this subject, since the results of my own experiments are supported by the authority of some of these observers and run counter to that of others.

The theories which have been put forth are, in the main, as follows:—

α. That the crystallization of supersaturated solutions is caused by purely mechanical agencies, such as agitation &c. The principal supporter of this view was Gay-Lussac, who wrote in 1819. It has since been shown to be utterly untenable.

β. That the sudden crystallization is due to some unknown catalytic force. Advocated by Lowell in 1850, but since disproved.

γ. That it is due to the entrance of a particle of the same salt. This explanation is favoured by the majority of the writers upon the question, such as Zis in 1809, Gernez in 1851, Violette in 1860, Dubrunfaut in 1869, by Lecoy de Boisdandran, and others.

δ. That crystallization is due to the presence of fatty, oily, greasy, or other matters in the form of thin films. This theory was propounded by Mr. Tomlinson in two papers* read before the Royal Society, in which also it is stated that certain liquids, such as absolute alcohol, act as nuclei in determining the solidification of such solutions by separating water from the solution, whereas the thin film, on the contrary, owes its activity to the greater attraction which it has for the salt held in solution.

Preparation of the Supersaturated Saline Solution.

A little water is placed in the flask, boiled, and sodic sulphate added to the boiling liquid until it ceases to dissolve any more and a deposit of the anhydrous salt begins to take place; the solution is then filtered and transferred to smaller flasks, usually of about 2 oz. capacity; these are then again boiled up after being covered with a small beaker, watch-glass, or plugged with cotton-wool. By this method any nuclei adhering to the watch-glass, beaker, or wool are rendered inactive, even should they fall into the solution.

The solutions are always used of such a degree of supersaturation that crystals of the anhydrous salt are deposited during the boiling.

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* Phil. Trans. vol. clviii. pt. ii. and vol. clxi. pt. i.

Do some liquids, such as alcohol, act as nuclei by combining with a portion of the water of the solution and liberating a little salt which acts as a nucleus?

Exp. Supersaturated solutions of sodic sulphate were prepared, in the manner described, in 2-oz. flasks, which were closed with a plug of cotton-wool through which a bulb-tube was passed, of the form figured*, containing absolute alcohol.

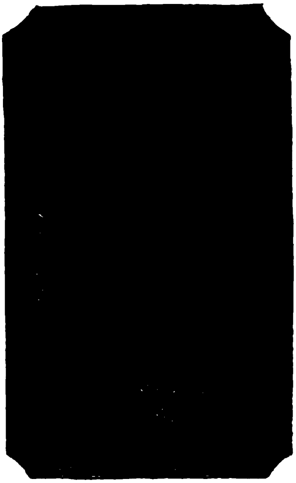
After waiting some time to be certain that nuclei had not gained admittance, some of the alcohol was run out on to the surface of the solution by momentarily loosening the stopper.

This experiment was repeated many times, at different temperatures and with alcohol of various strengths; but never did the alcohol act as a nucleus.

Previously to the experiment the alcohol had been boiled to destroy nuclei.

Exp. Concentrated sulphuric acid was substituted for the alcohol, but likewise with no result. The smallest quantity of acid was added, so as to prevent any undue rise in temperature, which would of course vitiate the result. The flask was likewise kept cold by a stream of water.

In a later form of these experiments, a small glass bulb with a long neck blown from glass tubing, such as is used in the elementary analysis of a fluid by combustion, was made use of.



The bulb was first well heated in a Bunsen burner, so as to destroy any nuclei which might adhere to it; then, while still hot, the open end was dipped into the alcohol or acid under trial, when, of course, as the air in the bulb cooled some of the liquid was forced up into it; its liquid contents were then boiled and the open end again dipped into the fluid; and as the vapour condensed, more fluid was forced up into it.

The tube was then surrounded by cotton-wool and inserted into the neck of the flask, and the supersaturated solution boiled up for a moment, so as to render the whole apparatus, cotton-wool included, inactive, the steam escaping through the interstices of the cotton and not affecting the spirit. When cold a drop of the spirit or acid was delivered by merely heating the glass bulb.

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* The loop was made in the tube at *a* so as to prevent any fluid from escaping until required.

Exp. Trial was next made of several solid dehydrating substances, such as calcic chloride, anhydrous chromic acid, phosphoric anhydride, freshly ignited quicklime, &c.

These bodies were placed in sealed thin glass bulbs and heated nearly to redness and then dropped into the supersaturated solution; the flasks were plugged with cotton-wool, through which a glass rod passed, and boiled up, after which they were allowed to cool for some hours; when quite cold the bulb was broken by means of the glass rod and its contents set free, but, as in the case of the liquid, with no result.

It should perhaps here be mentioned that each flask was always proved to be thoroughly supersaturated by dropping in a crystal of the salt or touching the solution with a dirty rod, after the substance made trial of was found to be wanting in nuclear power.

From the foregoing it appears that the crystallization of supersaturated saline solutions is not determined by the removal of water by chemical agency; neither do porous bodies, like wood, charcoal, sponge, spongy platinum, earthenware, &c., determine the solidification of solutions by mechanical absorption of the water.

Concerning the action of thin films.

In the same paper it is stated that while oils, fats, and greasy bodies generally do not act as nuclei when chemically clean and in the bulk, *i. e.* in the form of a solid mass, lens, or drop, yet these identical bodies when in the form of thin films do act as nuclei, and that any substance which possesses a nuclear action has derived such power from having become contaminated with a thin film of greasy matter, which it acquires by handling, wiping with a dirty cloth, or by mere exposure to the air containing the products of respiration and other excretions, &c.

Thus in the series of experiments detailed it was found that such bodies as ether, absolute alcohol, naphtha, turpentine, herring-oil, sperm-oil, castor-oil, and many others, while in the form of a lens or globule, did not act upon a supersaturated solution, but did immediately when spread out into a thin film.

It should be noticed that the oil was added to the solution by removing the cover of the flask, delivering the drop, and then replacing the cover; or a glass tube was used provided with a shield covering the mouth of the flask: both methods have the great objection of exposing the solution to the air, and so allowing nuclei to gain access.

It is stated that if the finger be cleaned by washing it in alcohol or caustic potash, or by passing it through the flame of a spirit-lamp, it may be held in a supersaturated solution for some time without causing crystallization—but that if it be rubbed against the sides of the flask, a greasy smear is produced which at once acts.

The writer has repeated this form of experiment several times, but never with the above result when sufficient care had been taken to free the finger from nuclei.

Exp. The finger was made greasy by dipping it into oil and imperfectly wiping it with a cloth; it was then passed many times through the flame of a spirit-lamp, and finally, while still far above its normal temperature, inserted into a flask of supersaturated solution: the flask was chosen with a neck such that it could be entirely closed by the thicker part of the finger. The flask was then transferred to a vessel of water, lowered artificially to 38° F., and there kept, with the finger still in it, for several minutes, varying in different experiments from 10 to 15, 20, 25, 30, and 35 minutes; and although the finger was strongly pressed against the sides of the flask, which was seen to be smeared all over, yet crystallization was not set up when the solution was made to flow over the finger-marks, which were plainly visible. That the solutions were not warmed by the heat of the finger, and so rendered inactive, is proved by their immediately solidifying on the insertion of a dirty glass rod.

Exp. By means of the two modifications of bulb-tube already described for the experiments with absolute alcohol, thin films of various oils and other bodies were formed upon the surface of supersaturated solutions without inducing crystallization. That is, a small glass bulb was filled with the oil or other body and boiled, then supported in the neck of the flask by a plug of cotton; the supersaturated solution was then boiled and allowed to cool; when quite cold a drop of the liquid was forced out of the bulb on to the solution; then by a sudden jerk the lens or small globule thus obtained was flattened out into a thin film, often iridescent—but without causing solidification.

In numerous instances the temperature of the solution was lowered by means of ice-cold water, so as to increase its sensitiveness, but with no different result.

In many cases the oil or fatty body, such as olive-oil, Russian tallow, citronella-oil, castor-oil, &c., was dissolved in ether and then used; this device was used for two reasons:—first, so that the greasy matter might be much diluted and so spread over a large surface, and then be left as a thin film on the evaporation of the ether; and second, so that a much smaller quantity of the oil might be delivered at a time. Usually the oil collected into globules shortly after the evaporation of the ether, but could generally be spread out into a film again by imparting a sharp twist to the flask.

Supersaturated solutions of sodic sulphate having films of oil, benzole, turpentine, citronella-oil, &c. upon their surface have been kept by the writer for several months together, and some even as long as eighteen months: it is true that the oil &c. soon lost the form of an iridescent film, but could be made to assume it at any moment; and the above lot of flasks were seldom allowed to stand for a day without being made to do so, *i.e.* for the first three months after their preparation and at greater intervals afterwards. Every now and then a flask was caused to crystallize in order to ascertain that the solutions had in no way lost their sensitiveness to a dirty rod; and when the last flask of all was proved, it had stood for rather more than eighteen months.

One explanation accounting for the activity of the thin film as prepared by the eminent author of the paper referred to may be this:—That in order to place the oil upon the solution, the flask was opened and exposed to the air, thus affording an opportunity for nuclei to gain entrance; and also they may have been carried in by the greasy rod itself, for there would be plenty of time in its passage for it to pick nuclei up: such nuclear bodies would probably float upon the surface of the disk or globules of oil, and would not come into contact with the solution itself; neither might they touch its surface even when the disk was broken up into small globules, for these globules would be immensely large in comparison with the dimensions of the nucleus itself; but, on the other hand, when the disk was flattened out into an iridescent film, and therefore one of excessive tenuity, the nuclei might then easily fall through it, come into contact with the supersaturated solution, and start its crystallization. As it is probable that several nuclei would enter at the same time, they would naturally become dispersed by the jerk, and hence crystallization would be set up at various points.

That nuclei will pass through the substance of a thin film is shown by the solidification which almost immediately takes place on exposing to air the solution covered merely by a film of oil, turpentine, &c.; a thick coating of oil is, of course, one of the best means we have of protecting a supersaturated solution from nuclei.

The principal substances made use of by the writer for the formation of thin films were as follows:—Citronella-oil, olive-oil, Russian tallow, castor-oil, camphor in alcohol, creosote, turpentine, benzole, chloroform, ether, &c.

Concerning the action of a crystal of the normal sodic sulphate upon a supersaturated solution of the same.

It is well known that there are three modifications of sodic sulphate crystals:—

1. *The anhydrous salt* (Na_2SO_4), crystallizing in octahedra, and deposited from a supersaturated solution on further concentration; these crystals are inactive to a supersaturated solution.

2. *The modified salt* ($Na_2SO_4, 7H_2O$), containing $7H_2O$, formed in a supersaturated solution by reduction of temperature and other causes; these also are inactive, and admitted to be so by all.

3. *The normal salt* ($Na_2SO_4, 10H_2O$), crystallizing in prisms with dihedral summits, and containing $10H_2O$. Usually regarded as the best nucleus. Experiments relating to its behaviour as such will be detailed.

It is always the normal salt ($Na_2SO_4, 10H_2O$) which is formed when a solution is caused to crystallize by touching it with a dirty rod or by exposing it to the air, &c.

Experiments were made with recently generated crystals of the normal salt.

Exp. Two beakers, containing fully supersaturated solutions, were covered with watch-glasses, and allowed to cool; in one of the beakers a small glass bucket, attached to a thread, had been placed and boiled up with the solution. Next, both beakers were arranged under a large bell-jar, and the silk thread from the bucket passed up between the stopper and the neck of the jar. The solutions were then uncovered, after waiting ten minutes for any nuclei which might have been disturbed to fall; a fine wire was passed down into the beaker containing the bucket, and as far as possible from the part of the solution through which it would pass on being drawn up.

The bucket, now full of the crystallized normal sodic sulphate ($Na_2SO_4, 10H_2O$), was raised, and lowered into the second beaker of still fluid solution; immediately that the point of one of the crystals hanging from the under surface of the bucket touched the solution, crystallization was set up instantaneously throughout the mass.

This experiment was performed many times, and with every possible care to prevent the entrance of nuclei other than those purposely borne by the wire.

A modification of the above plan was tried and with similar results.

Exp. A tubulated glass bell was fitted with a cork bearing two glass tubes, open below and closed above with cotton-wool; they were bent so as to permit both of them being placed in one and the same beaker, or into either separately.

In the first place, the ends of the tubes inside the bell were freed from nuclei by passing them through a flame; two beakers of cold supersaturated solution were then placed in position under the bell-jar, and their covers removed. After waiting five minutes or so for any dust to settle, both tubes were next lowered into one of the beakers, on opposite sides, so as to be as far apart as possible. A dirty wire was now passed down one of the tubes, when, of course, crystallization immediately took place, and was propagated across the beaker. The second tube, with its adhering crystals, was then raised and lowered into the second beaker, when, the moment the extreme point of the longest crystal touched the surface of the solution, crystallization immediately started from that point, and the whole contents became solid.

A third variation was then made in this experiment. One of the two beakers was replaced by a U-tube of thin, hard glass, one of the before-mentioned tubes being inserted into either limb. Crystallization, when set up in one limb, travelled round the bend and up into the other, from which crystals were transferred, as before, to a beaker or flask of solution also under the bell-jar.

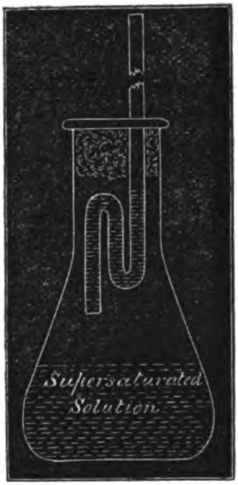
The three modifications of this form of experiment were tried time after time, and always with the same unvarying result. Solutions which were supersaturated although not perfectly, and therefore less sensitive, were operated upon in this way; but, even with such less favourable circumstances, the normal crystals always started crystallization in the solution to which they were added.

To ascertain, if possible, whether nuclei, other than crystals of the normal salt, were carried by the tube or its adhering crystals, a capsule of sulphuric acid was placed under the bell. The crust of crystals was by this means dried, and became effloresced to a greater or less extent. Now, on lowering them into a supersaturated solution of alum or of magnesic sulphate they were proved to be inactive, having been changed to the inactive anhydrous salt.

But such dried normal crystals were active to a solution of sodic sulphate, even after three days' exposure to the sulphuric acid*. It seems as if the normal crystals become covered with a coating of effloresced anhydrous salt, which acts as a protection to the underneath portions in the same way as oxide of lead does to metallic lead; hence it takes a long time to convert a crystal of the normal salt into the anhydrous by simple exposure to dry air, although it is an exceedingly short operation to perform at temperatures superior to 34° C.

Yet another form of this experiment was tried again and again, and always with the same result.

A glass tube bent into the form of an elongated letter S was suspended by a plug of cotton-wool in the neck of a flask containing a supersaturated solution; the solution was boiled, and the tube was also boiled in it, so as to get all nuclear particles adhering to it thoroughly destroyed.



The solution was then allowed to cool, with the tube still in it; the tube was then raised out of the solution and a dirty wire passed down it; crystallization was, of course, set up in the portion of supersaturated solution contained within the tube; the crystals gradually grew down the tube, then through the first bend, travelled up the upright portion, then travelled round the second bend, and finally down the third and last straight portion. Now, on lowering the extreme tip of the crystals formed at the end of the tube into the solution, crystallization was immediately set up from it as a centre, and thence throughout the mass.

By this arrangement access of extraneous nuclei was entirely prevented. The upper end of the tube was plugged with cotton-wool until the dirty wire was passed down.

That the normal crystals thus formed did not act by any transient molecular movements, which recently formed crystals might be supposed to have, is proved doubtless by the fact that such crystals were found to act just as readily even when they had been kept over the solution for 2½, 5, 10, 24, and 48 hours, and then lowered into the solution, and when any molecular agitation may with fairness be supposed to have ceased.

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* At a future day I hope to have the results of more experiments upon this point.

Exp. Supersaturated solutions of common potash alum were treated in the same way and with the like results; alum, perhaps, affords a prettier example even than sodic sulphate, since the crystals formed in the tube are of an opaque white, and can therefore be more readily observed during their growth.

Exp. Supersaturated solutions of magnesic sulphate were also operated upon and with the same success; but the experiment is not so striking, owing to the much longer time required by magnesic sulphate to crystallize.

Although pure clean crystals of the normal sodic sulphate are active to a supersaturated solution of sodic sulphate, yet, as might be expected, they are not active to a similar solution of alum or magnesic sulphate, and *vice versâ.*

For example, let us take a supersaturated solution of alum, and one of sodic sulphate, and also crystals of both their salts, which crystals have just formed and are taken from their still warm mother-liquors.

Exp. A crystal of alum from its mother-liquor was added to a supersaturated solution of alum. Crystallization immediately took place.

Exp. A like crystal of alum was then added to a supersaturated solution of sodic sulphate. No effect.

Exp. A crystal of the normal salt was taken from its mother-liquor and added to a solution of sodic sulphate. The solution instantly crystallized, although another crystal was inactive to a solution of alum.

Exp. A crystal of magnesic sulphate was added to solutions of alum and of sodic sulphate respectively. No effect on either, but active in a solution of magnesic sulphate.

Concerning the composition of the crystals of sodic sulphate formed by spontaneous evaporation of a supersaturated solution of the same.

When a supersaturated solution of sodic sulphate is allowed to evaporate spontaneously, a crust or ring of crystals forms on the surface of the solution, or a ring in the upper part of the vessel; these crystals are perfectly inactive, as has long been known; and this has been accounted for by regarding them as crystals of the modified salt ($Na_2SO_4, 7H_2O$), which is non-nuclear: but recently they have been regarded as crystals of the normal salt ($Na_2SO_4, 10H_2O$), and their want of action upon the supersaturated solution has been explained by saying that, unlike crystals which have been exposed to the air, they are chemically clean, and therefore free from any film of greasy or other matter; for this writer views the activity shown by the normal salt as being entirely due to impurity of this kind, and not as due to any property inherent in it.

Lōwel made analyses of this salt, formed by spontaneous evaporation, and found it to consist of the modified salt containing $Na_2SO_4, 7H_2O$.

Faraday also examined it and came to much the same conclusion, only that he gave it $8H_2O$, instead of $7H_2O$. There is no doubt that Faraday obtained this salt and not the normal with $10H_2O$, although he made what has since been proved to be a mistake in assigning $8H_2O$ to the modified salt.

The writer allowed some supersaturated solutions of sodic sulphate to evaporate spontaneously, and, after several vain attempts, at last succeeded in obtaining good crops of such crystals, without admixture of the normal salt, which, of course, is liable to crystallize out also on opening the receiver. The ring of crystals at the top of the solution only were taken.

Results of determinations of water of crystallization in crystals of sodic sulphate formed by spontaneous evaporation.

No. 1. ·365 grm. of salt, on drying in water-oven at 100° C., after first well drying the powdered salt with blotting-paper, lost ·170 grm. =46·57 per cent. OH_2, $=Na_2 SO_4 7OH_2$.

No. 2. ·172 grm. lost ·081 grm.=47·09 per cent.,=$Na_2SO_4, 7OH_2$.
No. 3. 2·708 grms. lost 1·273 grm.=47·00 per cent.,=$Na_2SO_4, 7OH_2$.
No. 4. 1·260 grm. lost ·605 grm.=47·00 per cent.,=$Na_2SO_4, 7OH_2$.
No. 5. 3·936 grms. lost 1·812 grm.=46·69 per cent.,=$Na_2SO_4, 7OH_2$.
No. 6. 3·275 grms. lost 1·520 grm.=46·41 per cent.,=$Na_2SO_4, 7OH_2$.
No. 7. 3·326 grms. lost 1·570 grm.=47·11 per cent.,=$Na_2SO_4, 7OH_2$.

No.	Weight. grms.	Loss. grm.		OH_2, per cent.
1.	·365	·170	=	46·57
2.	·172	·081	=	47·09
3.	2·708	1·273	=	47·00
4.	1·260	·605	=	47·00
5.	3·936	1·812	=	46·69
6.	3·275	1·520	=	46·41
7.	3·326	1·570	=	47·11

I trust that by the above-mentioned results I have clearly proved the following facts with respect to supersaturated solutions of sodic sulphate:—

1. That liquids and solids, such as alcohol, quicklime, &c., do not determine crystallization by removing water.
2. That thin films, when sufficient precautions are taken to guard against the entrance of nuclei, do not act as nuclei.
3. That chemically clean crystals of the normal salt ($Na_2 SO_4$, $10H_2O$) *do act* as nuclei and are most powerful.
4. That crystals of the normal salt are not produced in supersaturated solutions of sodic sulphate on allowing it to evaporate spontaneously, but that crystals of the modified (and known inactive) salt are.

In conclusion I may perhaps be permitted to state that the above series of experiments have extended over a period of three years, less a few months, and that most of them have been repeated a countless number of times, and with every conceivable modification and check. Some few of them have already been published in the 'Chemical News,' but are here referred to again for the sake of comprehensiveness.

At present the writer does not venture to put forth any definite theory respecting the presence and nature of the nuclei which are so universally diffused throughout the atmosphere; but when it is considered how much sodic chloride is constantly present in the air, and what quantities of sulphurous acid are evolved daily, which becomes partly converted into sulphuric acid, the presence of particles of sodic sulphate in the air would not be surprising; and that it does exist is proved by drawing air through water and finding comparatively large quantities in the solid matter arrested by water.

Sodic sulphate solutions, too, crystallize on exposure much more readily than those of any other salt. The other salts which form supersaturated solutions are certainly less diffused than sodic sulphate.

IX. *Intelligence and Miscellaneous Articles.*

ON THE DISTRIBUTION OF MAGNETISM. BY M. JAMIN.

AT the last Meeting of the Academy, M. Trève communicated a note on magnetism, in which he states, among other things, that the poles of a magnet are displaced, being removed from the extremities, when an armature of soft iron is applied. He believes he demonstrates this by placing opposite to the magnet a magnetized needle, by showing that its direction changes after the application of the contact, and by supposing that the pole is in the prolonged direction of the needle.

We ought first to define the word *pole*; we must then remark that, in this experiment, the magnetized needle is submitted to the attractive action of the steel which forms the magnet, of the iron which constitutes the contact, and, again, to the attraction or repulsion of the free magnetism of the magnet. Its direction is along the resultant of these actions—a resultant evidently very complicated, depending on the distance, the weight, and the form of the contact: it is clear that the direction of this resultant cannot lead to any precise conclusion.

What it is necessary to determine here is the distribution of the magnetism in the magnet, both before and after the application of the contact; this is a question with which I have long occupied myself, and on which I will now say a few words.

I study this distribution by two mutually corroborative processes. The first consists in placing upon a point of the magnet a small electromagnet of soft iron enveloped in copper wire which communicates with a galvanometer, and suddenly pulling it away. An induction-current is produced, a galvanometric deflection; and from the magnitude of the arc of impulsion I can calculate, after a suitable graduation, the magnetic intensity at the point touched.

The second process, analogous to Coulomb's, consists in placing on the point one wishes to study a small sphere of soft iron supported by a spring, which is stretched progressively till separation takes place. Its tension at that moment is proportional to the square of the magnetism at the point of contact.

If the magnet has its extremities free, it is ascertained that the free magnetism increases gradually from the neutral line to the extremities. The curve of the intensities is very near that indicated by Coulomb. When a contact is applied, all is changed: two poles make their appearance at the two extremities of the contact; the free magnetism of the magnet partially disappears; it augments with the distance to a maximum, to decrease afterwards towards the middle line of the magnet. The conditions of these modifications are very complicated, and merit a very close study. They contribute to the production of the changes of direction of the needle examined by M. Trève; but in no case can these changes reveal the distribution of the magnetism in the bar, and they cannot be summed up by saying that the poles of the magnet have changed their position.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxv. pp. 1572, 1573.

RELATION BETWEEN THE PRESSURE AND THE VOLUME OF SATURATED AQUEOUS VAPOUR WHICH EXPANDS IN PRODUCING WORK WITH NEITHER ADDITION NOR SUBTRACTION OF HEAT. BY H. RESAL.

Let V, p, ρ, r be the volume, pressure, density, and heat of volatilization of saturated aqueous vapour at t_0, and c the specific heat of water at the same temperature. Admitting that the suffix 0 refers to a determined weight of saturated dry vapour, and the suffix 1 to the vapour not condensed during the expansion, we have, on properly transforming one of Clausius's equations:—

$$\frac{V_1}{V_0}=\frac{273+t_1}{r_1}\frac{\rho_0}{\rho_1}\left(\frac{r_0}{273+t_0}-2\cdot30258\frac{c_0+c_1}{2}\log\frac{273+t_1}{273+t_0}\right).$$

I have considered some values of t_0, successively decreasing by 10 degrees, from 200 to 110; for each of them I diminished t_1 10 degrees, commencing at t_0-10. I was thus able to form Tables giving values of $\frac{V_1}{V_0}$, opposite to which I placed the corresponding values of $\frac{p_0}{p_1}$; and I found that the relation

$$\frac{p_0}{p_1}=\left(\frac{V_1}{V_0}\right)^{1.133}$$

very satisfactorily accords with the elements of those Tables, between the limits 1·25 and 15·37 of $\frac{V_1}{V_0}$.—*Comptes Rendus de l'Acad. des Sciences*, Dec. 2, 1872, p. 1475.

ON THE DEFINITION OF TEMPERATURE IN THE MECHANICAL THEORY OF HEAT, AND THE PHYSICAL INTERPRETATION OF THE SECOND FUNDAMENTAL PRINCIPLE OF THAT THEORY. BY E. MALLARD.

The study of calorific phenomena has led to the introduction into science of two quantities *sui generis*, the calorie and the temperature, which every theory must necessarily define.

Thermodynamics defines the calorie very clearly; it only indirectly defines the temperature, by means of Carnot's theorem: hence the difficult and embarrassed course of the theory. I have proposed to myself to remedy this defect.

In the first part of the work which I have the honour to submit to the Academy I demonstrate that Carnot's theorem is identical with the following:—*The mean* vis viva *of an atom forming part of a body, the absolute temperature of which is* τ, *may be expressed by* $a\tau$, a *being a specific coefficient which can only be dependent on the nature of the atom.*

The following is a compendium of the demonstration of this proposition:—

If we produce a change in the elementary state of a body submitted at every instant to external forces which balance the internal forces, we shall have

$$d\mathfrak{T}+\mathrm{E}d\mathrm{Q}=d\Pi+d\Phi+d\Psi,$$

$d\mathfrak{T}$ being the variation of the external work, dQ the quantity of heat supplied to the body, $d\Pi$ the variation of the potential function corresponding to the displacement of each atom from its mean position, $d\Phi$ the variation of the actual energy, and $d\Psi$ the variation of that part of the potential energy which depends on the vibratory motion. On account of the constant equilibrium between the external and internal forces, we have

$$d\mathfrak{T}=d\Pi,$$

and there remains

$$\mathrm{E}d\mathrm{Q}=d\Phi+d\Psi.$$

Instead of determining, according to custom, the condition of a body at any instant by two quantities such as the pressure p and the volume v, I suppose it determined by two auxiliary variables F and R, chosen so that

$$\mathrm{FR}=2\Phi,\quad \mathrm{F}d\mathrm{R}=d\Psi.$$

The lines of equal energy will then be equilateral hyperbolas

$$\mathrm{FR}=2\Phi. \quad \ldots\ldots\ldots \quad (1)$$

The adiabatic lines satisfy the equation

$$\mathrm{F}d\mathrm{Q}=\mathrm{F}d\mathrm{R}+d\Psi=0,$$

or

$$\mathrm{FR}^3=\text{const.} \quad \ldots\ldots\ldots \quad (2)$$

Equations (1) and (2) having the same forms as the equations of isothermic and adiabatic lines for perfect gases, analogous consequences are deduced from them—namely, that if Q_0 is the quantity of heat supplied to the body, in a cycle analogous to that of Carnot, along the line of equal energy Φ_0, and Q_1 the quantity of heat expended by the body during that cycle along the line of equal energy Φ_1,

$$\frac{Q_0}{Q_1}=\frac{\Phi_0}{\Phi_1}.$$

Let us suppose Q_0 and Q_1 infinitely small, as well as the arcs of the lines of equal energy corresponding to them. Let us draw, through the intersection of the line of equal energy Φ_0 with the adiabatic line on the left, an infinitely small arc of an isothermal curve stopping at the adiabatic line on the right; and let us make the same construction for the point of intersection of the curve of equal energy Φ_1 and the left adiabatic line. The quantities of heat Q_0' and Q_1' which must be respectively supplied to and withdrawn from the body along the isothermal arcs thus traced will, in virtue of Carnot's theorem, be connected by the relation

$$\frac{Q_0'}{Q_1'}=\frac{\tau_0}{\tau_1},$$

τ_0 and τ_1 being the absolute temperatures corresponding to the isothermal lines.

Now it is easily demonstrated that Q_0 and Q_0', Q_1 and Q_1', respectively differ only by infinitely small quantities of the second order. We have therefore

$$\frac{Q_0}{Q_1}=\frac{Q_0'}{Q_1'}=\frac{\tau_0}{\tau_1}=\frac{\Phi_0}{\Phi_1},$$

from which, finally, is deduced the general relation between the mean actual energy Φ and the absolute temperature τ,

$$\Phi=\beta\tau,$$

β being a specific coefficient depending only on the nature of the body.

This equation, true for any body or portion of a body whatever, is also true for an atom; hence the theorem stated above.

In the second part, I demonstrate that if, as experiment indicates, the condition of a body is determined when the atomic arrangement produced by a certain equilibrium between the external and internal forces is known and a single quantity, the temperature, is given, this arises from the circumstance that the atoms, instead of merely being in the presence of each other, are under the influence of the æther—a fluid whose vibration-period has an extremely small duration in comparison with that of the atomic vibration.

I show that the $3n$ expressions which determine the mean *vires vivæ* of the n atoms constituting any body whatever contain $3n$ arbitrary constants, so that the *vires vivæ* are independent of the arrangement and mutual forces of the atoms. They are dependent, therefore, only on the action of the æther; so that the atoms of the body vibrate as if, the body being completely disaggregated, they, immersed in the æther, no longer exerted any action upon one another.

I hence infer, by very simple considerations, that, if ϕ represents the mean *vis viva* of an atom, or of the centre of gravity of any system of atoms in equilibrium of temperature, we have

$$a\phi = f(\tau), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

a being a specific coefficient, and $f(\tau)$ a function of the absolute temperature, which is the same for all the atoms or systems of atoms.

Lastly, from the value, ascertained by experiment, of the *vis viva* of the centre of gravity of the molecule of any perfect gas, I deduce that a is the same for all the atoms or atom-systems, and that equation (3) may be put in the form

$$\phi = a\tau,$$

a being a constant which plays, in regard to the temperature, a part analogous to that of the mechanical equivalent in regard to the calorie.

This is, with one more degree of generality, the consequence which I deduced from Carnot's theorem, which is thus, I think, for the first time rationally demonstrated.

Ampère, in a remarkable note inserted (in 1833) in vol. lviii. of the *Annales de Chimie et de Physique*, had indicated some ideas analogous to those I have developed in my memoir.

In another paper I shall point out some of the most important consequences which may be deduced from my theory. I shall particularly dwell on those which have drawn upon the theory of the atomic volumes of bodies—a theory of great importance for chemistry and mineralogy, and the study of which was the starting-point of my researches in thermodynamics.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. pp. 1479–1482.

ON ELECTROMAGNETISM. BY M. TRÈVE.

It is known that steel needles can be magnetized by discharges from a Leyden jar passing through a wire running round them; consequently static electricity has in this case the same properties as dynamic currents. This property belongs also to Ruhmkorff's induction-coil when its currents are caused to pass in a *glass solenoid* containing a rarefied gas.

I placed in the axis of the solenoid some bars of steel, which a single discharge was sufficient to transform into magnets. This was a thing foreseen. But though this experiment establishes no new fact, it is perhaps of a kind to make comprehensible the phenomenon by means of which Ampère completed his theory of terrestrial magnetism. Indeed, suppose we make the glass solenoid big enough to envelop the earth, and cause currents from an induction-machine to pass through it, we shall magnetize every thing magnetic found there, and all needles will be deflected in proportions variable with the intensity of the current.

I think we have here a tolerably correct picture of the electrified gaseous currents which, according to Ampère, incessantly circulate round our globe from east to west, forming as it were an immense coil that acts on the variable horizontal and vertical currents.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. p. 1624.

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X. *On the Spectrum of the Bessemer-flame.* By W. MARSHALL WATTS, *D.Sc., Physical Science Master in the Giggleswick Grammar School*[*].

[With Two Plates.]

IN the Philosophical Magazine for December 1867 I published the results of observations on the spectrum of the Bessemer-flame, made with a one-prism spectroscope on the London and North-Western Company's Works at Crewe. These experiments showed that the Bessemer-spectrum contained, besides the lines of potassium, sodium, and lithium, certain lines due to iron; but most of the lines were not found to be coincident with the known lines of carbon or of any other element. Nevertheless I held strongly the opinion that the spectrum was mainly due to carbon, for the following reasons:—

(*a*) Carbon is known to give more than one spectrum (Phil. Mag. Oct. 1869, Chem. News, Oct. 1870); and though the Bessemer-spectrum does not coincide with any recognized spectrum of carbon, it is yet observed in the flame of burning coke, and in other cases where carbon would seem to be the essential element present.

(*b*) The spectrum disappears almost precisely at the right moment for stopping the blast, which is supposed to be when the carbon in the pig iron has been burnt out and the iron is in the condition of molten wrought iron.

I have now to give the results of further observations on the spectrum of the Bessemer-flame, which show that the lines are mainly due to *manganese oxide* (not, as has been stated, to *manganese*, no single line of whose spectrum coincides with a Bessemer line). These observations were made at the works of the Barrow Hæmatite Steel Company at Barrow; and I am glad to have this opportunity of expressing my sense of the liberality with which the Directors of the Company assisted a purely scientific investigation, not only by affording every facility for the carrying out of experiments, but even by contributing to the expense of the investigation.

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[*] Communicated by the Author.

The instrument employed was Browning's automatic spectroscope of six prisms. Most of the measurements were made by means of the micrometer tangent-screw of the instrument, which requires 2·94 turns to carry the cross-wires of the telescope from the lithium-*orange* line (wave-length 6101) to the least-refrangible D line (wave-length 5895). In some of the later measurements much more exact results were obtained by the use of a micrometer eyepiece furnished with two pairs of cross-wires. With this instrument the interval between the same two lines is represented by 12·49 turns of the micrometer-screw. The spectrum was mapped throughout on the scale of wave-lengths, the wave-lengths of the lines of the Bessemer-spectrum being obtained by interpolation from the wave-lengths of the known lines of some metal, whose spectrum was arranged so as to be visible together with the Bessemer-spectrum.

It was found, however, that (in consequence, probably, of the complicated motion of the prisms in the automatic arrangement) the observing-telescope did not always travel at exactly the same rate, so that the interval between two given lines was not always represented by the same reading. For example, in twelve successive measurements of the interval between the most refrangible D line and the least refrangible *b* line the following numbers were obtained:—

14·11, 13·71, 13·73, 13·68, 13·69, 13·77, 13·77,
13·74, 13·77, 13·78, 13·75, 13·78.

But although the rate of motion varies, yet in any one passage of the telescope (and prisms) from the red end to the blue end of the spectrum the motion is uniform, so that the wave-length of any line in the Bessemer-spectrum is correctly obtained by interpolation from the wave-lengths of two known lines between which it lies. Accordingly in each "blow" the spectrum was mapped steadily from the red end to the blue end, the tangent-screw being kept firmly clamped and the telescope never being allowed to return upon itself. The readings obtained in each blow were then reduced to wave-lengths by means of one interpolation-curve; and although the curves obtained vary slightly in degree of curvature, they yield concordant results.

The following determinations of the wave-lengths of two of the brightest lines will show about the degree of accuracy attainable:—

	Reference-spectrum.		Bessemer-spectrum.	
	Wave-length.	Reading.	Reading.	Deduced wave-length.
Air	5678	11·33		
	..	..	13·18	5580
Air	5534	14·09		
	..	..	17·73	5359
Copper	5292	19·24		
Copper	5217	21·00		
Lead	5607	9·77		
	..	..	10·28	5580
Lead	5372	14·55		
	..	..	14·84	5358
Air	5495	12·21		
Lead	5372	14·63		
	..	..	14·89	5360
Lead	5045	22·50		
Cadmium	5378	14·38		
	..	..	14·81	5358
Cadmium	5337	15·27		

The following measurements of the same two lines were made with the micrometer eyepiece:—

	Reference-spectrum.		Bessemer-spectrum.	
	Wave-length.	Reading.	Reading.	Deduced wave-length.
Tin	5563	0·10		
	..	..	1·55	5580
Tin	5588	2·20		
Cadmium	5338	0·10		
Thallium	5348	1·28		
	..	..	2·22	5359
Cadmium	5378	3·72		
Tin	5347	0·10		
	..	..	1·12	5359
Tin	5368	1·88		

The observatory was placed against the wall of one of the sheds, about on a level with the top of the convertors, close to two convertors, and commanding a distant view of two others. The distant convertors were found to be the best for careful measurement, the shaking being less than when the blow was taking place at one of the near convertors, although the spectrum from these was, of course, the most intense. The best method of introducing the reference-spectrum was found to be, to throw an image of the Bessemer-flame upon the slit by means of a large lens of about 10 inches focus, and to bring the spark-discharger (or Bunsen burner) between the lens and slit. A screen was arranged so as to cut off the light of the Bessemer-flame when required, so that either of the two spectra could be obtained alone at pleasure, or the one could be superposed on the other.

The metals employed to furnish the reference-lines were the following, besides which the lines of the air-spectrum were made use of:—aluminium, copper, cadmium, iron, lithium, lead, magnesium, manganese, platinum, sodium, thallium, tin, and zinc. Further, the Bessemer-spectrum was carefully *compared* with various spectra, especially with those of iron, sodium, lithium, manganese, and manganese oxide. The spectra were either arranged under the Bessemer-spectrum by the use of the reflecting prism, or were superposed on it in the manner described above. The spectra of iron and manganese were obtained by taking the electric spark between wires of these metals—the spectra of sodium, lithium, and manganese oxide by the use of a Bunsen-burner, or by heating the substance in the flame of the oxyhydrogen blowpipe. When manganese chloride (or manganese carbonate or pyrolusite) is heated in the oxyhydrogen flame a very brilliant spectrum is obtained, which, as will be seen afterwards, is for the most part coincident with the Bessemer-spectrum. Observations were, further, made on the spectrum of the flame obtained on adding the Spiegeleisen, on the temperature of the flame at different stages in the process, and on the differences in the spectrum caused by the employment of different iron. The results obtained will be detailed in succession.

I. *Measurement of the Wave-lengths of the Lines in the ordinary Bessemer-spectrum, in the Spiegel-spectrum, and in the spectrum of Manganese Oxide* (Plate IV.).

	Bessemer.	Spiegel.	Manganese oxide.	Remarks.
	6560 ?	6560	......	Two red lines, not always seen, estimated position only.
	6460 ?	6460	......	
α	6234	6234	6234	Fine line.
	6218	......	......	Fine line; occurs only in Bessemer.
	6204	6204	6204	Most refrangible edge of band, shading away (like all the bands in the Bessemer-spectrum) towards the red. Appears in the Spiegel as a bright line, and in the manganese-oxide spectrum as the *least*-refracted edge of a very narrow band. See figure (Pl. IV.).
	6185	......	6185	Fine line.
	6178	6178	6178	Edge of band. The remark to 6204 applies also to this.
	6161	6161	......	Fine line, faint.
β	6109	6109	......	Conspicuous pair of red lines, absent in manganese-oxide spectrum.
	6097	6097	......	
			6080 ?	Four faint lines, too strong in drawing; position estimated only.
			6060	
	6040	6040	......	Faint line.
	6012	6012	......	Pair of faint lines.
	6006	6006	......	
γ	5972	5972	....	Strong line, absent in the manganese-oxide spectrum.
	......	5946	......	Four equidistant lines, not very bright, of which the most refrangible read 5946.
	......	5932	5932	Line. In manganese-oxide spectrum only, and sometimes in Spiegel.
	5917	......	......	
	5909	......	5909	In Bessemer-spectrum as a faint line; in the manganese-oxide spectrum as the edge of a band.
	5895	5895	......	Sodium lines.
	5889	5889	......	
	5872	5872	......	Two faint lines, absent in the manganese-oxide spectrum.
	5865	5865	......	
δ	5847	5847	5847	Maximum of light.
	5819	5819	......	Brightest edge of band, fading away towards the red.
	5807	5807	......	Fine line.
	5790	5790	......	Strong fine line, forming the brightest edge of the whole group.
ε	5705	......	......	Fine line.
	5688	5688	5688	Splendid double line, conspicuous among the fine lines which make up the whole of this group, nearly coincident with $\mathrm{Na}\,\beta\begin{cases}5687\\5681\cdot\end{cases}$
	5683	5683	5683	
	5644	5644	5644	Brightest edge of band, shading off into red.
	5607	5607	5607	Brightest edge of band.
	5580	5580	5580	Edge of band, hazy in the manganese-oxide spectrum, but sharply defined in the Bessemer and Spiegel.

Table (*continued*).

Bessemer.	Spiegel.	Manganese oxide.	Remarks.
5547	5547	......	Group of three lines, the middle one the strongest. In Bessemer and Spiegel only.
ζ 5532	5532	......	
5529	5529	......	
5462	5462	......	Three faint lines, absent in the manganese-oxide spectrum.
5454	5454	......	
5443	5443	......	
5433	5433	5433	Fine line.
5423	5423	5423	Brightest edge of band, fading off towards red.
η 5405	......	......	Line.
5395	5395	5395	Strong line.
5391	5391	5391	Edge of band.
5371	5371	......	Strong line, absent in the manganese-oxide spectrum.
5359	5359	5359	Bright edge of this group, which fades away towards the red.
5327	5327	......	Strong line.
5269	5269	......	Strong line, coincident with the solar line E.
5229	5229	5229	Edge of band.
θ 5192	5192	5192	Edge of band.
5167	5167	......	Line, absent in the manganese-oxide spectrum.
5157	5157	5157	Edge of band.
5107	5107	......	Line, absent in the manganese-oxide spectrum.
5099	5099	5099	Edge of band.
ι 5052	5052	5052	Edge of band.
5018	5018	5018	Edge of band.
4984	4984	4984	Edge of band.
......	4943	4943	Edge of band.
......	4904	4904	Edge of band.
......	4862	4862	Edge of band.
......	4836	4836	Edge of band.
......	4802	4802	Line.
......	4783	4783	Line, forming edge of band.
4481	4481	......	Line.
4432	4432	......	Line.
κ 4404	4404	......	Line.
4383	4383	......	Line.
4373	4373	......	Line.

Of the Bessemer lines which do not occur in the manganese-oxide spectrum, the following are due to iron:—

5371, 5327, 5269, 5192, 5107, 4383.

The line at 5167 is also nearly coincident with the double iron line $\left\{\begin{matrix}5167\\5168\end{matrix}\right.$; and the edge of the band at 5229 coincides nearly with the double line $\left\{\begin{matrix}5226\\5232\end{matrix}\right.$.

Fig. 1, Plate V. shows these coincidences. Those lines only of the iron spectrum are drawn which were actually mapped together with the Bessemer-spectrum, when the two were seen simultaneously.

The same figure shows also the result of a direct comparison of the Bessemer-spectrum with that of manganese oxide: those lines only of the manganese oxide spectrum are drawn which were seen to coincide with the Bessemer (or Spiegel) lines.

It is worthy of remark that the identification of these iron lines proves that iron may exist as vapour at a temperature below the melting-point of iron, since the Bessemer-flame is not hot enough to melt wrought iron. The presence of a few only out of the 180 lines which constitute the spectrum of the electric spark between iron poles, is only the same thing that we observe in the case of sodium, lithium, and thallium when the spectrum is produced at a low temperature.

The following lines in the Bessemer-spectrum remain unidentified:—

	6560	Red lines not always seen.
	6460	
	6218	Fine lines.
	6161	
β	6109	Strong red lines.
	6097	
	6040	Faint line.
	6012	Faint lines.
	6006	
γ	5972	Strong red line.
	5917	Faint line
	5872	Faint yellow lines.
	5865	
δ	5819	Bright light-green group.
	5807	
	5790	
ζ	5547	
	5532	
	5529	
	5462	Three faint lines.
	5454	
	5443	
	5405	Faint line.
	5167	Fine blue line.
	4481	Blue lines.
	4432	
	4404	
	4383	

Two other blue lines, whose position could not be determined, were also seen occasionally.

II. *On the Temperature of the Flame, and the differences observed at different times.*

It is in the flame obtained on adding the highly manganiferous Spiegeleisen that the manganese-oxide spectrum is most fully developed. In the experiments at Crewe a marked difference was almost always observed between the ordinary Bessemer-spectrum and that of the Spiegel, arising from a difference in the relative intensity of the different lines. The difference was so marked that it was not at first perceived that the two spectra were in any way the same; but at Barrow this difference does not exist, the Spiegel-spectrum being identical with the Bessemer, only more intense. The ordinary Bessemer-spectrum at Barrow is in fact identical with the Spiegel-spectrum as obtained at Crewe.

A reference to fig. 2, Plate V. (which represents the two spectra on the scale of a one-prism spectroscope), will render this difference clearer.

It will be observed that in the Barrow spectrum there is a more marked division of the separate lines into groups having a general resemblance, the group η (65 to 67) being the brightest; while in the Crewe spectrum the brightest portion is nearer the red (58 and 59), and the spectrum does not extend so far into the blue. This difference is probably due simply to difference of temperature; but it can be distinctly connected with the kind of iron employed: the steel at Crewe was intended for the axles and tires of wheels; that made at Barrow was to be employed for rails. The same difference was remarked in observing two "blows" at the Bolton Iron and Steel Works. The first was of iron of No. 1 brands, and the second of inferior iron. The spectrum in the first case was the same as the Crewe one, but the Spiegel-spectrum was the same as that observed at Barrow; in the second "blow" the spectrum was nearly that of Barrow.

An attempt was made to determine the temperature of the flame by observing whether wires of gold, platinum, and of an alloy of 90 per cent. platinum and 10 per cent. iridium were melted when held in the flame. It was found that towards the middle and end of the blow the gold was always melted; but on no occasion was either the alloy or the platinum melted, even when the wire was kept in the flame for several minutes. At the beginning of the blow, however, gold does not melt. A wire held in the flame from the commencement was not melted when the sodium-line appeared flashing across the continuous spectrum, but did melt about the time that the sodium-line became constant, and before any of the Bessemer lines proper made their appearance. If we take the melting-point of gold to be 1300° C.

and of platinum 2000° C., it follows from these observations that the temperature of the flame at the commencement of the blow is below 1300°, but gradually rises, never, however, reaching 2000° C.

This result is confirmed by the fact, that of the sodium-spectrum only the double line D is present. In the Philosophical Magazine for August 1870, I showed that the sodium-lines $\left\{\begin{matrix}5681\\5687\end{matrix}\right.$ may be employed as an index of temperature, since they are present in the spectrum of any flame containing sodium the temperature of which is high enough to melt platinum, but do not appear at lower temperatures. The Bessemer-flame obviously contains abundance of sodium; but this sodium double line is absent. The lithium orange line also, which comes out at a somewhat less temperature, is absent.

The Bessemer-spectrum is seen more or less distinctly in several other flames. The jet of flame which issues with the Spiegel from the Spiegel cupola shows it brilliantly. This flame also melts gold but not platinum. The Bessemer-spectrum is seen also in the flame from the melting-cupola for the pig iron, in the flame from the bottom of the blast-furnaces at work at Barrow, and in the flame of the coke used in warming the cupola after re-lining (which spectrum exhibits also the lines of sodium, lithium, and potassium brilliantly), and in several other flames. The flame obtained on adding the Spiegel, which gives the Bessemer-spectrum most brilliantly, is also incapable of melting platinum, but melts gold.

There are certain of the lines which seem to linger after the rest, and which, when the blow has been carried rather far, are occasionally seen alone (upon a continuous spectrum) after the regular Bessemer-spectrum has disappeared. Some, but not all of these, are iron lines. The following are those which have been noticed:—

5107, 5167, 5269 Fe, 5327 Fe, 5359, 5370 Fe, 5395,
5433, 5443, 5453, 5462.

The manganese present in the pig iron used at Barrow never exceeds 0·6 per cent. I am indebted to Mr. Richards, chemist to the Company, for the following numbers, representing the mean composition of the pig iron and spiegel iron used:—

		Pig iron.	Spiegeleisen.
Carbon	Graphite	3·01	0·25
	Combined	0·33	4·01
	Silicon	3·01	0·49
	Sulphur	0·06	0·04
	Phosphorus	0·02	0·25
	Manganese	0·42	10·15
	Copper	..	trace
	Iron	93·15	84·81
		100·00	100·00

It is very difficult to understand why these lines, which are not those of carbon, should disappear at the exact point at which the blast ought to be stopped, and that they should be due to a substance of which so little can be present. The following analyses of the metal at different stages in the process, quoted from Mr. Snelus's paper read at the Meeting of the Iron and Steel Institute in December 1870, show that in some cases the quantity of manganese present is inappreciable:—

		Pig used.	6 minutes after starting.	9 minutes after starting.	Before adding Spiegel, 13 minutes after starting.
Carbon.	Graphite	2·07			
	Combined	1·20	2·17	1·55	0·097
	Silicon	1·952	0·795	0·635	0·020
	Sulphur	0·014	trace	trace	trace
	Phosphorus	0·048	0·051	0·064	0·067
	Manganese	0·086	trace	trace	trace

XI. *On the Experimental Determination of the Relative Intensities of Sounds; and on the Measurement of the Powers of various substances to Reflect and to Transmit Sonorous Vibrations. By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U. S. A.*[*]

WHILE the problems of the determination of the pitch of sound and the explanation of timbre have received their complete elucidation at the hands of Messenne, Young, De la Tour, König, and Helmholtz, the problem of the accurate experimental determination of the relative intensities of given sonorous vibrations has never been solved.

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[*] Communicated by the Author, having been read before the National Academy of Sciences at Cambridge, Massachusetts, November 21, 1872.

The method I here present will, I hope, open the way to the complete solution of this difficult and important problem; and I trust that the success I have met with will encourage others more learned and patient to attack with superior acumen a subject which must necessarily become of fundamental importance in the future progress of acoustic research.

1. *The determination of the Relative Intensities of Sounds of the same Pitch.*

If two sonorous impulses meet in traversing an elastic medium, and at their place of meeting the molecules of the medium remain at rest, it is evident that at this place of quiescence the two impulses must have opposite phases of vibration and be of equal intensity.

I have in the following manner experimentally applied this principle to the accurate determination of the relative intensities of vibrations giving the same note and propagated from their sources of origin in spherical waves.

Clothe two contiguous rooms with a material which does not reflect sound, and place in each room one of the sounding bodies, and maintain these sounds of a constant intensity; or the two sources of sound may be placed in the open air and separated from each other by a non-reflecting partition. Fix at a certain distance from each sounding body a resonator responding to its note; attach to each resonator the same length of firm gum tubing, and lead these tubes to a forked pipe so that the impulses from the two resonators meet at the confluence of the two branches of the forked tube, and connect the branch of the forked tube, in which the sounds meet, with one of König's manometric capsules. Now sound continuously one of the bodies, and the manometric flame, when viewed in a revolving mirror, will present its well-known serrated appearance. On sounding the second body, impulses from it will meet those from the first body; and if the phases of vibration of the impulses on the manometric membrane are opposed and of equal intensities, the membrane will remain at rest, and the flame will now appear in the mirror as a band of light with a rectilinear upper border. But although the intensities of the pulses can easily be rendered equal by altering the distance of one of the resonators from its sounding body, yet this change of position will alter the relation of the phases reaching the membrane—so that if by mere chance we get them opposed in the first position of the resonator, they will no longer be so after its change of position. But on stopping the vibrations of one of the bodies, and setting it in vibration at intervals, we may finally succeed in causing the impulses on reaching the membrane to have opposite phases of vibration.

Such a method, which relies only on chance, can be of little value, on account of its uncertainty and the tediousness of its application.

The above difficulty I have entirely removed by the following means. I cut a piece out of one of the tubes equal in length to a half-wave of the note we are experimenting on, and replace this piece of tubing with a glass tube of the same length, into which slides, air-tight, another glass tube, also half a wave in length. The experimentation now becomes expeditious and certain. Sound both bodies continuously, and place in a fixed position one of the resonators. Move the other to a certain distance from its sounding body, and then pull out the inner glass tube until exact opposition of phase of the impulses is brought to the manometric membrane. This condition will be known when the serrations have dropped to their minimum of elevation. If the latter do not entirely disappear from the band of light in the mirror, we must place the moveable resonator at another distance and readjust the sliding tube. A few trials will give in the mirror a band of light with a straight, unruffled top border; then we have opposed phases of vibration at the confluence of the branches of the forked tube, and pulses of equal intensity are traversing the two tubes leading from the resonators.

The distance of each resonator from its sounding body is now measured; and the inverse ratio of the squares of these distances will be the ratio of the intensities of the vibrations at the sources of the sounds, if the intensities of the pulses sent through a tube from a resonator varies directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator.

It will be observed that the accuracy of the determinations by this experimental method depends on three conditions:—First, that the vibration-effects of the same area of a spherical sonorous wave diminish in intensity as the reciprocals of the squares of the distances of this area from the point of origin of the wave. There is every dynamic reason to believe in the truth of this proposition. The second necessary condition is, that the elongation of one of the resonator-tubes beyond the other by a half wave-length of firm glass tubing does not diminish the intensity of the impulses which have traversed it. Numerous experiments, especially those of Biot and Regnault on the aqueduct-tubes of Paris, show that so short a connecting-tube of glass cannot in any way affect the accuracy of the measures. The third condition is, that the intensities of pulses sent through a tube from a resonator vary directly with the intensities of the vibrations of the free air in the plane of the mouth of the resonator. This is a very important consideration; and as I believe there is no entirely reliable discussion of this relation, the problem will have to be experimentally solved with the greatest care. If, however, the relation between the intensities of pulses inside the tube and those outside the mouth of the resonator shall be shown to be different (and I think they will be) from what we, for illustration, have here assumed, only the process of the numerical reduction of the experiments will be modified, while the experimental method remains secure. Indeed I cannot but consider that I have here, by applying the principle of interference, so fertile in results in optics, been the first to give an experimental method which will determine with precision the relative intensities of two sonorous vibrations producing the same note.

Savart and many other experimenters have determined the relative intensities of two sounds by placing sand or other light particles on membranes and receding from the source of sound until no motions of the particles were visible. Also Drs. Renz and Wolf (Pogg. *Ann.* vol. xcviii. p. 595) give the results of experiments on the determination with the ear of the intensity of the sounds of a ticking watch. More recently Dr. Heller (Pogg. *Ann.* vol. cxli. p. 566) has made an elaborate research on the intensities of sounds, deducing mathematically his determinations from the observed amplitudes of vibration of a membrane; and Mr. Bosanquet (Phil. Mag. Nov. 1872) has just published a paper in which he proposes, for the measure of the intensities of sounds of pipes of different pitch, the determination of the quantity of air which each pipe consumes in sounding. But all these experimenters acknowledge the want of precision in their measures, and the difficulties in the actual practice of their methods.

When the resonators have such distances from their corresponding sounding bodies that the phases of their impulses on the membrane are opposed while their intensities are different, a residual action is given; and the intensity of this action on the membrane will depend on the relative intensities of the sounding bodies and the relative distances at which the resonators are placed. It may here be interesting to consider the simplest case,—that is, when the intensities of vibration at the two sources of origin of the sounds are the same and the two resonators are placed at various distances from these points of origin, but always differ in their distances by one half wave-length. Let us call A one of the resonators, B the other. Let A be successively placed at distances from its sounding body equal to 1, 2, 3, &c. wave-lengths, and B successively at distances equal to 1½, 2½, 3½, &c. wave-lengths. At each position of the resonators we will suppose that the phases of vibration reaching the membrane are opposed. The following Table gives the calculations made on the assumption that the intensities of the vibrations diminish as the reciprocals of the squares of their distances from the sounding bodies:—

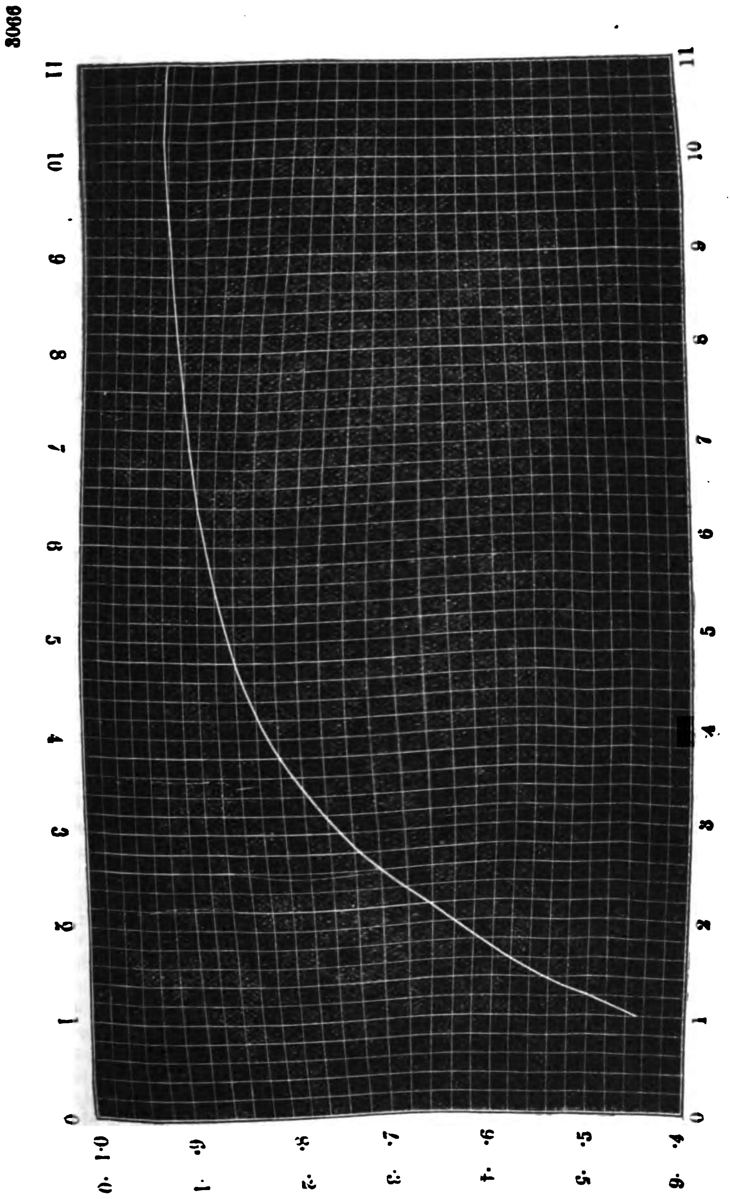
A's dist. in λ.	B's dist. in λ.	Ratios of intensities.	Residual effects.
1	1·5	·444	·556
2	2·5	·640	·360
3	3·5	·734	·266
4	4·5	·790	·210
5	5·5	·826	·174
6	6·5	·854	·146
7	7·5	·871	·129
8	8·5	·885	·115
9	9·5	·897	·103
10	10·5	·907	·093
11	11·5	·914	·086
12	12·5	·921	·079
13	13·5	·927	·073
* * *	* * *	* * *	* * *
24	24·5	·959	·041
25	25·5	·961	·039

We have projected these related numbers in the accompanying curve, whose abscissæ represent the distances of A from the source of sound, and whose ordinates give the ratios of intensities between A, taken at the distances on the axis of abscissæ, and B at distances from the sounding body always one half wave-length greater than A's distance from its sounding body. The formula of the curve is

$$y=\frac{x^2}{(x+\frac{1}{2})^2}.$$

If the curve be placed upside down and referred to the corresponding numbers on the abscissæ and ordinates (which numbers are equal to unity minus the numbers at the corresponding points of the curve when in its first position), we have the graphical representation of the variation of the resultant intensities contained in the fourth column of the Table.

In the case of notes of different pitch, the higher note will necessarily force the air to make its vibrations with a greater velocity; the intensities will therefore not alone depend on the amplitudes of these vibrations, but also on their velocities; and it has been deduced from established principles of dynamics that the apparent intensities of notes of different pitch will vary directly as the squares of the amplitudes, and inversely as the fourth power of the wave-length or periodic time (see Mr. Bosanquet "On the Relation between the Energy and Apparent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·4
3066

Intensity of Sounds of different Pitch," Phil. Mag. Nov. 1872). Hence the determination of the relative intensities of notes of different pitch becomes very complicated, and the experimental solution of the problem is encompassed with many difficulties. I, however, hope to be able at some future day to present some work in this direction, when I have succeeded in obtaining results worthy of the appellation of measures of precision.

2. *Measurement of the powers of various substances to Transmit and to Reflect sonorous vibrations**.

After we have succeeded in obtaining a measure of the intensity of the vibrations of the air at a certain distance from the

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* In the Smithsonian Report for 1857 will be found an account of very interesting and valuable experiments by Professor Joseph Henry, bearing on "Acoustics applied to Public Buildings." In these investigations Professor Henry determined the *relative* powers of various substances to reflect, transmit, and absorb sonorous vibrations by placing on the bodies the foot of a tuning-fork, and comparing the duration of its sound when thus placed with that given when the fork was suspended in free air by a fine cambric thread. Thus suspended the fork vibrated during 252 seconds. Placed on a large thin pine board its vibrations lasted about ten seconds. In this case "the shortness of duration was compensated by the greater intensity of effect produced." The fork having been placed successively on a marble slab, a solid brick wall, and on a wall of lath and plaster, its vibrations lasted respectively 115 seconds, 88 seconds, and 18 seconds.

Placed on a cube of india-rubber, the sound emitted by the fork was scarcely greater than when it was suspended from the cambric thread, but its *duration* was only 40 seconds. Here Henry puts the question, "What became of the impulses lost by the tuning-fork? They were neither transmitted through the india-rubber nor given off to the air in the form of sound, but were probably expended in producing a change in the matter of the india-rubber, or were converted into heat, or both. Though the inquiry did not fall strictly within the line of this series of investigations, yet it was of so interesting a character in a physical point of view to determine whether heat was actually produced, that the following experiment was made. . . . . The point of a compound wire formed of copper and iron was thrust into the substance of the rubber, while the other ends of the wire were connected with a delicate galvanometer. The needle was suffered to come to rest, the tuning-fork was then vibrated, and its impulses transmitted to the rubber. A very perceptible increase of temperature was the result; the needle moved through an arc of from one to two and a half degrees. The experiment was varied and many times repeated; the motions of the needle were always in the same direction, namely in that which was produced when the point of the compound wire was heated by momentary contact with the fingers." We have pleasure in again calling attention to this beautiful experiment of Professor Henry; for he was, I believe, the first to obtain the production of heat by the absorption (so to speak) of sonorous vibrations; and although several experimenters have subsequently obtained the same results, they seem to be unaware of Henry's antecedent work in the same direction. In 1868 I published a full account of the above experiment in my 'Lecture-Notes on Physics,' p. 79 (Van Nostrand, New York).

In the same paper Professor Henry obtained a few qualitative relations on the reflecting-powers of various substances, by placing a watch between sounding body, we can measure the powers of various substances to transmit and to reflect sonorous vibrations.

To accomplish this I place one of the sounding bodies in the focus of a parabolic reflector, and bring the two resonators to such distances from their sounding bodies that the intensities of the pulses traversing their respective tubes are equal. We then place in front of, but not too near, the mouth of the resonator in front of the reflector the plane surface of the substance whose transmitting and reflecting powers we would determine. Serrations now appear in the flame, because part of the force of the pulses which previously sounded the resonator are now reflected. The resonator which has not the reflecting surface in front of it is now gradually drawn away from its sounding body; and at each successive point of remove the pulses propagated through the resonator-tubes are brought to opposition of phase on reaching the membrane by means of the glass telescoping tube. Equality of impulses having been obtained, we measure the distance of the resonator from its sounding body; and this measure, together with the previously known distance of this resonator when equality was attained before the interposition of the reflecting surface, gives the data for the computation of the intensity of the *transmitted* vibrations. This number, subtracted from the measure of the intensity when the substance was not before the resonator, taken as unity, gives the *reflecting-power* of the substance.

It is very important in such measures to be sure that a plane-wave surface is reflected from the mirror. This character of wave can be approximately obtained by placing the mouth of a closed organ-pipe at or very near the principal focus of the mirror, and testing, by the method we have described above, the equality of intensity of the vibrating air in front of the mirror as we recede along its axis. We thus by trial at last succeed in obtaining a sufficiently plane-wave surface. Care must also be taken that the surface of the reflecting substance we experiment on is so large that no inflected vibrations can act on the resonator.

I have made several measures of Intensity and of Transmitting and Reflecting powers; but as the experiments were made in a room whose walls, ceiling, and floor gave reflected sonorous waves, I will not present measures until I have arranged suitable apartments for their accurate determination.

November 13, 1872.

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the centre and focus of a concave mirror; he then receded along the axis of the diverging sonorous beam with a hearing-trumpet. Paper and flannel were now stretched between the watch and the mirror; and the intensity of the sound was found to be diminished by the reflecting and absorbing powers of these substances.

XII. *Note on the History of certain Formulæ in Spherical Trigonometry.* By I. TODHUNTER, *M.A., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

THERE are four formulæ in Spherical Trigonometry which are usually called *Gauss's Theorems* or *Gauss's Analogies*. These formulæ are

$$\sin \tfrac{1}{2} a \cos \tfrac{1}{2}(B-C) = \sin \tfrac{1}{2}A \sin \tfrac{1}{2}(b+c)$$

and three others of a like nature.

The formulæ, however, are really due to Delambre; but in consequence of an erroneous reference, his claim has been obscured, and mathematicians have been put to inconvenience in investigating the matter.

Gauss printed the formulæ in 1809 in his *Theoria Motus*, p. 51. He says they would be sought for in vain in books on trigonometry; he omits the demonstration for the sake of brevity.

Delambre, in the first volume of his *Astronomie*, published in 1814, claims the formulæ (see his pages 164 and 195); in both places he refers to the *Connaissance des Tems* for 1808. The reference, however, should be to the *Connaissance des Tems* for 1809, which was published in April 1807. Here the four formulæ are given without demonstration, together with some others which follow immediately from them. One of these other formulæ is

$$\tan \tfrac{1}{2} a = \frac{\tan \tfrac{1}{2}(b-c) \sin \tfrac{1}{2}(B+C)}{\sin \tfrac{1}{2}(B-C)};$$

this Delambre ascribes to M. Henri.

As Delambre is here discussing the solution of an astronomical problem given by M. Henri, it might at first sight have appeared probable that *after* this formula had been used by M. Henri, the four formulæ improperly ascribed to Gauss were investigated by Delambre. But it may be inferred from the top of page 446 of Delambre's remarks, that he had been previously acquainted with the formulæ.

Delambre claims the formulæ and gives the correct reference on p. 349 of the *Connaissance des Tems* for 1812, which was published in July 1810: Delambre is here reviewing Gauss's *Theoria Motus*.

Delambre's erroneous reference has been adopted by some writers. Thus Bowditch says on p. 737 of the first volume of his translation of the *Mécanique Céleste*, "Delambre, in his *Astronomie*, vol. i. p. 164, observes that he had given several of these theorems in the *Connoisance des Tems*, 1808, before the publication of the work of Gauss. . . ." T. S. Davies says, on p. 37 of the second volume of the twelfth edition of Hutton's 'Course of Mathematics,' "The four formulæ . . . are usually known as *Gauss's Analogies*, their demonstration having been first given by that illustrious geometer in his *Theoria Motus Corporum Cœlestium* (1809): but they had been published by Delambre some years previously in the *Connaissance des Temps* (for 1808) . . ." Here, besides the expansion of an exact *two* years into an indefinite *some* years, we have the statement that Gauss gave a *demonstration* of the formulæ in 1809; but as we have already stated, Gauss omitted the demonstration. In a note Mr. Davies adds: "Gauss did not deliver his theorems, or their investigations, in precisely the forms given in the text . . ." But Gauss *did* deliver his theorems in those forms. Then what Mr. Davies goes on to say respecting the forms and investigations may perhaps apply to some other work, but does not apply to the *Theoria Motus*, where Gauss delivered the theorems.

It must be observed that Gauss had been anticipated even in Germany in the publication of the formulæ. They were given by Mollweide in Zach's *Monatliche Correspondenz* for November 1808, with a demonstration.

The subject is noticed in an article in Klügel's *Mathematisches Wörterbuch*, vol. v. p. 211; the passage has been reproduced in the 'Proceedings of the London Mathematical Society,' vol. iii. p. 320. The writer states correctly the positions of Gauss and Mollweide; and then he adds that Delambre published the formulæ in the *Connaissance des Tems* for 1808, and so French writers usually call them after him. But these few words relating to Delambre seem to me to fall below the usual high standard of German accuracy. For in the first place the erroneous date (1808) must have been borrowed without verification, although there is nothing to warn us of this. And in the next place the writer apparently puts the claims of Mollweide and Delambre as equal, by ascribing to both the date 1808, overlooking the fact that the *Connaissance des Tems* for an assigned year is published in advance of that year.

Thus, finally, although Mollweide has priority over Gauss, yet he comes about a year and a half after Delambre; and therefore until any other person can be shown to have published the formulæ before April 1807, they must be justly ascribed to Delambre.

Demonstrations of the formulæ in two ways were published by Delambre in his *Astronomie* (see pp. 164 and 196 of his first volume). It would appear from his page 164 that he considered this to be the first publication of a demonstration; but, as we have stated, Mollweide gave a demonstration in 1808. In his second way, Delambre makes use of a diagram from which he obtains both his own Analogies and those of Napier. This way of demonstration is substantially the same as that which was independently discovered and printed in the 'Proceedings of the London Mathematical Society,' vol. iii. p. 13. One step in the recent process, however, is simpler than the corresponding step in Delambre's, namely the proof of the equality of the angles M V A and C V P.

It may be remarked that if one of Napier's Analogies is given, we may deduce another immediately by using one of the triangles *associated* with the fundamental triangle; and then Napier's two other Analogies follow by the aid of the polar triangle. Thus we may say that the other three may be deduced immediately from any one of them. But with respect to Delambre's Analogies, the case is rather different. Take these in the order in which they are given in my 'Spherical Trigonometry.' Then (1) and (4) are so related that either can be deduced from the other by using an associated triangle; but nothing new is obtained from (1) or from (4) by using the polar triangle. And (2) and (3) are so related that either can be deduced from the other by using an associated triangle, *or* by using the polar triangle. Thus from one of Delambre's Analogies we cannot deduce immediately the other three. If one of Napier's Analogies is given and one of Delambre's, we can deduce immediately the other six. Also the other six may be deduced immediately from (1) and (2) of Delambre's Analogies; and the other six may be deduced immediately from (3) and (4) of Delambre's Analogies.

I. TODHUNTER.

Bourne House, Cambridge.

XIII. *On the Law of Gaseous Pressure. By* ROBERT MOON *M.A., Honorary Fellow of Queen's College, Cambridge*[*].

I DESIRE to offer some remarks upon Mr. Strutt's further criticism[†] of my views as to gaseous pressure, for which I have not had opportunity hitherto.

I fail to find in Mr. Strutt's second paper any reply to my inquiry why we are to reject the formulæ

$$p = -\frac{a^2}{\rho} + \phi\left(v + \frac{a}{\rho}\right), \quad . \quad . \quad . \quad . \quad . \quad (1)$$

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[*] Communicated by the Author.

[†] See Phil. Mag. for September last.

$$v+\frac{\alpha}{\rho}=\psi_1\left\{x-\frac{\phi'(u)-\alpha}{\rho}.t\right\},$$

$$\frac{1}{\rho}+\int\frac{du}{\phi'(u)-2\alpha}=\psi_2\left\{x-\frac{\alpha}{D}.t\right\}$$

(where $u=v+\frac{\alpha}{\rho}$), which, as Mr. Strutt appears to be fully conscious, satisfy the equation of motion

$$0=\frac{d^2y}{dt^2}+\frac{1}{D}\frac{dp}{dx}\text{*}. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In his first paper (see Phil. Mag. for July last) Mr. Strutt appeared to regard Boyle's law as experimentally established in all cases of rest or motion—a circumstance which, if it were true, would be decisive as to any value which the above formulæ may possess in physics. Inasmuch, however, as after my pointing out that the proof of Boyle's law was limited to the case of equilibrium, Mr. Strutt does not repeat his statement, I might conclude that he had altered his opinion, but for the occurrence of certain expressions of a contrary tendency.

I desire to know definitely, therefore, first, whether Mr. Strutt still considers that Boyle's law has been experimentally proved in the case of motion, and, secondly, what are the experiments upon which he rests this conclusion.

From not having bestowed sufficient consideration on the formulæ, Mr. Strutt has completely misapprehended their significance.

For, suppose that, when $t=0$, ρ and v have respectively the definite values $f_1(x)$, $f_2(x)$, then (1) gives us

$$p=-\frac{\alpha^2}{f_1(x)}+\phi\left\{f_2(x)+\frac{\alpha}{f_1(x)}\right\}; \quad . \quad . \quad . \quad (3)$$

but this does not determine the law of pressure which prevails when $t=0$, for the obvious reason that ϕ is arbitrary. So far is it from being the fact that a knowledge of the law of density and the law of velocity prevailing in the fluid at a given time

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* Mr. Strutt, after writing the equation

$$\frac{dv}{dt}=\frac{1}{D}\frac{dp}{dx},$$

properly remarks that another equation requires to be added, viz.

$$\frac{d}{dt}\left(\frac{D}{\rho}\right)=\frac{dv}{dx}.$$

This last, however, is included in the single equation of the text, in virtue of the analytical condition, $\frac{d}{dt}\left(\frac{dy}{dx}\right)=\frac{d}{dx}\left(\frac{dy}{dt}\right)$, and of the fact that $\frac{dy}{dx}=\frac{D}{\rho}$.

enables us to determine the law of pressure in the fluid at that time, that the law of pressure at the time in question may be any whatever. In fact, if when $t=0$ we have $p=f_3(x)$, where f_3 denotes any continuous function, we shall have

$$f_3(x)=-\frac{a^2}{f_1(x)}+\phi\left\{f_2(x)+\frac{a}{f_1(x)}\right\}, \quad . \quad . \quad (4)$$

an equation which can be satisfied by means of ϕ, and of which indeed it is the special and exclusive office to determine ϕ. This can be done as follows:—

Putting
$$f_2(x)+\frac{a}{f_1(x)}=\omega,$$

and solving with respect to x, we get

$$x=\text{funct. }\omega=f_a(\omega)\text{ suppose.}$$

Hence (4) becomes

$$f_3\{f_a(\omega)\}=-\frac{a^2}{f_1\{f_a(\omega)\}}+\phi(\omega),$$

which determines the form of ϕ.

It thus appears that the office performed by (1) is of this kind; viz. the law of pressure prevailing at a certain time, and also the laws of velocity and density prevailing at the same time being given, (1) enables us from those data to determine the law of pressure prevailing in the same case of motion at any other time. Obviously, therefore, it is as unreasonable for Mr. Strutt to ask me to state "the real physical law of pressure true at all times and places," as it would be for me to require him to determine in any particular case of motion the velocity and density at any instant by means of Euler's equations, without affording him any information as to the circumstances of the initial motion.

Of the particular cases of failure which I have adduced against the received law of gaseous pressure, the first which Mr. Strutt considers is that of a closed cylinder filled with air, which at the time t is destitute of velocity, but in which the density to the right of a certain plane is uniformly equal to 2D, while that to the left of the plane is D. I contend that if the received law of pressure held under these circumstances, it would contradict the principle that action and reaction are equal and opposite.

Mr. Strutt meets this by suggesting that "an infinitely small . . . . layer of air situated at the boundary is subject to an infinite acceleration," and that the fact "that the pressures which act on its two faces are unequal is therefore not in contradiction to any true principle."

Now by "infinitely small" Mr. Strutt must mean here "indefinitely small," whereas all the circumstances which I have supposed to exist at the time *t* are precise and definite. That any thing indefinite should arise out of that state of circumstances is simply impossible. In the case supposed, acceleration may, and indeed will occur at the time *t*; but the breadth, great or small, over which it prevails must be a *definite* breadth; and whether great or small, if it have magnitude, if it have existence, throughout that breadth Boyle's law does not hold. Mr. Strutt's apology for the law, therefore, in this case of its manifest failure, is, in effect, simply an admission of its failure, coupled with an wholly unfounded assertion that the failure will be confined within extremely narrow limits.

The next case treated of by Mr. Strutt is where a vertical cylinder closed at its lower end has an air-tight piston, capable of working freely in the upper part of it, which is exactly supported by the air beneath. I contend that if the received law of pressure were true, the placing of an additional weight at the time *t* upon the piston under the above circumstances would not destroy the equilibrium; for at the time *t*, when the weight is upon the piston, the density is unaltered; therefore, according to the received law the pressure of the air upon the piston will be unaltered, and the pressure of the piston upon the air (which must be exactly equal to the latter) will also be unaltered; *i. e.* the introduction of the additional weight leaves the actions between the different parts of the system precisely what they were during equilibrium*.

Mr. Strutt, on the other hand, maintains that "precisely the same argument may be used to prove that a body cannot begin to fall under the influence of gravity; for a body cannot leave its initial position without acquiring velocity, and (by the law of energy) cannot possess a velocity without having already fallen.

Now the fallacy of this argument, to prove that a body cannot fall from rest under the influence of gravity, may be exposed in a moment. If by the allegation "a body cannot leave its initial position without acquiring velocity" is meant that acquisition of velocity is the necessary result of its leaving the initial position, the argument involves a *non sequitur*. For, granting what is here affirmed, and granting, with or without the aid of the law of energy, that the body "cannot possess a velocity without having fallen," the conclusion arrived at simply does not follow from the premises.

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* Another way of expressing the argument is as follows:—Change of density can only occur through motion of the particles; but the particles being originally at rest and the system in equilibrium, motion of the particles can only occur from change of pressure. In other words, change of pressure (the cause) must precede the motion which it effects, and must precede therefore the change of density which results from the latter. But the received law of pressure asserts the contrary, viz. that change of density must precede change of pressure, which is absurd.

If, on the other hand, by the above allegation is meant that a body cannot move from a particular position unless it have previously acquired a certain velocity, the point to be proved is simply taken for granted.

If I have misunderstood the point of Mr. Strutt's paradox, I trust that he will set me right in regard to it; and as he must be well acquainted with the fallacy which it involves, he will perhaps not object to state distinctly in what that fallacy consists.

As to the paradox which I have brought forward, I contend that its fallacy consists in the false assumption of the received law of pressure. If Mr. Strutt is not satisfied with this explanation, I must call upon him to state definitely the particular point in which my reasoning is defective, instead of contenting himself with a vague assertion of analogy, where, as I contend, nothing of the kind exists.

Mr. Strutt thinks I "must admit that it is remarkable that so apparently reasonable a law should lead to such absurd conclusions."

I view the matter in a totally different light. In any systematic investigation of the subject which may be made in the present state of our experimental knowledge, it appears to me so much a matter of course to assume that the expression for the pressure will contain both velocity and density, and the contrary supposition appears to me to be so opposed to every sound principle, that the signal failure of the latter is exactly what I should have expected. It was in fact this anticipation that led me to the examination which has resulted in the detection of the cases of failure I have here and elsewhere adduced. With the statement of one of them, of somewhat peculiar character, I will conclude these remarks.

In the last case, a vacuum being supposed to exist above the piston, suppose that instead of an additional weight being introduced the piston is suddenly removed at the time t, then at the time t, the density of the air being throughout unaltered, we shall have according to the received law a finite pressure at the highest point of the aërial mass; i. e. *we shall have a pressure where nothing is pressed!*—a conclusion opposed alike to the dictates of common sense and the significance of language.

6 New Square, Lincoln's Inn,
January 6, 1873.

XIV. *On Manometric Flames.*

By Dr. Rudolph König (*of Paris*).

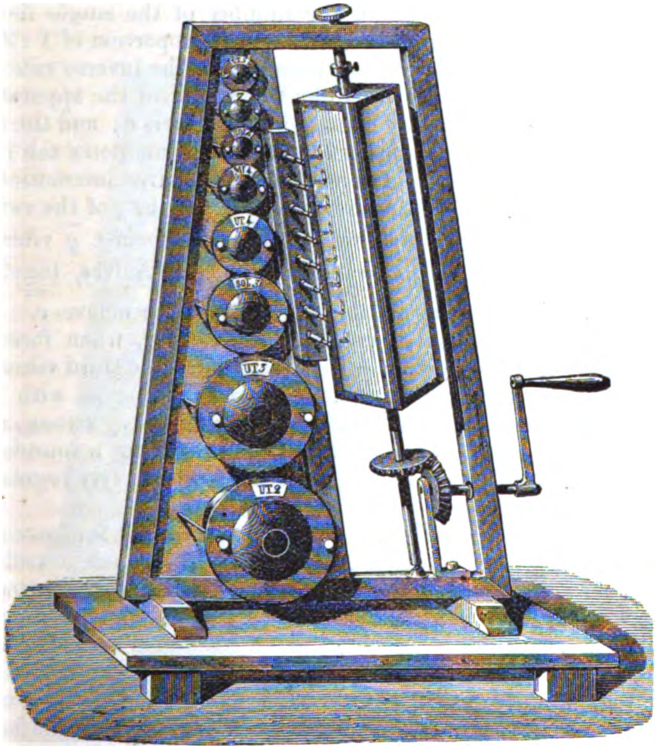
[Concluded from p. 18.]

[With a Plate.]

Decomposition of Sounds into their simple Tones.

THE same resonators (Helmholtz's) which serve for analysis of sounds by means of the ear, are also of use for the visible dissection of sounds by the flames. To this end I construct an apparatus with eight resonators tuned to the harmonic notes of *c*, each of which is connected with a manometric flame. These eight flames are placed in a slanting line one above the other, and show, in the rotating mirror fixed in the same direction, eight parallel bands of light when in repose, and when in vibration eight waved lines (fig. 11), Of course in this case each flame must be perfectly independent of the other, and each flame

Fig. 11.



vibrate only when its particular resonator is put in action by a note in unison; the notes *not* contained in the series of resonators must have no effect whatever on any of the flames. In order to show how far the apparatus fulfils these conditions, I usually employ a series of tuning-forks on sounding-chests, which, particularly a few moments after being sounded, give almost simple notes.

I first take forks which are in tune with the resonators, and sound them singly, and show that only the bands of light which correspond to their notes dissolve into vibrations, so that several simple notes must be sounded to cause the appearance of several serrated bands of light. By means of a tuning-fork not in tune with the resonators, I can then show that its note, even when sounded with considerable force, has no effect on the flames. A note sounded with very great intensity may indeed have some effect on all the flames, through the resonators; but this case will not give rise to error, as all the flame-series appear equal, whereas, when resonance takes place, the number of the single flame-waves in the series increases upwards in the proportion of 1 : 2 : 3 &c., and their width, of course, decreases in the inverse ratio.

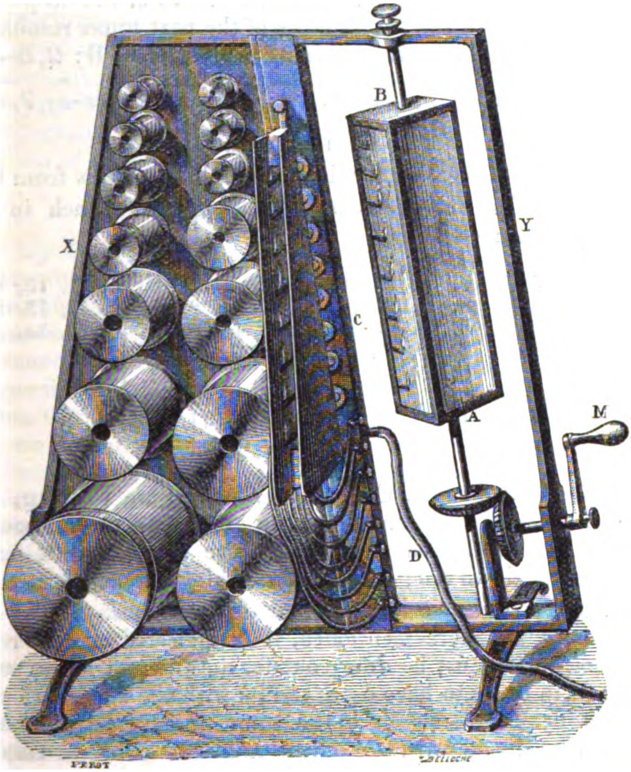
After demonstrating in this way the nature of the apparatus, I produce before it a sound whose fundamental is *c*; and the serrated bands of light then show by what harmonic notes the fundamental is accompanied, as well as the relative intensities of these notes. If before the apparatus we play the *g* of the violin, for which the apparatus has no resonator, the octave $\overline{g}$ vibrates strongly, and the $\overline{c}$ of the same instrument resolves, together with the flame of the fundamental, that of the octave $\overline{\overline{c}}$. An open organ-pipe, of small diameter, tuned to *c*, when forcibly blown, set the first five flames in vibration, but the third vibrated more strongly than the octave. A closed organ-pipe with the same fundamental caused the twelfth to appear very strong, and the fifth very weak. A protruding tongue without a sounding-cup resolved the first six harmonic notes with pretty regularly decreasing intensity.

On singing the vowel U, the octave as well as the fundamental shows rather strong vibrations, and only sometimes a trifling effect may be observed on the third note. D, on the contrary, excites the flames of the third and fourth notes very strongly, while the vibrations of the octave are weaker than with U. The fifth flame-band is serrated, but slightly, with O. With O A the region of greatest intensity becomes higher; it is the fourth and fifth notes which show the deepest indentations in the band of light, while the lower notes are weaker. With A all the flames are resolved up to the seventh, and the fourth, fifth, and sixth vibrate with great force. When E is sung, we see the fundamental accompanied by the octave weakly, and very strongly by the twelfth. The double octave and its third show vibrations of moderate intensity; and the seventh flame shows traces of the existence of the seventh tone. The letter I sung on *c* gives a strong movement to the flames of the octave and the fundamental only, while all the other flames are in repose.

The resonators 7 and 8 $(\overline{\overline{\overline{c}}})$ of the apparatus cause their flames to vibrate with difficulty, and the notes must be very strong. We have now reached the limits within which the flames can be usefully employed.

As this apparatus does not permit us to choose the fundamental tone of the vowel or of any other sound which is to be analyzed, it is adapted rather to demonstration than to further investigation. However, to make it more useful for the latter purpose, I have constructed a second model (fig. 12), in which the

Fig. 12.



eight spherical resonators are replaced by fourteen universal resonators. These resonators consist each of a cylinder, its length about equal to its diameter, which is formed by two pipes placed one within the other. The outer of these pipes terminates at one end in a hemisphere, from which the tube for the ear is carried, as in the spherical resonators. The opposite end of the inner pipe is closed by a plate, in the middle of which there is an opening for the passage of the enclosed air to the exterior atmosphere. This arrangement permits us by drawing out the pipe to increase the mass of air in the resonator, and to lower its tone by a third. On the inner tube lines are drawn which indicate how far the outer one must be drawn out for the different notes. The deeper resonators of the series are so constructed that the highest note of the larger shall always reach to the lowest of the next smaller one. In the higher-toned resonators this would not be sufficient, because the sixth, seventh, and eighth accessory notes approach each other so nearly that the necessity might occur of forming two of them with the same resonator. Since, therefore, the highest notes of the deeper are a whole note above the lowest notes of the next upper resonator, the whole series contains the following notes:—1, G–B; 2, B–*dis*; 3, *dis*–*fis*; 4, *fis*–*a*; 5, a–$\overline{c}$; 6, $\overline{c}$–$\overline{e}$; 7, $\overline{e}$–$\overline{gis}$; 8, $\overline{gis}$–$\overline{\overline{c}}$; 9, $\overline{\overline{c}}$–$\overline{\overline{e}}$; 10, $\overline{\overline{d}}$–$\overline{\overline{f}}$; 11, $\overline{\overline{e}}$–$\overline{\overline{gis}}$; 12, $\overline{\overline{f}}$–$\overline{\overline{a}}$; 13, $\overline{\overline{gis}}$–$\overline{\overline{\overline{e}}}$; 14, $\overline{\overline{\overline{c}}}$–$\overline{\overline{\overline{d}}}$.

The series of overtones for the notes of both octaves from C–$\overline{c}$ are to be found in the resonators placed opposite to each in the following Table:—

C:	2, 4, 5, 6, 7, 8, 9, 10.	*c* :	2, 5, 7, 8, 9, 11, 13, 14.
D:	2, 4, 6, 7, 8, 9, 10, 11.	*d* :	2, 6, 8, 9, 10, 12, 13, 14.
E:	3, 5, 6, 7, 8, 9, 10, 11.	*e* :	3, 6, 8, 9, 11, 13, 14.
F:	3, 5, 7, 8, 9, 10, 11, 13.	*f*	3, 7, 8, 11, 12, 13.
G:	1, 4, 6, 7, 8, 9, 10, 11,	*g* :	4, 7, 9, 11, 13.
A:	1, 4, 6, 8, 9, 10, 11, 12,	*a* :	5, 8, 9, 12, 14.
B:	1, 5, 7, 8, 9, 11, 12. 13,	*b* :	5, 8, 11, 12.
		$\overline{c}$:	5, 8, 11, 13.

For the fundamentals C–F the resonators are wanting, but one can make observations up to the ninth note of the sound. For the sounds G–*d* the resonators serve to the eighth note; then they begin to fail; at *e* we can employ only six flames, at *f* five; and at last at $\overline{c}$ but three for the overtones.

Although, as before mentioned, it is indicated on each resonator how far it must be drawn out for the different notes, yet it is as well, in order to have exact results with the apparatus, particularly if the fundamental of the sound to be investigated does not exactly coincide with one of the indicated notes, to employ the following mode of giving the desired pitch to the resonators in question.

Tune a string of the sonometer to the fundamental tone of the sound, and produce on it the harmonic notes one after another. Then place the proper resonator in communication with the ear instead of with the manometric capsule; and while the india-rubber tube is in the ear, it becomes very easy to determine their arrangement and the position for the strongest resonance.

After having tuned eight of these resonators to *c* and its overtones, I repeated the same experiments with this apparatus as I tried on the spherical-resonator apparatus, and obtained exactly the same results. There was not the least sign of any weakened sensitiveness in the flames; so that this apparatus appears to me exactly fitted for more exact and searching experiments on sounds in general, and particularly those of the human voice, at least those composed of notes which do extend beyond $\overset{=}{c}$. It is to be remarked that direct employment of the resonators with the ear does not succeed far beyond this limit.

Unfortunately I am now convinced that the state of my voice does not permit me to investigate any further in this direction, as I had intended; so I must be content to show the capabilities of the apparatus, as I shall again, when describing the method of experimenting on the vowel-sounds, and others also, by the elimination of single accessory notes, or whole series of them.

Interference-phenomena.

In my description of the results obtained by the combination of the notes of two organ-pipes I have not mentioned *unison.* The combination of two notes in unison has a special interest, on account of the communication of the vibrations and the interference-phenomena which may be observed therein. I therefore preferred deferring their description until now, when I could explain them in connexion with other similar experiments.

If we place two organ-pipes tuned in unison in communication with two flames and sound only one of them, the flame of the other shows that its air-column also vibrates in sympathy through communication; and this passing on of the vibrations takes place even if the organ-pipes are not in exact unison with each other, and therefore when sounded together cause *beats* to be heard. But it is to be remarked that in this case the sympathetic pipe does not form its own vibrations, but only vibrations which are exactly in unison with those of the one acting on it, so that beats are neither heard nor their effects seen in the flame. If, however, we blow the second organ-pipe and thus cause its own vibrations, they unite with the resonance-vibrations, and the flame shows clearly, by its violent flickering, the existence of beats, which are also heard distinctly.

I draw particular attention to this isolated occurrence of the resonance-vibrations in the air-column, because it is not exhibited by the influence-phenomena of two strings stretched above the same sounding-board; but the proper vibrations combined with resonance-vibrations appear in the string influenced, without its being struck or bowed.

It is known that the beats of two such mutually sympathetic strings accommodate themselves to each other in such a manner that the one reaches the maximum amplitude of its vibrations when the other is at the minimum. Now the flames of the two sympathetic organ-pipes exhibit the same phenomenon, for as the one rises the other falls; both, however, must be blown at the same time, whilst it is only necessary to play on one of the strings.

When the pipes are in perfect unison, and their single vibrations mutually adapt themselves in the same way as the beats did, *i. e.* that in the node of the one there is a condensation of the atmosphere when in the other a rarefaction takes place, then the whole process can be clearly observed in the two flames if we place them one beneath the other in a vertical line. Both flames show their vibrations unweakened; yet their individual pictures in the rotating mirror are not beneath each other in the two lines, but alternate.

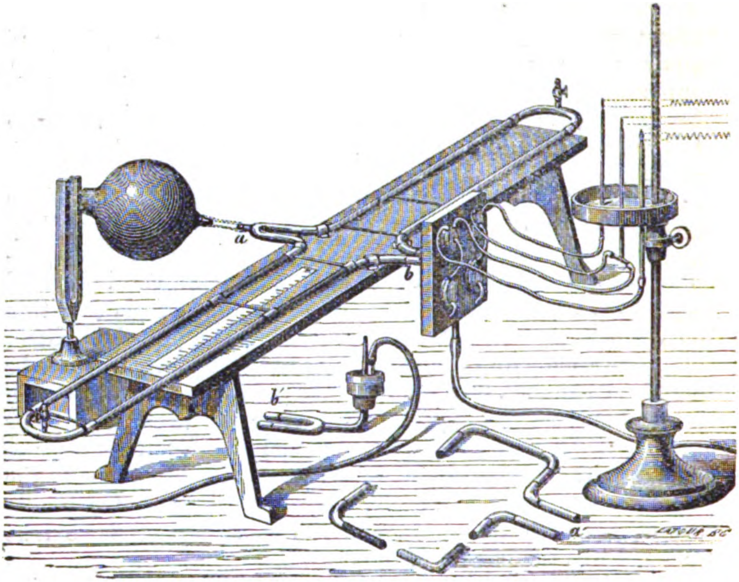
If both notes act together on the same flame, they, of course, at the beats show more violent flickerings than did the two flames; for the latter were produced by direct and by sympathetic and therefore unequally strong vibrations in the same air-column, whilst the present ones are formed by direct and therefore nearly equally strong notes in two similar air-columns. If the two notes are approximated gradually to unison, we observe that the oscillations cannot be made slower at will, as with tuning-forks, but at a certain limit they disappear suddenly, and both air-columns vibrate as one system, *i. e.* as two somewhat differently tuned bodies that are so closely united and therefore act so strongly on one another that neither can give its proper note in its integrity, and the consequence is that only a single intermediate note is produced. This note is more powerful than that of a single organ-pipe; and the flame shows in the centre of its interior a brilliant waist, which rises above a non-brilliant blue broad hollow space. As it approaches perfect unison more and more, the height of this dark space increases, the brilliant waist vanishes; and when unison is attained the flame appears in complete repose. At the same moment the strong fundamental has almost disappeared, and we hear the first overtone clearly produced; for it is known that, there being a difference of half a vibration-period between two equal sounds in unison, while the fundamental and the odd overtones are destroyed, all the even overtones in both sounds vibrate without difference of phase and strengthen one another. The flame also makes the octave recognizable in the rotating mirror, since we see a series of low wide flame-pictures, of which each single one is forked. It is well in this experiment to employ a rather stronger air-pressure, in order to increase the intensity of the octave in the sound of the pipes.

As this prominence of the octave at the interference of the fundamentals of two sounds is demonstrated particularly well by means of the double siren of Helmholtz, I represented the phenomenon in this case also by the flames. To this end I provided each of the two sounding-chests over the turning plates with a tube, which permitted its interior space to be placed in direct communication with the tube leading to the capsule. This tube was of india-rubber, thus retaining the power of movement within certain limits for the upper wind-chest of the siren, so as to be able by its different positions to produce the interference or to withdraw it. Invariably, if we approach the siren-chest to the interference-place, we see the great vibrations of the fundamental gradually disappear, and the short forked flame take their place as the picture of the octave.

A particular apparatus, which I construct for the observation of interference-phenomena of the most various kinds, is founded on the method first employed by Herschel, and after him by many natural philosophers. This is to produce interference by permitting the waves from the same source to traverse two courses differing in length by half a wave, and then to reunite them. It consists of a tube that between its ends branches into two arms, one of which can by drawing out be lengthened at will (fig. 13). If we wish a complete interference, we must introduce a simple note into the tube, which is joined to a resonator, before which we sound the proper tuning-fork. If we now lengthen the one arm until the difference of length of the two is equal to half the wave-length of the note of the tuning-fork, the waves coming from the two arms are mutually destroyed at the other end of the tube; and if we fix this into a small cavity, over which a manometric capsule is placed, we see, on drawing out one of the arms of the tube, how the at first deeply serrated flame-series in the rotating mirror gradually transforms itself into a simple band of light, until the difference of a half wave-length is attained.

But the interference can be shown still more beautifully by another arrangement. Instead of causing the arms united to a single tube to act on a capsule, I place a small apparatus to both exits of the two tube-branches; this is so arranged that now each branch is in communication with a separate capsule. These two capsules, whose action on each other is annulled by two accessory capsules, are provided with two gas-pipes instead of one. On a stand are placed three burners, which are fixed at different elevations; the centre one is arranged for the reception of two india-rubber tubes. I connect now one gas-pipe of the one capsule with the highest burner, one pipe of the other capsule with the lowest, and by means of the remaining two exit-pipes I place both capsules in communication with the centre burner. If I now strike the tuning-forks while the lengths of the tube-branches are equal, the three flames in the rotating mirror show three equally deeply serrated flame-series one above another, of which the centre alone changes into a simple band of light on lengthening one of the branches a half wave-length of the note, while both the other flames continue to vibrate with unchanged intensity. Thus we have here at the same time a view of the action of the sound-waves when they approach through the one arm alone, when they have passed through the second only, and also when they arrive united at the flame after passing through both.

Fig. 13.



If in these experiments we employ instead of a tuning-fork with a resonator an open organ-pipe of not too great diameter, during the interference of the waves of the fundamental the vibrations of the octave become again prominent. By interference we can remove not only the fundamental, but any overtone we please from a sound, as may be clearly demonstrated with the above-described covered pipe. I conduct the sound into the apparatus, while I connect with it, after the removal of the gas-burner, the capsule at the end by means of an india-rubber tube. If I then draw out the one tube so far that interference ensues for note 3, the centre flame in the mirror shows the simple flame-series of the fundamental, while the two others form the picture before described (fig. 5, Pl. I.), resulting from the combination of notes 1 and 3. In the same way we can banish from vowel-sounds various overtones, or rather whole series of them, which offers a new and fruitful method for the investigation. In these experiments the arrangement with three flames is particularly useful, because the upper and lower flames remaining always unchanged permits the slightest alteration in the middle one to be observed. Thus, for example, the vowel U sung on $\bar{c}$ into the apparatus shows the fundamental only weakly, accompanied by the octave. If we place the apparatus so that the waves of $\bar{\bar{c}}$ interfere, every trace of this octave is lost, whereas on the interference of the fundamental two narrow flames of almost equal height take the place of each wide flame; these narrow flames represent the octave, now almost alone. With O sung on the same note (when the fundamental is accompanied much more strongly by the octave than with U) we can make the same experiments; only here at the interference of the octave the note 3 becomes prominent, whilst the wide flame of the fundamental spreads out into three diminishing summits. A sung on $\bar{c}$, at the interference of the third note brings forward strongly the octave with the fundamental. If the waves of the octave interfere, there appears a group of five flame-summits, which appear to indicate the notes 1, 3, and 5. If we suppress the fundamental and with it the notes 3, 5, &c., there appears a simple flame-series, which is formed by the octave alone.

These phenomena are nevertheless not always of so simple a nature as in these examples, when it is a question of more composite flame-groups of the deeper sounds; and therefore I will now call attention to the fact that, on lengthening one of the tubes of the apparatus, we often see suddenly very great changes in the flame-picture when the former is between the interference-points of two successive overtones of the sound. This is then the interference-point of the lower octave, or twelfth of a higher overtone of the sound, which is in this way removed.

In the place of the forked tube into which in all the foregoing experiments the note or sound was introduced, we can put two separate tubes of exactly equal length and form, each consisting of three separate pieces inserted one in another and capable of being turned round so that we can move the two openings at their ends in any direction we please without alteration of the length of the tube or of the form of its turnings. This arrangement permits then the entrance of the note of two different points of a vibrating body into the apparatus—for example, of two vibrating bridges of a plate with contrary signs, or of the same place on its opposite surfaces: in both these cases the interference takes place when the two paths are equal, and the tone first becomes audible when the interference is destroyed by lengthening one of the compound tubes.

In order to adapt the apparatus to the demonstration of the wave-lengths of a note in different gases, and for the experiments of Zoch, I have provided the pipes with two cocks, which serve to fill and empty them. Of course, if we experiment with any other gas than atmospheric air, the resonator cannot remain in direct communication with the interior of the pipe; and therefore we must in that case place between them a small cavity, which is divided in the centre by a thin membrane into two halves—the one to be united with the pipe, the other with the resonator. Besides we must then have india-rubber rings to draw over the ends of the tubes which are only placed within each other, so that the gas cannot escape at these places.

It is, of course, understood that this apparatus permits the direct observation of different interference-phenomena by the ear, and consequently the repetition of the experiments of Mach, Quincke, and others. For this purpose we have but to place one of the forked tubes before the apparatus and connect the former with the ear by an india-rubber tube.

XV. *On the best Arrangement of* Wheatstone's *Bridge for measuring a given resistance with a given Galvanometer and Battery.* *By* Oliver Heaviside, *Great Northern Telegraph Company, Newcastle-on-Tyne**.

In the figure, *a*, *b*, *c*, and *d* are the four sides of the electrical arrangement known as Wheatstone's bridge or balance, *e* the galvanometer, and *f* the battery branch. Throughout this paper *d* is supposed to be the resistance to be measured, and *e* and *f* both known. The problem is to find what resistances should be given to the sides *a*, *b*, and *c* (which we are able to vary), so that the galvanometer may be affected the most by any slight departure from the balance which occurs when $a:b=c:d$.

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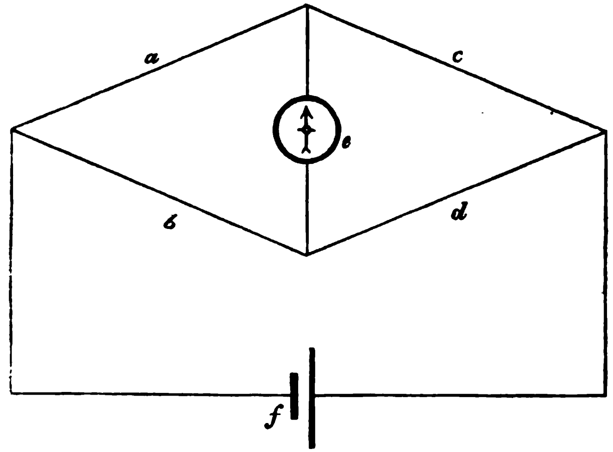
* Communicated by the Author.



The nature of this problem may be more easily understood from the following considerations :—

1. If b, c, d, e, and f are given, then there is only *one* value of a which will produce a balance, viz. $a=\dfrac{bc}{d}$.

2. But if c, d, e, and f are given, but not b, then there is an infinite number of pairs of values of a and b which will produce a balance by satisfying the relation $a:b=c:d$; and one particular pair will constitute the best arrangement, by which is meant that the galvanometer will be most sensitive to any slight departure from the equality of $\dfrac{a}{b}$ and $\dfrac{c}{d}$ when those particular values of a and b are used.

3. And if only d, e, and f are given, then for any value we give to c there is a pair of values of a and b which constitutes the best arrangement for that value of c; and there will be a particular value of c which, with the corresponding values of a and b, will be the best arrangement for the given values of d, e, and f.

In order to find what functions a, b, and c must be of d, e, and f to constitute the best arrangement, it will be first necessary to find the best values of a and b when c, d, e, and f are given. This I now proceed to do.

It is well known, and may be easily proved by Kirchhoff's laws, that the current passing through the galvanometer is represented by $u=\mathrm{E}\times$

$$\frac{(a+b+c+d)(ad-bc)}{\{(a+b)(c+d)+(a+b+c+d)e\}\{(a+c)(b+d)+(a+b+c+d)f\}}, \quad . \quad (1$$

in which E is the electromotive force of the battery. $(ad-bc)$ may be positive, negative, or nothing, in which last case $u=0$, and a balance is obtained, no current passing through the galvanometer.

Dividing both numerator and denominator of (1) by

$$(a+b+c+d)^2,$$

it becomes

$$u=\mathrm{E}\times\frac{\dfrac{ad-bc}{a+b+c+d}}{\left\{\dfrac{(a+b)(c+d)}{a+b+c+d}+e\right\}\left\{\dfrac{(a+c)(b+d)}{a+b+c+d}+f\right\}}; \quad . \quad (2)$$

from the form of which it may easily be seen that the best value of the resistance of the galvanometer e, when a balance is obtained and the other resistances are fixed, is, as Schwendler has shown in the Philosophical Magazine for May 1866,

$$e=\frac{(a+b)(c+d)}{(a+b+c+d)}=b\cdot\frac{c+d}{b+d}; \quad . \quad . \quad . \quad . \quad (3)$$

that is, the resistance of the galvanometer should equal the resistance external to the galvanometer, being the joint resistance of the two parallel branches $(a+b)$ and $(c+d)$. Also it may be proved that the best arrangement of the battery is obtained when its resistance equals the external resistance, that is,

$$f=\frac{(a+c)(b+d)}{a+b+c+d}=c\cdot\frac{b+d}{c+d}, \quad . \quad . \quad . \quad . \quad (4)$$

the joint resistance of the two parallel branches $(a+c)$ and $(b+d)$.

(In passing, I may notice that Schwendler, in the paper above referred to, and also in a later one in the Philosophical Magazine for January 1867, has assumed it to be necessary for the battery resistance to be very small, in order that the relation exhibited in equation (3) may be satisfied. This appears to me to be totally unnecessary; for the resistance external to the galvanometer when a balance is obtained is quite independent of f, the battery resistance. In fact the proper resistance for the battery when it is to be most advantageously used is given by equation (4).)

As in the present paper we are only concerned with such values of a, b, c, and d as produce a balance, or nearly so, one of these four resistances may be eliminated at once. Let it be a. Then

$$\frac{(a+b)(c+d)}{a+b+c+d}=b\,.\,\frac{c+d}{b+d},$$

$$\frac{(a+c)(b+d)}{a+b+c+d}=c\,.\,\frac{b+d}{c+d},$$

and

$$a+b+c+d=\frac{(b+d)(c+d)}{d}.$$

Substituting these in equation (2), we get

$$u=\text{E}\times\frac{\dfrac{(ad-bc)d}{(b+d)(c+d)}}{\left\{b\dfrac{c+d}{b+d}+e\right\}.\left\{e\dfrac{b+d}{c+d}+f\right\}}$$

$$=\text{E}d\times\frac{ad-bc}{(bc+ef)(b+d)(c+d)+ce(b+d)^2+bf(c+d)^2}.\quad.\quad(5)$$

Now c, d, e, and f being fixed, and b the variable, we have to make u a maximum. As Ed is constant, it may be dismissed. As to the numerator $(ad-bc)$, it vanishes when at a balance; but of course such a thing as an exact balance is unattainable. Let $d\pm\Delta$ be the real value of the resistance we are measuring, d being the calculated value $\frac{bc}{a}$, and Δ a small difference, then

$$a(d\pm\Delta)-bc=\pm a\Delta.$$

Therefore the numerator varies as a or as b, since in the present case a and b vary together. Hence we may write b for $(ad-bc)$. Thus

$$u=\frac{b}{(bc+ef)(b+d)(c+d)+ce(b+d)^2+bf(c+d)^2}.$$

By differentiation and putting $\frac{du}{db}=0$, we obtain

$$(bc+ef)(b+d)(c+d)+ce(b+d)^2+bf(c+d)^2=bc(b+d)(c+d)$$
$$+b(bc+ef)(c+d)+2bce(b+d)+bf(c+d)^2;$$

therefore

$$ef(b+d)(c+d)+ce(b+d)^2=b(bc+ef)(c+d)+2bce(b+d),$$
$$def(c+d)+ce(b+d)^2=b^2c(c+d)+2bce(b+d),$$
$$b^2c(c+d+e)=de(cd+df+fc),$$

which gives the relation sought,

$$b=\sqrt{\frac{d}{c}\,.\,\frac{cd+df+fc}{c+d+e}}\,.\,e;\quad.\quad.\quad.\quad.\quad(6)$$

and as $a=\frac{bc}{d}$, therefore

$$a=\sqrt{\frac{c}{d}\cdot\frac{cd+df+fc}{c+d+e}\cdot e}. \quad . \quad . \quad . \quad . \quad (7)$$

These values of a and b will be found to make $\frac{d^2u}{db^2}$ negative; therefore they give the most sensitive arrangement for the fixed values of c, d, e, and f.

If b vary from nothing upwards, it will be found that u rapidly increases up to its maximum value and then slowly decreases, from which it may be concluded that it is better to use too large values of a and b than too small.

In case $c=d$, formulæ (6) and (7) become

$$a=b=\sqrt{ce\frac{c+2f}{2c+e}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

As a numerical example of these formulæ, suppose the resistance to be measured $d=1000$ ohms, the galvanometer $e=500$ ohms, the battery resistance $f=100$ ohms, and we make $c=1000$ ohms; then the best values for a and b will be found to be $\sqrt{240{,}000}=100\sqrt{24}$, or nearly 500 ohms.

Having thus determined the relations of a and b to c, d, e, and f, the latter resistances being fixed, we now proceed to the second part of the problem, to determine the best values of a, b, and c when only d, e, and f are given. This is the case which occurs so often in practice, when we have a battery, a galvanometer, and a resistance to be measured, and three sides of a bridge to which we may give any values we choose (within certain limits).

Insert the values of a and b, as given in equations (6) and (7), in equation (5); then, after some reductions, we obtain $u=$

$$\frac{ad-bc}{2de(cd+df+fc)+\{(c+d+e)(cd+df+fc)+cde\}\sqrt{\frac{d}{c}\cdot\frac{cd+df+fc}{c+d+e}e}}.$$

We must now consider c the independent variable, a and b being dependent variables. $(ad-bc)$ still varies as a. It does not, however, vary as b, but as the product bc or ad, since d is constant. Therefore we may put the known value of bc in the numerator instead of $(ad-bc)$. Thus $u=$

$$\frac{\sqrt{cde\frac{cd+df+fc}{c+d+e}}}{2de(cd+df+fc)+\{(c+d+e)(cd+df+fc)+cde\}\sqrt{\frac{d}{c}\cdot\frac{cd+df+fc}{c+d+e}e}}.$$

Multiply numerator and denominator by $\sqrt{\frac{c}{de}\cdot\frac{c+d+e}{cd+df+fc}}$, and we have

$$u=\frac{c}{2\sqrt{cde(c+d+e)(cd+df+fc)}+(c+d+e)(cd+df+fc)+cde},$$

which has to be made a maximum. Differentiating and putting $\frac{du}{dc}=0$,

$$2\sqrt{cde(c+d+e)(cd+df+fc)}+(c+d+e)(cd+df+fc)+cde$$

$$=\frac{c}{\sqrt{cde(cd+df+fc)(c+d+e)}}$$

$$\times\left\{cde(cd+df+fc)+cde(c+d+e)(d+f)+de(c+d+e)(cd+df+fc)\right\}$$

$$+c(c+d+e)(d+f)+c(cd+df+fc)+cde.$$

Therefore

$$2\sqrt{cde(c+d+e)(cd+df+fc)}+df(d+e)-c^2(d+f)$$

$$=\frac{cde\left\{c(cd+df+fc)+c(c+d+e)(d+f)+(c+d+e)(cd+df+fc)\right\}}{\sqrt{cde(cd+df+fc)(c+d+e)}}.$$

Multiplying both sides of this equation by the denominator on the right-hand side and reducing, we get

$$\left\{df(d+e)-c^2(d+f)\right\}\sqrt{cde(c+d+e)(cd+df+fc)}$$

$$=cde\left\{c^2(d+f)-df(d+e)\right\},$$

which is satisfied by

$$df(d+e)-c^2(d+f)=0,$$

which gives the required relation,

$$c=\sqrt{df\frac{d+e}{d+f}}; \quad \ldots\ldots \quad (9)$$

that is, c equals the square root of the product of the joint resistance of the battery and the resistance to be measured, into the sum of the resistance of the galvanometer and the resistance to be measured. Inserting this value of c in (6) and (7), we find the values of a and b to be

$$a=\sqrt{ef}, \quad \ldots\ldots\ldots \quad (10)$$

$$b=\sqrt{de\frac{d+f}{d+e}}. \quad \ldots\ldots \quad (11)$$

In using the Wheatstone's bridge for measuring very high resistances, as, for instance, the insulation resistances of (good) telegraph lines, the battery resistance is usually very small in comparison with that of the line; hence $\frac{df}{d+f}$ will be very little different from f. When this is the case, formula (9) becomes

$$c=\sqrt{f(d+e)}.$$

If also the galvanometer resistance is small compared with the resistance to be measured, then these equations are sufficient for the determination of b and c,

$$b=\sqrt{de},$$
$$c=\sqrt{df}.$$

As a numerical example of these formulæ, suppose $f=100$ ohms, $e=1000$ ohms, and d is known to be about 1,000,000 ohms. Then by (10),

$$a=\sqrt{100,000}=316 \text{ ohms}.$$

By (9),

$$c=\sqrt{\frac{10^6\times 10^2}{10^6+10^2}(10^6+10^3)}=10004 \text{ ohms},$$

$$b=\frac{ad}{c}=31608 \text{ ohms}.$$

These values of a, b, and c will be the best. The more convenient arrangement,

$$a=300, \quad c=10,000,$$
$$b=30,000, \quad d=1,000,000,$$

would be very nearly the best.

It appears to me that the formulæ (9), (10), and (11), or those following, will be found of considerable practical value. If the same battery and galvanometer be always used, the side a of the bridge will be a constant resistance, and a Table of the nearest convenient values of b and c could be easily calculated for different values of d. Formula (3), which is Schwendler's, can evidently have only a very limited application, as, for instance to the construction of galvanometers for particular purposes. Formula (4) could be sometimes used; but it is a troublesome thing to make combinations of cells for "quantity" or "intensity," besides spoiling them if they are not all precisely similar.

In conclusion, if, to measure a certain resistance, the best resistances for the galvanometer, battery, and the three sides a, b, and c were required, then we should have to make $a=b=c=d=e=f$, which can be proved by combining equations (3), (4), (6), and (10). This, however, is more curious than useful.

XVI. *The Chemistry of Sulphuric Acid-manufacture.*

By H. A. Smith.

[Continued from p. 37.]

Section III. *On the Temperature at which Nitric Acid acts upon Sulphurous Acid.*

In Section I. I gave one or two laboratory experiments showing some of the conditions under which these gases act upon each other. I now wish to show the temperature at which this action takes place. These experiments were made in a similar manner to the former. I took the glass globe formerly used; and into this the mixed gases were led with the addition of a little water; it was then placed in another vessel containing cold water, arranged so that it could be raised to any required temperature, or boiled if necessary. A thermometer communicated with the interior, its bulb being nearly in contact with the water at the bottom of the globe. The temperature also of the exterior water was accurately observed. At the commencement of the experiment the temperatures were:—

Experiment I.

	Fahrenheit.
Interior of globe	36°·7
Exterior water	40·3

the interior being thus a little cooler than the exterior. The water was now cautiously and slowly heated, the temperature being observed from time to time, whilst the first formation of acid in the vessel was carefully noted, the results being:—

Experiment II.

Minutes.	Outside water (Fahrenheit).	Inside globe (Fahrenheit).	Remarks.
Commencement .	40°·3	36°·7	No action, ruddy fumes.
2 .	62·4	39	„ „
4 .	127·6	122·8	Ruddy fumes begin to disappear.
6 .	154·3	200·2	Remarkably quick and energetic action.

The globe was now withdrawn from the hot water in which it was and again plunged in cold; the temperature soon fell to 81°·5 Fahr.; but no change took place in the action, that continuing as active as ever. After it had been left in the water some time, it was seen that the great fall in temperature was only temporary; it soon began to rise.

Experiment III.

Minutes.	Outside water (Fahrenheit).	Inside globe (Fahrenheit).
Commencement	45°·6	81°·5
2	45·6	92·3
6	45·9	96·6

at which temperature it remained till the end of the experiment. I find, then, from this experiment that at 200° F. action commences, that at that temperature the sulphurous acid begins to act upon the nitric acid, whilst the second experiment shows that the heat developed by the action itself is pretty considerable.

The globe was now left for twenty-four hours in the cold water (see exp. II.), and after that time had elapsed the contents were submitted to analysis. I here give the result of three analyses:—

	per cent.
Sulphurous acid	6·21
Nitric acid	..
Sulphuric acid	93·91
	100·12

the temperatures at time of this analysis being

	Fahrenheit.
Inside globe	46°·9
Outside water	47·3

In the above experiments it is seen that the temperature never rose to the point of boiling water, but that that degree was very nearly approached.

SECTION IV. *The Distribution of Heat in the Lead Chamber.*

We have seen in the preceding section that 200° F. is the temperature at which nitric acid begins to act upon sulphurous acid. I now wish to show the temperature of the lead chamber in which the preceding action takes place on a large scale, and then to see if I can draw some conclusion as to the best temperature at which to keep the sulphuric acid-chamber.

In order to obtain a good idea of the temperature, I took daily observations at different points in the chamber during a year, and have condensed the results obtained into the form of diagrams.

Ordinary maximum and minimum thermometers were employed; but instead of being fixed, as usual, upon a wooden back, a *glass* back was employed, upon which the degrees were etched, and the thermometers fixed thereto by thick platinum wire, thus having instruments capable of resisting all acids.

These thermometers were lowered into the chamber at the different points by long "threads" of lead and allowed to remain for about two hours, the yield of vitriol and general appearance of the chamber being carefully noted each day. The results obtained by this investigation have been very completely borne out by those already shown in Section III., which, although I considered their proper place in the paper to be before the present section, nevertheless came later in the course of investigation.

As in a former case, I divided the chamber into separate parts, so that I could have some definite plan of procedure. In this case the chamber was divided along its length at four heights, thus—

1st, at the height of 24 feet from bottom of chamber,
2nd, " " 15 " " " "
3rd, " " 8 " " " "
4th, " " 3 " " " "

the temperature being taken every 10 feet along the length at these heights.

In looking at Diagram I. the first thing that strikes us is the very sudden fall in temperature which takes place in passing from 10 feet from the end of chamber (at the entrance) to 20 feet, there being a fall here of 87° F. After this the temperature is comparatively constant till it reaches 110 feet from end, when it again falls continually till it reaches 113° F., at which temperature the gas passes from the chamber. This is that portion of the chamber which I have previously called the "reservoir," and in which very little, if any, action between the gases takes place; and it is worthy of notice that, with the exception of the first ten feet, at no place in this portion does the temperature rise above 130° F. And if we turn to the previous section of this paper, we find that in exp. II. the nearest approach to this temperature is 122°·8 F.; and here we have the remark, "ruddy fumes begin to disappear." Here, then, is another proof, if another were required, that the upper part of the lead chamber is not of use as a condensing "space," but merely as a reservoir for containing the gases, and, *if necessary,* assisting proper mixture.

In this diagram we may see also the points in the chamber at which steam is thrown in. At 40 feet and at 70 and 110 feet respectively we have decided falls of temperature, these being very nearly the places of the steam-pipes. The high temperature at the beginning may be accounted for by the fact that shortly below this the pipe by which the gases are conducted to the chamber is inserted.

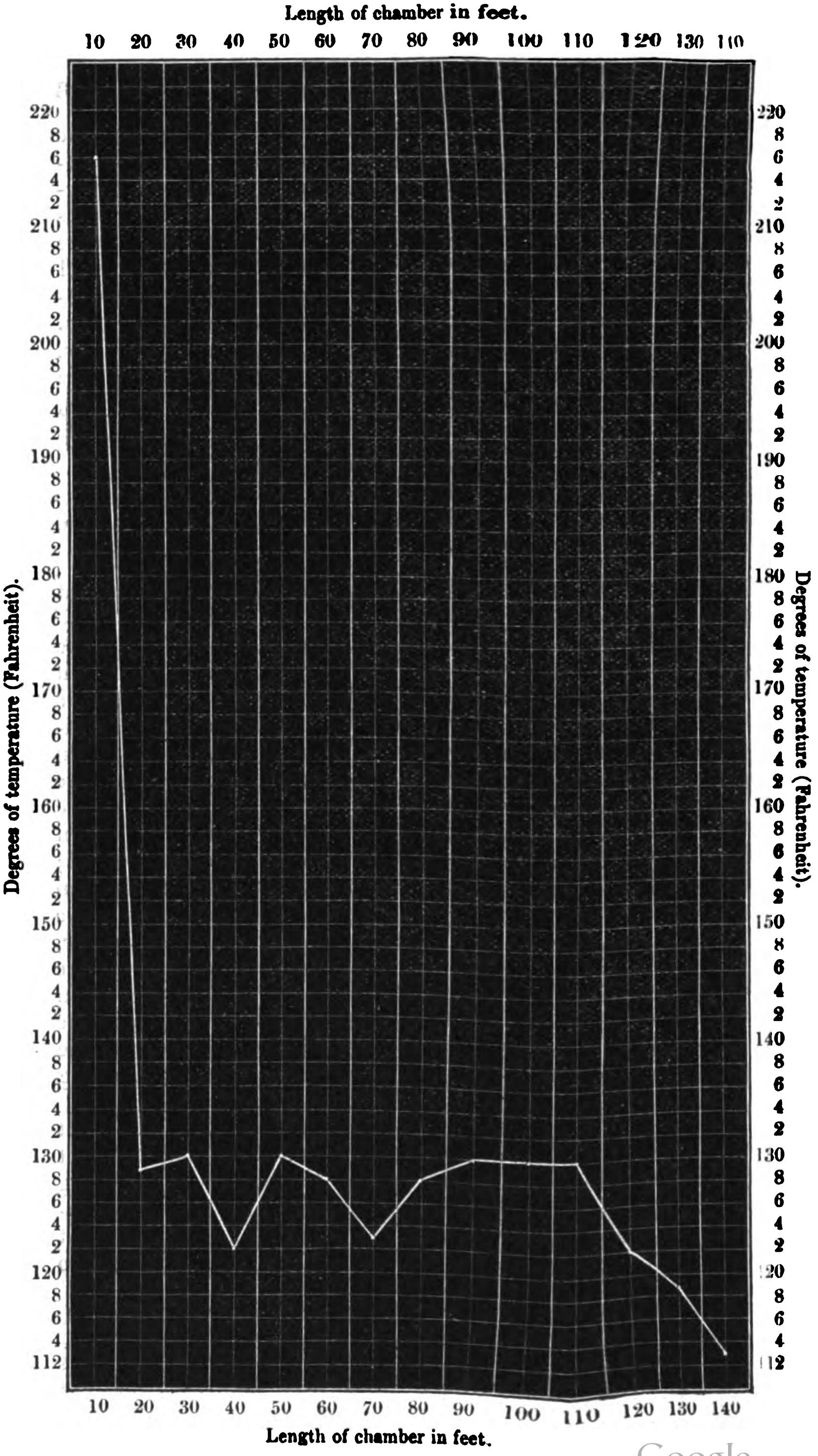


Diagram I.—Heat of Chamber at 24 feet from bottom.

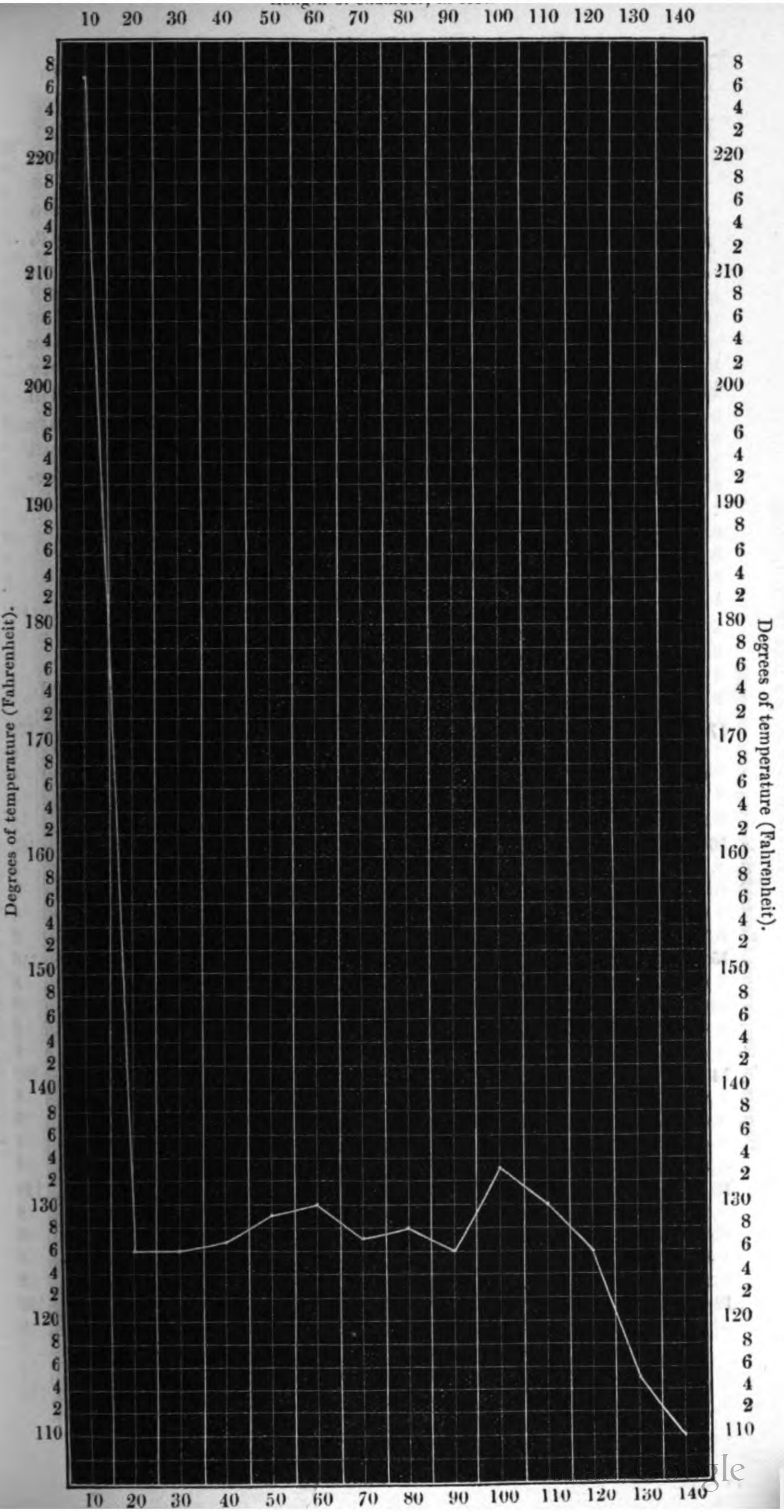


Diagram II.—Heat of Chamber at 15 feet from bottom.

Little is to be said respecting this diagram which has not been said on the former. In this also is noticeable the great fall in temperature which has taken place from 10 to 20 feet from end of chamber; in this case the fall is even greater than before, changing here from 227° F. to 126° F., a difference of 101° F. This is accounted for by the fact that the entrance-pipe is much nearer this point than the former, being in fact just above the spot where the temperature was observed. The regularity of the heat after this sudden fall is also, as in the former case, very remarkable—the variation being between 126° and 133° F. till we reach 120 feet from end of chamber, when the temperature falls to 110° F., the average temperature, however, being about 126° to 128° F.,—in this case also bearing out the fact (by reference to exp. II., previous section) that the upper part of the chamber is unnecessary.

Diagram III.

Heat of Chamber 8 feet from bottom.

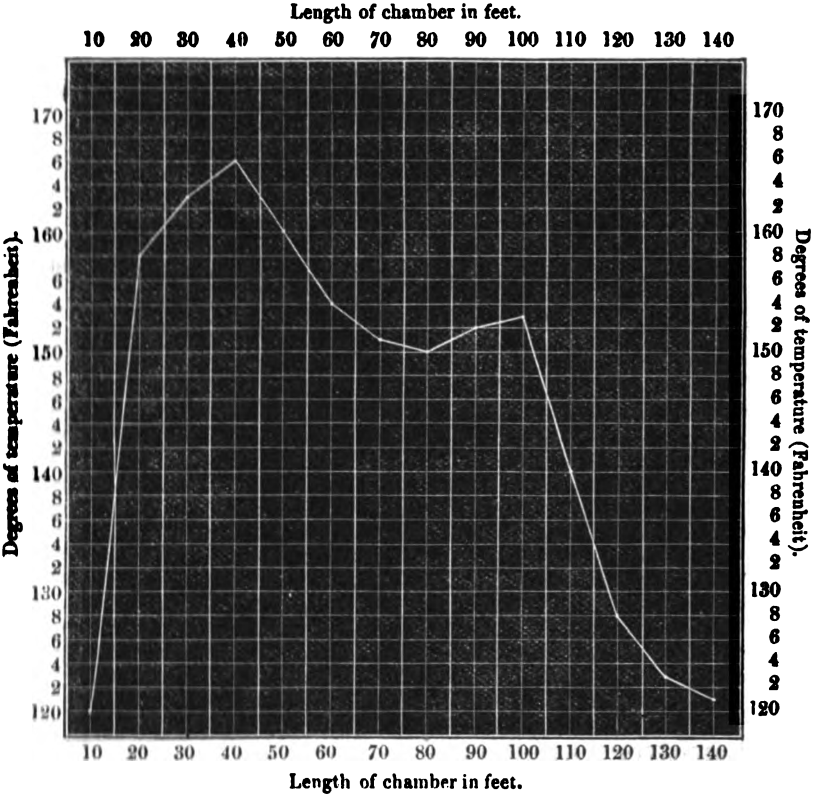
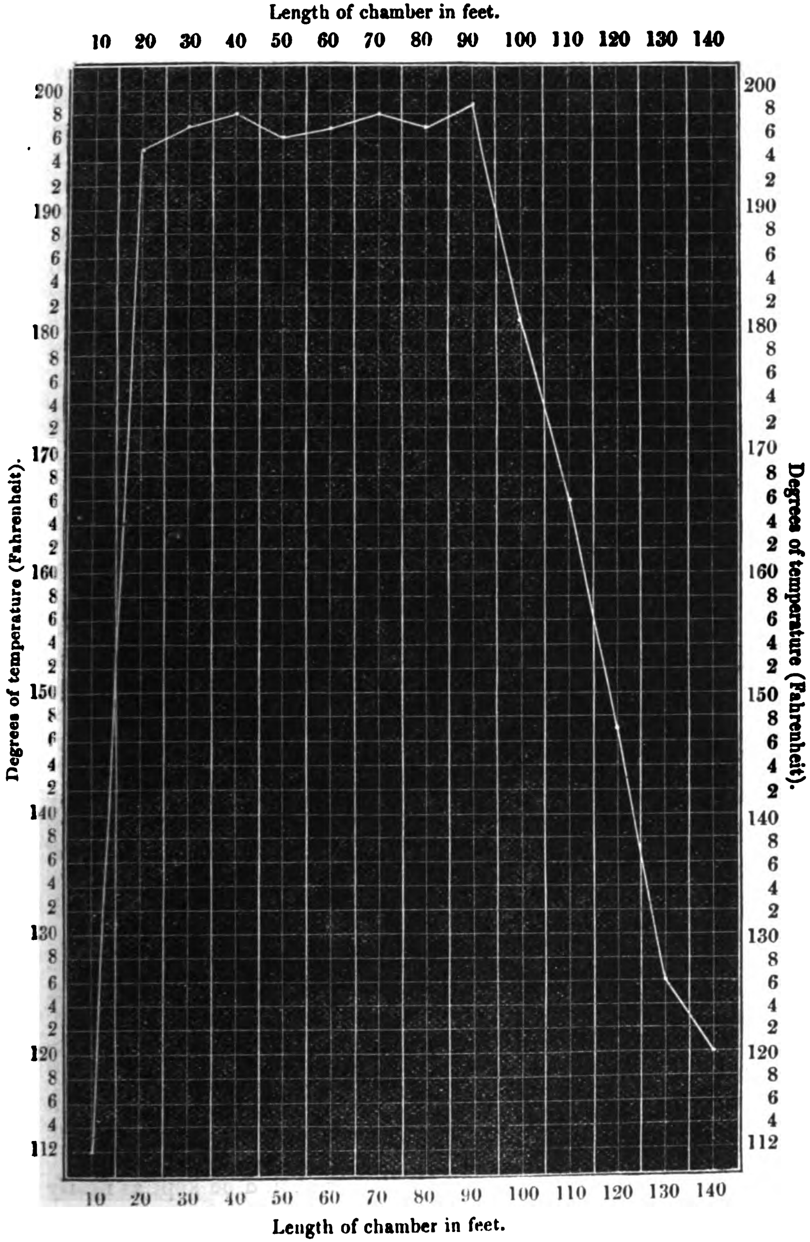


Diagram IV.

Heat of chamber 3 feet from bottom.



In Diagram III. we find a higher average of temperature, varying for the most part between 150° and 166° F.; whilst at entrance and exit the temperature is comparatively low, rising towards the centre of the chamber. Here, now, we approach very nearly the required temperature, 200° F. being the observed degree at which nitric acid acted on sulphurous acid; and by reference to my former paper we find that this portion of the chamber is really the "working" portion. It is necessary also to observe along with this diagram the next, Diagram IV. In my former paper I showed experimentally, by analyses of the gases at 3 feet from bottom of chamber, that the greatest amount of action went on at that portion; and I have shown also (Section III.) that the temperature at which nitric acid began to act on sulphurous acid was 200° F.; so that now by observing these two diagrams we see how closely the laboratory and the manufacturing results agree. I have already spoken of this in Diagram III.; but it is much more distinctly observed in Diagram IV. Beginning at 112° F., the temperature rises suddenly till at 20 feet from entrance it attains 195° F., *whilst the temperature is for the most part from* 195° *to* 199° F. After 90 feet from entrance of chamber it falls at an almost regular amount of 20 degrees for each 10 feet of chamber length, until at 140 feet from entrance of chamber the temperature is 120° F.

Here, then, we have an example of almost perfectly suitable temperature. At this time also the yield of vitriol obtained from this chamber was as nearly the theoretical amount as could practically be obtained; and it was found that whenever the temperature of the chamber was allowed to increase or diminish the result was bad. At this time the amount of nitric acid escaping was almost *nil*, whilst the colour of the liquid coming from the Gay-Lussac tower showed that the gas escaping was really NO^2 with a scarcely appreciable amount of nitric acid. (I may say here that whenever the colour of this liquor is of a *dark* red colour, it is a sign of the escape either of nitric acid or some of the higher oxides of nitrogen.)

The conclusions I draw from these and the preceding investigations may be summed up thus:—

1. The best form of chamber to be employed is one which is long and not high, the analyses pointing to one of somewhat the following dimensions—150 feet long, 25 or 30 feet wide, and about 10 or 12 feet high. We have thus a large condensing surface, the mixed gases coming readily into contact with all parts of the chamber, whilst they are also in contact with the previously condensed acid which rests on the sides of the chamber.

2. The temperature of the chamber should be kept as nearly

as possible about 200° F.,—this also acting as a regulator for the amount of steam thrown into the chamber.

3. That in "starting" a chamber, sulphuric acid should be run on the bottom in preference to water, which is at present generally employed.

XVII. *On Fractional Distillation.* *By* J. Alfred Wanklyn, *Corresponding Member of the Royal Bavarian Academy of Sciences*[*].

IT is an extraordinary fact that there is, up to the present time, no theory of fractional distillation. Chemists are perfectly aware that, on distilling a mixture of two unequally volatile liquids, they will find some of the less volatile liquid in an early portion of the distillate, and some of the more volatile liquid in a later portion of the distillate. And, on the other hand, chemists are quite as well aware that, notwithstanding this clinging together of different liquids, almost perfect separations are attainable when fractional distillation is laboriously and systematically practised. Every chemist's own experience supplies instances of the kind; and most of us have our own peculiar methods of performing the operation. My object, on the present occasion, is to lay down a general theory of this process, and to explain how it comes to pass that it is at once so laborious and yet so successful when duly performed.

In order to gain a clear idea of the effect of admixture, let the hypothetical case of two liquids of precisely the same volatility be supposed. In such a case each fraction of the distillate will have the same composition as every other fraction and as the original mixture. If the two liquids constituting the mixture be named A and B respectively, and if the relative quantities of them existing in the original mixture be expressed by a and b, then $a:b$ will express the ratio in which A and B are present in the original liquid and in every fraction of the distillate from first to last. If we picture to ourselves the course of events on heating such a mixture, we perceive that, on applying heat to it, the quantity of heat which is received by each constituent is exactly proportional to the quantity of each constituent present in the mixture. If a and b be the relative quantities of A and B, then a and b will also be the relative quantities of heat received by A and B in a given period of time. In the case under consideration the volatilities of A and B are supposed to be equal; therefore a and b, which express the original quantities of A and B, and which (as just shown) express the relative quantities of heat received by A and B, must also express the relative quantities of A and B in the vapour evolved on boiling, and consequently in the distillate.

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[*] Communicated by the Author.

Now let us pass from the hypothetical case of liquids of equal volatility to the actual cases of liquids of unequal volatilities. In such cases the composition of the vapour which escapes when the mixture is boiled will be different from the composition of the original mixture; and putting v for the coefficient of volatility of A, and v' for the coefficient of volatility of B, we have av and bv' for the relative quantities of A and B which escape in the form of vapour on distilling the mixture. If we put δ for an infinitely small first distillate, we have $\frac{av\delta}{av+bv'}$ for the quantity of A in the first unit of distillate, and $\frac{bv'\delta}{av+bv}$ for the quantity of B in the first unit of distillate. Collecting the formulæ, we have:—

$a+b =$ relative quantities of A and B in the original mixture.

$av+bv' =$ relative quantities of A and B in the first unit of distillate.

$\frac{av\delta}{av+bv'} =$ quantity of A in first unit of distillate.

$\frac{bv'\delta}{av+bv'} =$ quantity of B in first unit of distillate.

After the escape of the first unit of distillate $(=\delta)$ the composition of the liquid left behind in the retort will have altered, and the relative quantities of A and B will now be represented not by a and b, but by the formulæ

$$\left(a-\frac{av\delta}{av+bv'}\right)$$

and

$$\left(b-\frac{bv'\delta}{av+bv'}\right).$$

In order to find the values of A and B in the second unit of distillate, these formulæ must be substituted for a and b. It will be obvious that as the distillation progresses, the composition of the successive units of distillate must progressively alter. If v be greater than v', then the quantity of A in the units of distillate must progressively diminish; if v be less than v', the quantity of A in the units of distillate must progressively increase. Obviously, too, the greater the difference between v and v', the more rapid the alteration in the composition of successive units of distillate.

Such, I believe, is the real march of the process of fractional distillation; and these formulæ offer an explanation why by frequent repetitions and judicious management the chemist is able to effect almost perfect separations by means of fractional distillation. The coefficient of volatility, however, is something more than the tension of the vapour at the boiling-point of the mixture. Ten years ago I showed that the density of the vapour had to be taken into account (and Berthelot has also independently, but, I believe, subsequently, insisted on the necessity of regarding vapour-density in judging of relative volatility). The coefficient of volatility comprises at least the tension of the vapour and the density of the vapour.

The heat rendered latent during evaporation must also be represented in this coefficient. Possibly, too, adhesion between the liquids will require representation in certain classes of instances; and this will cause difficulty and complication. I am of opinion, however, that in the vast majority of cases the conjoined influences of tension, density, and latent heat will adequately express the coefficient of volatility. If t be put for the tension and d for the vapour-density, then I propose $v=td^2$ for the value of the coefficient of volatility. This will be more fully explained on a future occasion. At present my main object is to insist on the fact that, when distilling in mixture, different liquids have different coefficients of volatility, and that the composition of successive portions of distillate is governed by these coefficients and by the original proportions of the liquids present in the mixture, in the manner set forth.

I have made some experiments on mixtures of A and B wherein the quantity of A is overwhelmingly greater than that of B. This disposition of the quantities offers certain advantages for investigating the general march of fractional distillation.

When a is immense compared with b, $\frac{b}{a+b}$ is undistinguishable from $\frac{b}{a}$. And putting $v=1$, when a is immense compared with b we get $\frac{bv'}{av+bv'}$ undistinguishable from $\frac{bv'}{a}$. I have made four experiments, wherein

	a + b
Exp. I. . .	1000000 + 1000
Exp. II. . .	1000000 + 1·00
Exp. III. . .	1000000 + 0·50
Exp. IV. . .	1000000 + 0·20

In these cases the quantity of b in a unit of the original mixture is $\frac{b}{a+b}$ undistinguishable from $\frac{b}{a}$; viz.

$$\text{Exp. I.} \quad \frac{1000}{1001000} \text{ or } \frac{1000}{1000000}$$

$$\text{Exp. II.} \quad \frac{1}{1000001} \text{ or } \frac{1}{1000000}$$

$$\text{Exp. III.} \quad \frac{0{\cdot}50}{1000000{\cdot}5} \text{ or } \frac{0{\cdot}50}{1000000}$$

$$\text{Exp. IV.} \quad \frac{0{\cdot}20}{1000000{\cdot}2} \text{ or } \frac{0{\cdot}20}{1000000}$$

In like manner in these instances the quantity of B in a unit of distillate is expressed indifferently either by $\frac{bv'}{av+bv'}$ or by $\frac{bv'}{a}$. Now, if it were possible to distil off and work with an infinitesimally small quantity of distillate, we should get $\frac{bv'}{a}$ for the value of B in a unit of distillate; whence the relation of the value of B in a unit of original liquid to the value of B in a unit of distillate is very simple, viz. $\frac{b}{a}:\frac{b}{a}v'$. Although we cannot distil off and work with an infinitely small fraction of distillate, yet we can, in a series of experiments, distil off and work with equal fractions (which will be equal numbers of the infinitesimal fractions). In each of the four experiments I took a kilogramme of A and B, and I distilled off 50 grammes (or one twentieth of the whole). The results were as follows:—

A = water. B = ammonia.

	Quantity of ammonia contained in a litre of original liquid.	Quantity of ammonia contained in the first 50 cubic. centims of distillate.
	milligrms.	milligrms.
Exp. I. . . .	1000	480
Exp. II. . .	1·00	0·50
Exp. III. . .	0·50	0·235
Exp. IV. . .	0·20	0·095

Now on inspection it will be manifest that, within the limit of experimental error, the strength of the distillate is proportional to the strength of the original liquid. In each of these four experiments 1 litre of liquid was taken and 50 cubic centims. distilled off; that is to say, one twentieth of the whole was distilled. If instead of 50 cubic centims. I distil off 100 cubic centims., I also find that the strength of the distillate is proportional to the strength of the original liquid; and it is a fact that in the case of dilute solutions of ammonia, if the same fraction be distilled off, the strength of that fraction is proportional to the strength of the original liquid. I cannot but regard this as a result of extreme importance, and, bearing in mind the immense range over which it holds good, consider that it affords proof that the general march of fractional distillation is as has been represented.

Putting $v=1$, let us endeavour to make out the value of v' from the four experiments. Inasmuch as the quantity of distillate was 50 cubic centims., and the quantity of original liquid was 1 litre, we must multiply the ammonia in the distillate by 20 in order that it may be comparable with that in the original liquid. Having done this, we see that the distillate is 9·6 times as strong as the original liquid. If the 50 cubic centims. were an infinitesimal fraction of distillate, then v' would equal 9·6; but the 50 cubic centims. is so large a fraction, that during the evolution of it the liquid has gone down in strength from 1000 milligrammes of ammonia per litre to about 547 milligrammes per litre. During the distillation of this 50 cubic centims. the strength of the successive portions of distillate must have varied so that the ratio of the strength of the first infinitesimal to the strength of the last infinitesimal shall be 1000 : 520. The real value of v' is therefore between 13 and 14.

It is my intention to make determinations of the "coefficients of volatility" of different liquids.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 76.]

June 13, 1872.—Sir John Lubbock, Bart., Vice-President, in the Chair.

THE following communication was read:—

"On the Spectrum of the Great Nebula in Orion, and on the Motions of some Stars towards or from the Earth." By William Huggins, LL.D., D.C.L., F.R.S.

In my early observations of the spectrum presented by the gaseous nebulæ, the spectroscope with which I determined the coincidence of two of the bright lines respectively with a line of nitrogen and a line of hydrogen was of insufficient dispersive power to show whether the brightest nebular line was double, as is the case with the corresponding line of nitrogen.

Subsequently I took some pains to determine this important point by using a spectroscope of greater dispersive power. I found, however, that the light furnished by the telescope of eight inches aperture, to which the spectroscope was attached, was too feeble, even in the case of the brightest nebulæ, to give the line with sufficient distinctness when a narrow slit was used. The results of this later examination are given in a paper I had the honour of presenting to the Royal Society in 1868. I there say*:—"I expected that I might discover a duplicity in the line in the nebula corresponding to the two component lines of the line of nitrogen; but I was not able, after long and careful scrutiny, to see the line double. The line in the nebula was narrower than the double line of nitrogen; this latter line may have appeared broader in consequence of irradiation, as it was much brighter than the line in the nebula." When the spark was placed before the object-glass of the telescope, the light was so much weakened that one line only was visible in the spectroscope. "This line was the one which agrees in position with the line in the nebula, so that under these circumstances the spectrum of nitrogen appeared precisely similar to the spectra of those nebulæ of which the light is apparently monochromatic. This resemblance was made more complete by the faintness of the line; from which cause it appeared narrower, and the separate existence of its two components could no longer be detected. When the line was observed simultaneously with that in the nebula, it was found to appear but a very little broader than that line." I also remark:—"The double line in the nitrogen-spectrum does not consist of sharply defined lines, but each component is nebulous, and remains of a greater width than the image of the slit. The breadth of these lines appears to be connected with the conditions of tension and temperature of the gas. Plücker† states that when an induction-spark of great heating-power is employed, the lines expand so as to unite and form an undivided band. Even when the duplicity exists, the eye ceases to have the power to distinguish the component lines, if the intensity of the light be greatly diminished." I state further:—"I incline to the belief that it [the line in the nebula] is not double."

One of the first investigations which I proposed to myself when, by the kindness of the Royal Society, I had at my command a much more powerful telescope, was the determination of the true character of the bright line in the spectra of the nebulæ which is apparently coincident with that of nitrogen. From various circumstances, chiefly connected with the alterations and adjustments of new instruments, I was not able to accomplish this task satisfactorily until within the last few months.

Description of Apparatus.

It seems to me desirable to give a description of the spectroscopic apparatus with which the observations in this paper were made. In the former paper, to which I have already referred, I gave some reasons‡ to show that the ordinary method of comparison, by reflecting light into the spectroscope by means of a small prism placed before one half of the slit, is not satisfactory for very delicate observations unless certain precautions are taken. I then describe an

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[*] Phil. Trans. 1868, pp. 542, 543.

[†] Ibid. 1865, p. 13.

[‡] Ibid. 1868, pp. 537, 538.

arrangement for this purpose, which, with one or two modifications, is adopted in the collimator constructed for use with the Royal Society's telescope. I give the description from that paper* :—

"The following arrangement for admitting the light from the spark appeared to me to be free from the objections which have been referred to, and to be in all respects adapted to meet the requirements of the case. In place of the small prism, two pieces of silvered glass were securely fixed before the slit at an angle of 45°. In a direction at right angles to that of the slit, an opening of about $\frac{1}{10}$ inch was left between the pieces of glass for the passage of the pencils from the object-glass. By means of this arrangement the spectrum of a star is seen accompanied by two spectra of comparison, one appearing above and the other below it. As the reflecting surfaces are about 0·5 inch from the slit, and the rays from the spark are divergent, the light reflected from the pieces of glass will have encroached upon the pencils from the object-glass by the time they reach the slit, and the upper and lower spectra of comparison will appear to overlap to a small extent the spectrum formed by the light from the object-glass. This condition of things is of great assistance to the eye in forming a judgment as to the absolute coincidence or otherwise of lines. For the purpose of avoiding some inconveniences which would arise from glass of the ordinary thickness, pieces of the thin glass used for the covers of microscopic objects were carefully selected; and these were silvered by floating them upon the surface of a silvering solution. In order to ensure that the induction-spark should always preserve the same position relatively to the mirror, a piece of sheet gutta percha was fixed above the silvered glass; in the plate of gutta percha, at the proper place, a small hole was made of about $\frac{1}{20}$ inch diameter. The ebonite clamp containing the electrodes is so fixed as to permit the point of separation of these to be adjusted exactly over the small hole in the gutta percha. The adjustment of the parts of the apparatus was made by closing the end of the adapting-tube, by which the apparatus is attached to the telescope, with a diaphragm with a small central hole, before which a spirit-lamp was placed. When the lines from the induction-spark, in the two spectra of comparison, were seen to overlap exactly, for a short distance, the lines of sodium from the light of the lamp, the adjustment was considered perfect. The accuracy of adjustment has been confirmed by the exact coincidence of the three lines of magnesium with the component lines of *b* in the spectrum of the moon."

The modifications of this plan consist in the substitution of a thin silver plate polished on both surfaces for the pieces of silvered glass. The opposite side of the silver plate to that from which the terrestrial light is reflected to the slit reflects the images formed by the object-glass to the side of the tube where a suitable eyepiece is fixed. This arrangement forms a very convenient finder; for it is easy to cause the image of the star to disappear in the hole in the silver plate. When this is the case, the line of light formed by the star falls on the slit, and its spectrum is visible in the spectroscope. This collimator is so constructed that, by means of a coupling-screw, any one of three spectroscopes can be conveniently attached to it.

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* Phil. Trans. 1868, p. 538.

This apparatus performs admirably; but it seemed to me desirable, for observations of great delicacy, to be able to dispense with reflection, and to place the source of the light for comparison directly before the slit. Formerly I accomplished this object by placing the spark or vacuum-tube before the object-glass of the telescope. The great length of the present telescope renders this method inconvenient; but a more important objection arises from the great diminution of the light when the spark is removed to a distance of 15 feet from the slit. I therefore resolved to place the spark or vacuum-tube within the telescope at a moderate distance from the slit. For this purpose holes were drilled in the tube opposite to each other, at a distance of 2 feet 6 inches within the principal focus. Before these holes short tubes were fixed with screws; in these tubes slide suitable holders for carrying electrodes or vacuum-tubes. The spark is thus brought at once nearly into the axis of the telescope. The final adjustment is made in the following manner:—A bright star is brought into the centre of the field of an ordinary eyepiece; the eyepiece is then pushed within the focus, when the wires or vacuum-tube can be seen across the circle of light formed by the star out of focus. The place of discharge between the electrodes, or the middle of the capillary part of the vacuum tube, is then brought into the centre of the circle of light. The vacuum-tubes are covered with black paper, with the exception of a space about a ¼ inch long in the middle of the capillary part; through this small uncovered space the light passes to reach the slit.

The accuracy of both methods of comparison, that by reflection and that by the spark within the tube, was tested by the comparison of the three bright lines of magnesium and the double line of sodium with the Fraunhofer lines *b* and D in the spectrum of the moon. I greatly prefer the latter method, because it is free from several delicate adjustments which are necessary when the light is reflected and which are liable to be accidentally displaced.

Spectroscope A is furnished with a single prism of dense glass with a refracting angle of 59° 42′, giving 5° 6′ from A to H.

Spectroscope B has two compound prisms of Mr. Grubb's construction, which move automatically to positions of minimum deviation for the different parts of the spectrum. Each prism gives about 9° 6′ for minimum deviation from A to H.

Spectroscope C is furnished with four similar prisms.

The small telescopes of the three spectroscopes are of the same size: diameter of object-glass 1¼ inch; each is furnished with three eyepieces magnifying 5·5, 9·2, and 16·0 diameters.

Spectrum of the Nebula of Orion.

With spectroscopes A and B four* lines are seen; they are represented in the diagram which accompanies this note. The scale in the diagram gives wave-lengths.

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* The fourth line was first seen in nebula 18 H. IV. (Phil. Trans. 1864, p. 441).

First line.—With spectroscope B and eyepiece 1 and 2, the slit being made very narrow, this line was seen to be very narrow, of a width corresponding to the slit, and defined at both edges, and undoubtedly not double. The line of nitrogen when compared with it appeared double, and each component nebulous and broader than the line of the nebula. This latter line was seen on several nights to be apparently coincident with the middle of the less refrangible line of the double line of nitrogen. This observation was on one night confirmed by observation with the more powerful spectroscope C.

The question suggests itself whether, under any conditions of pressure and temperature, the double line of the nitrogen-spectrum becomes single; and further, if this should be found to be the case, whether the line becomes single by the fading out of its more refrangible component, or in what other way the single line of the nebula comes to occupy in the spectrum, not the position of the middle of the double line of nitrogen, but that of the less refrangible of the lines.

I stated in my former paper that when for any reason the light from the luminous nitrogen is greatly reduced in intensity, the double line under consideration is the last to disappear, and consequently a state of things may be found in which the light of nitrogen is sensibly monochromatic when examined with a narrow slit*. Under these circumstances the line of nitrogen appears narrower, and the separate components can be detected with difficulty, if at all.

I stated also that the breadth of the component lines appears to be connected with the conditions of density and temperature of the gas. As was to be expected from theoretical considerations, the lines become narrower and less nebulous as the pressure is diminished. My observations of this change seemed to show that the diminution of the breadth of the lines takes place chiefly at the outer sides of the lines; so that in the light from very rarefied gas the double line is narrower, but the space of separation between the components is not as much wider as would be the case if the lines had decreased equally in width on the sides towards each other.

When the pressure of the gas is reduced to about 15 inches of mercury, the line-spectrum fades out to give place to Plücker's spectrum of the first order. During this process a state of things occurs when, for reasons already stated, the spectrum becomes *sensibly* monochromatic when viewed with a narrow slit and a spectroscope of several prisms. The line is narrower but remains double, and has the characters described in the preceding paragraph.

As the pressure is diminished, the double line fades out entirely, and the spectrum of the second order gives place to the spectrum of the first order. When, however, the pressure becomes exceedingly small, from 0·1 inch to 0·05 inch, or less, of mercury, there is a condition of the discharge in which the line again appears, while the other lines remain very faint. Under these conditions I have always been able, though with some difficulty, on account of the faint light when the necessary dispersive power (spectroscope B with second or third eyepiece) and a narrow slit are used, to see the line to be double; but it is narrower than when the gas is more dense, and may be easily mistaken for a single line. I have not yet been able to find a condition of luminous nitrogen in which the line has the same characters as those presented by the line in the nebula, where it is single and of the width of the slit.

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* Phil. Trans. 1868, pp. 540–546. Observations on this point were subsequently made by Frankland and Lockyer (Proc. Roy. Soc. vol. xvii. p. 453). It should be stated that the authors make no reference to this observation, though they refer to a purely hypothetical suggestion contained in the same paper.

Upon the whole I am still inclined to regard the line in the nebula as probably due to nitrogen.

If this should be found to be the case, and that the nebular line has originally the refrangibility of the middle of the double line of nitrogen, then we should have evidence that the nebula is moving from the earth. The amount of displacement of the nebular line from the middle of the double line of nitrogen corresponds to a velocity of 55 miles per second from the earth. At the time of observation the part of the earth's orbital motion, which was from the nebula, was 14·9 miles per second. From the remaining 40 miles per second would have to be deducted the probable motion from the nebula due to the motion of the solar system in space. This estimation of the possible motion of the nebula can be regarded as only approximate.

If the want of accordance of the line in the nebula with the middle of the double line of nitrogen be due to a recession of the nebula in the line of sight, there should be a corresponding displacement of the third line as compared with that of hydrogen. For reasons which will be found in a subsequent paragraph, I have not been able to make this comparison with the necessary accuracy.

In my former paper* I gave reasons against supposing so large a motion in the nebula; these were based on the circumstance that the nebular line falls upon the double nitrogen line, which the present observations confirm. I was not then able to use a slit sufficiently narrow to show that the nebular line is single and not coincident with the middle of the double line of nitrogen.

I am still pursuing the investigation of the parts of this inquiry which remain unsettled.

Second line.—This line was found by my former comparisons to be a little less refrangible than a strong line in the spectrum of barium. Three sets of measures give for this line a wave-length of 4957 on Ångström's scale; this would show that the line agrees nearly in position with a strong line of iron. At present I am not able to suggest to what substance this line belongs.

This line is also narrow and defined. I suspect that the brightness of this line relatively to the first line varies in different nebulæ.

Third and fourth line.—My former observations show that these lines agree in position with two lines of the spectrum of hydrogen, that at F and the line near G.

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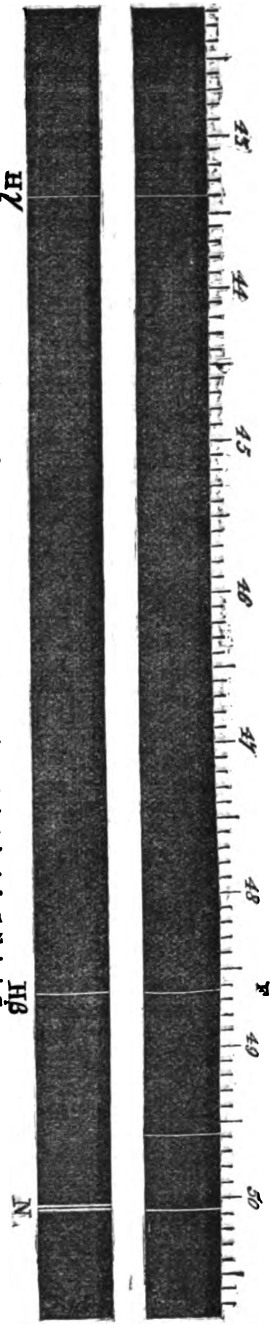
* Phil. Trans. 1868, pp. 542, 543.

These lines are very narrow and are defined; the hydrogen therefore must be at a low tension.

The brightness of these lines relatively to the first and second lines varies considerably in different nebulæ; and I suspect they may also vary in the same nebulæ at different times, and even in different parts of same nebula; but at present I have not sufficient evidence on these points*. I regret that, in consequence of a continuance of bad weather, I have not yet been able to obtain decisive observations as to the possible motion of the nebula in the line of sight. With spectroscope B and eyepiece 2, the lines appear to be coincident with those of hydrogen. In consequence of the uncertainty of the character of the first line, which is single, while that of nitrogen is double, this determination can now only be made by means of the comparison of the third line with that of hydrogen. This third line becomes very faint from the great loss of light unavoidable in a spectroscope that gives a sufficient dispersive power, and the comparison can only be attempted when the sky is very clear and the nebula near the meridian.



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* Since writing this sentence, I have seen a note by Prof. D'Arrest in the 'Astronomische Nachrichten,' No. 1885. Speaking of the nebula H. IV. 37, he says:—"Sein Spectrum ist ausser von Huggins bisher nur noch von Dr. H. Vogel untersucht worden. In No. 1864, Ast. Nachr. theilt letzterer mit, dass er im Jahre 1871, im Widerspruch mit Huggins' Angabe, die Linie Neb. $(3)=(2)$, bisweilen sogar $(2)<(3)$ gefunden habe. Nach Huggins war dagegen im Jahre 1864 positiv $(2)>(3)$. Ist Vogel's Beobachtung, wie ich nicht bezweifle, zuverlässig, so wird seine Vermuthung einer Veränderung hier in der That begründet sein, denn diesen Winter, namentlich im Februar und März 1872, fand ich wiederum, so wie es Huggins früher gesehen hat, unzweifelhaft $(2)>(3)$. Die relative Intensität der drei Lichtarten habe ich mehrfach in Zahlen geschätzt und erhielt z. B. in den letzten Nächten:

	März 6.	März 13.
(1)	100	100
(2)	58	63
(3)	49	52"

§ 2. *On the Motions of some Stars towards or from the Earth.*

In the early part of 1868 I had the honour of presenting to the Royal Society some observations on a small change of refrangibility which I had observed in a line in the spectrum of Sirius as compared with a line of hydrogen, from which it appeared that the star was moving from the earth with a velocity of about twenty-five miles per second, if the probable advance of the sun in space be taken into account *.

It is only within the last few months that I have found myself in possession of the necessary instrumental means to resume this inquiry, and since this time the prevalence of bad weather has left but few nights sufficiently fine for these delicate observations.

Some time was occupied in obtaining a perfectly trustworthy method of comparison of the spectra of stars with those of terrestrial substances, and it was not until I had arranged the spark within the tube, as described at the beginning of this note, that I felt confidence in the results of my observations.

It may be well to state some circumstances connected with these comparisons which necessarily make the numerical estimations given further on less accurate than I could wish. Even when spectroscope C, containing four compound prisms, and a magnifying-power of 16 diameters are used, the amount of the change of refrangibility to be observed appears very small. The probable error of these estimations is therefore large, as a shift corresponding to five miles per second (about $\frac{1}{70}$ of the distance of D^1 to D^2), or even a somewhat greater velocity, could not be certainly observed. The difficulty arising from the apparent smallness of the change of refrangibility is greatly increased by some other circumstances. The star's light is faint when a narrow slit is used; and the lines, except on very fine nights, cannot be steadily seen, in consequence of the movements in our atmosphere. Further, when the slit is narrow, the clock's motion is not uniform enough to keep the spectrum steadily in view; for these reasons I found it necessary to adopt the method of estimation by comparing the shift with a wire of known thickness, or with the interval between a pair of close lines. I found that, under the circumstances, the use of a micrometer would have given the appearance only of greater accuracy. I wish it, therefore, to be understood that I regard the following estimations as provisional only, as I hope, by means of apparatus now being constructed, to be able to get more accurate determinations of the velocity of the motions.

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* Phil. Trans. 1868, pp. 529–550. As a curious instance in which later methods of observation have been partially anticipated, a reference may be made to an ingenious paper in the Philosophical Transactions for 1783, vol. lxxiv., by the Rev. John Michell, entitled "On the means of discovering the Distance, Magnitude, &c. of the Fixed Stars, in consequence of the Diminution of the Velocity of their Light." The author suggests that by the use of a prism "we might be able to discover diminutions in the velocity of light as perhaps a hundredth, a two hundredth, a five hundredth, or even a thousandth part of the whole." But he then goes on to reason on the production of this diminished velocity by the attraction produced on the material particles of light by the matter of the stars, and that the diminutions stated above would be "occasioned by spheres whose diameter should be to the sun, provided they were of the same density, in the several proportions of 70, 50, 30, and 22 to 1 respectively."

Sirius.—The comparison of the line at F with the corresponding line of hydrogen was made on several nights from January 18 to March 5. Spectroscope C and eyepieces 2 and 3 were used. These observations confirm the conclusion arrived at in my former paper, that the star is moving from the earth; but they ascribe to the star a velocity smaller than that which I then obtained.

These observations on different days show a change of refrangibility corresponding to a velocity of from 26 miles to 36 miles per second. The part of the earth's orbital motion from the star varied on these days from 10 miles to 14 miles per second. We may take, therefore, 18 to 22 miles per second as due to the star.

The difference of this estimate, which is probably below rather than in excess of the true amount, from that which I formerly made may be due in part or entirely to the less perfect instruments then at my command. At the same time, if Sirius be moving in an elliptic orbit, as suggested by Dr. Peters, that part of the star's proper motion which is in the direction of the visual ray would constantly vary[*].

Betelgeux (*α Orionis*).—In the early observations of Dr. Miller and myself on this star, we found that there are no strong lines coincident with the hydrogen lines at C and F. The line H α falls on the less refrangible side of a small group of strong lines, and H β occurs in the space between two groups of strong lines where the lines are faint. On one night of unusual steadiness of the air, when the finer lines in the star's spectrum were seen with more than ordinary distinctness, I was able with the more powerful instruments now at my command to see a narrow defined line in the red apparently coincident with H α, and a similar line at the position of H β. These lines are much less intense than the lines C and F in the solar spectrum; there are certainly no bright lines in the star's spectrum at these places.

The most suitable lines in this star for comparison with terrestrial substances for ascertaining the star's motion are the lines of sodium and of magnesium. The double character of the one line agreeing exactly with that of sodium, and the further circumstance that the more refrangible of the lines is the stronger one, as is the case in spectrum of sodium and in the solar spectrum, and the relative distances from each other and comparative brightness of the three lines, which correspond precisely to the triple group of magnesium, can allow of no doubt that these lines in the star are really produced by the vapours of these substances existing there, and that we may therefore safely take any small displacement of either set of lines to show a motion of the star towards or from the earth. The lines due to sodium are perhaps more intense, but are as narrow and defined as the lines D_1, D_2 in the solar spectrum; they fall, however, within a group of very fine lines; this circumstance may possibly account for the nebulous character which has been assigned to them by some observers.

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[*] Dr. H. Vogel at Bothkamp seems to have repeated my observations on Sirius with the necessary care. He says (Astron. Nachr. No. 1864):—"Mit der eben beschriebenen Anordnung gelang es Herrn Dr. Lohse und mir am 22. März (1871) bei ganz vorzüglicher Luft die Nichtcoincidenz der drei Wasserstofflinien H α, H β, und H γ, der Geissler'schen Röhre mit den entsprechenden Linien des Siriusspectrum zu sehen ...... mit Berücksichtigung der Geschwindigkeit der Erde zur Zeit der Beobachtung berechnet sich die Geschwindigkeit mit welcher sich Sirius von der Erde bewegt zu 10·0 Meilen in der Secunde, wogegen Procyon sich 13·8 Meilen in der Secunde von unserer Erde entfernen würde."

The bright lines of sodium were compared with spectroscope B and eyepiece 3; they appeared to fall very slightly above the pair in the star, showing that the stellar lines have been degraded by the star's motion from the earth. The amount of displacement was estimated at about one fifth of the distance of D_1 from D_2, which is probably rather smaller than the true amount. This estimation would give a velocity of separation of 37 miles per second. At the time of observation the earth was moving from the star at about 15 miles per second, leaving 29 miles to be due to the star.

When magnesium was compared, a shift in the same direction, and corresponding in extent to about the same velocity of recession, was observed; but in consequence of other lines in the star at this place, the former estimation, based on the displacement of the lines of sodium, was considered to be more satisfactory.

Rigel.—The lines of hydrogen are strong in the spectrum of this star, and are suitable for comparison.

The line $H\beta$ is not so broad as it appears in the spectrum of Sirius, but is stronger than F in the solar spectrum: this line was compared by means of spectroscope C and eyepieces 2 and 3. The line of terrestrial hydrogen falls above the middle of the line in the star; the star is therefore receding from the earth. The velocity of recession may be estimated as rather smaller than Sirius, probably about 30 miles per second, the earth at the time of observation moving from the star with a velocity of 15 miles, leaving about 15 miles as due to the star. This estimate is probably rather smaller than the true velocity of the star.

Castor.—The spectra of the two component stars of this double star blend in the spectroscope into one spectrum. The line $H\beta$ is rather broad, nearly as much so as the same line in the spectrum of Sirius.

The narrow line of rarefied hydrogen was compared in spectroscope B with eyepiece 3; it appeared to fall on the more refrangible side of the middle of the line in the star, leaving more of the dark line on the side towards the red. The shift seemed to be rather greater than that in Sirius, and may probably be taken at from 40 to 45 miles per second; but the earth's orbital motion was nearly 17 miles from the star, thus leaving about 25 miles for the apparent velocity of the star. This result rests at present on observations on one night only, but they seemed at the time to be satisfactory.

Regulus.—The line at F rather broad. The corresponding line of hydrogen falls on the more refrangible side of the middle of the dark line in the star. The air was unfavourable on all the evenings of comparison; a rough estimate gives a velocity of from 30 to 35 per second. The earth's motion was 18 miles, leaving from 12 to 17 miles for the velocity of recession between the star and the sun.

β and δ Leonis.—These stars were compared with hydrogen; they appear to be moving from the earth, but the want of steadiness in the air prevented me from making a satisfactory estimate of their velocity. I suspected their motion to be rather smaller than that of Regulus.

β, γ, δ, ε, ζ Ursæ majoris.—All these stars have similar spectra, in which the line F is strong, though there are small differences in the breadth of the line. They were compared with hydrogen, and appear to be moving from our system with about the same velocity. Probably their motion may be taken to be not far from 30 miles per second. The earth's motion at the time of observation was from 9 miles to 13 miles from these stars, leaving a probable velocity of recession of 17 to 29 miles per second. In the case of the double star ζ, the spectrum consisted of the light of both stars.

η Ursæ majoris was also compared with hydrogen. I believe it shows a motion from the earth; but the observations of this star are at present less satisfactory.

α Virginis and α Coronæ borealis.—These stars were compared with hydrogen. I suspect that they are receding, but I have not had nights sufficiently fine to enable me to make satisfactory observations of these stars.

In addition to these stars some observations (which are less satisfactory on account of the unfavourable state of the weather at the time) appear to show that the stars Procyon, Capella, and possibly Aldebaran are moving from the earth.

The stars which follow have a motion of approach.

Arcturus.—In the spectrum of this star the lines of hydrogen, of magnesium, and of sodium are sufficiently distinct for comparison. I found the comparison could be most satisfactorily made with magnesium.

The bright lines of magnesium fall on the less refrangible side of the corresponding dark lines in the star's spectrum, showing that the star is approaching the earth. I estimated the shift at about $\frac{1}{3}$ to $\frac{1}{4}$ of the interval between Mg_2 and Mg_3; this amount of displacement would indicate a velocity of approach of 50 miles per second. To this velocity must be added the earth's orbital motion from the star of 5·25 miles per second, increasing the star's motion to 55 miles per second.

When I can get favourable weather, I hope to obtain independent estimations from the lines of sodium and of hydrogen.

α Lyræ.—In the spectrum of Vega the line corresponding to H β is strong and broad. Comparisons were made on several nights, but on one evening only was the air favourable. The observations are accordant in showing that the narrow bright line from a Geissler's tube falls on the less refrangible side of the middle of the line in the star, thus leaving more of the line on the side towards the violet. The estimations give a motion of approach between the earth and the star of from 40 to 50 miles per second, to which must be added 3·9 miles for the earth's motion from the star.

α Cygni.—The hydrogen line at F in the spectrum of this star is narrower than in the spectrum of Sirius and of *α* Lyræ, though probably rather broader than the same line in the solar spectrum. I have at present observations made on two evenings only, on both of which the state of the air was unfavourable for the comparison of this line with that of terrestrial hydrogen. They give to the star a motion of approach of about 30 miles per second, which would have to be increased by 9 miles, the velocity at the time of the earth from the star.

Pollux.—The lines of magnesium and those of sodium are very distinct in the spectrum of this star. As the air was not very steady at the time of my observations, I found it more satisfactory to use for comparison the lines of magnesium, which are rather stronger than those of sodium. The three lines of magnesium appeared to be less refrangible than the corresponding dark lines in the spectrum of the star by about one-sixth of the interval from Mg_2 to Mg_3. This estimation would represent a velocity of approach equal to about 32 miles per second. The earth's motion from the star was 17·5 miles, which increases the apparent velocity of approach to 49 miles per second. On one evening only was the air favourable enough for a numerical estimate, but the observations were entered in my observatory-book as satisfactory.

α Ursæ majoris.—The spectrum of this star is different from the spectra of the other bright stars of this constellation. The line at F is not so strong, while the lines at *b* are more distinct, and are sufficiently strong for comparison with the bright lines of magnesium. The bright lines of this metal fall on the less refrangible side of the dark lines, and show a motion of approach of from 35 to 50 miles per second. The earth's motion of 11·8 miles from the star must be added.

γ Leonis and ε Boötis.—In both these double stars the compound spectrum due to the light of both component stars was observed. Both stars are most conveniently compared with magnesium. I do not consider my observations of these stars as quite satisfactory, but they seem to show a movement of approach; but further observations are desirable.

The stars *γ Cygni*, *α Pegasi*, *γ Pegasi*, and *α Andromedæ* were compared with hydrogen on one night only. It is probable that these stars are approaching the earth, but I wish to reobserve them before any numerical estimate is given of their motion.

γ Cassiopeiæ.—On two nights I compared the bright lines which are present in its spectrum at C and F with the bright lines of terrestrial hydrogen. The coincidence appeared nearly perfect in spectroscope C with eyepieces 2 and 3; but on the night of best definition I suspected a minute displacement of the bright line towards the red when compared with H β. As the earth's orbital motion from the star at the time was very small, about 3·25 miles per second, which corresponds to a shift that could not be detected in the spectroscope, it seems probable that γ Cassiopeiæ has a small motion of recession.

In the calculation of the estimated velocities the wave-lengths employed are those given by Ångström in his 'Recherches sur le spectre solaire' (Upsal, 1868). The velocity of light was taken at 185,000 miles per second.

The velocities of approach and of recession which have been assigned to the stars in this paper represent the whole of the motion in the line of sight which exists between them and the sun. As we know that the sun is moving in space, a certain part of these observed velocities must be due to the solar motion. I have not attempted to make this correction, because, though the direction of the sun's motion seems to be satisfactorily ascertained, any estimate that can be made at present of the actual velocity with which he is advancing must rest upon suppositions, more or less arbitrary, of the average distance of stars of different magnitudes. It seems not improbable that this part of the stars' motions may be larger than would result from Otto Struve's calculations, which give, on the supposition that the average parallax of a star of the first magnitude is equal to $0''\cdot209$, a velocity but little greater than one fourth of the earth's annual motion in its orbit.

It will be observed that, speaking generally, the stars which the spectroscope shows to be moving from the earth (Sirius, Betelgeux, Rigel, Procyon) are situated in a part of the heavens opposite to Hercules, towards which the sun is advancing, while the stars in the neighbourhood of this region, as Arcturus, Vega, α Cygni, show a motion of approach. There are in the stars already observed exceptions to this general statement; and there are some other considerations which appear to show that the sun's motion in space is not the only, or even in all cases, as it may be found, the chief cause of the observed proper motions of the stars*.

There can be little doubt that in the observed stellar movements we have to do with two other independent motions—namely, a movement common to certain groups of stars, and also a motion peculiar to each star.

Mr. Proctor has brought to light strong evidence in favour of the drift of stars in groups having a community of motion, by his graphical investigation of the proper motions of all the stars in the catalogues of Mr. Main and Mr. Stone†. The probability of the stars being collected into systems was early suggested by Michell and the elder Herschel‡. One of the most remarkable instances pointed out by Mr. Proctor are the stars β, γ, δ, ε, ζ of the Great Bear, which have a community of proper motions*, while α and η of the same constellation have a proper motion in the opposite direction. Now, the spectroscopic observations show that the stars β, γ, δ, ε, ζ have also a common motion of recession, while the star α is approaching the earth. The star η, indeed, appears to be moving from us, but it is too far from α to be regarded as a companion to that star.

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* As the velocities assigned to the stars are, for reasons already stated, provisional only, I feel some hesitation in drawing from them the obvious conclusions which they would suggest. The velocities given in the Tables for those stars which are moving in direction in accordance with the sun's motion towards Hercules do not bear to each other the relation which they should have if they were mainly produced by the sun's motion. Even for these stars, therefore, we must look elsewhere for the cause to which they are chiefly due.

† See "Preliminary Paper on certain Drifting Motions of the Stars," Proc. Roy. Soc. vol. xviii. p. 169.

‡ Sir William Herschel writes:—"Mr. Michell's admirable idea of the stars

Although it was not to be expected that a concurrence would always be found between the proper motions which indicate the apparent motions at right angles to the line of sight and the radial motions as discovered by the spectroscope, still it is interesting to remark that in the case of the stars Castor and Pollux, one of which is approaching and the other receding, their proper motions also are different in direction and in amount—and further, that γ Leonis, which has an opposite radial motion to α and β of the same constellation, differs from these stars in the direction of its proper motion.

TABLE I.—Stars moving from Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion from sun.
Sirius	H	28 to 36	−10 to 14	18 to 22
Betelgeux	Na	37	−15	22
Rigel	H	30	−15	15
Castor	H	40 to 45	−17	23 to 28
Regulus	H	30 to 35	−18	12 to 17
β Ursæ majoris	H	30	− 9 to 13	17 to 21
γ " "	H			
δ " "	H			
ε " "	H			
ζ " "	H			
β Leonis	H			
δ Leonis	H			
η Ursæ majoris	H			
α Virginis	H			
α Coronæ borealis	H			
Procyon	H			
Capella	H			
Aldebaran?	Mg			
γ Cassiopeiæ	H			

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being collected into systems appears to be extremely well founded, and is every day more confirmed by observations, though this does not take away the probability of many stars being still as it were solitary, or, if I may use the expression, intersystematical ...... A star, or sun such as ours, may have a proper motion within its own system of stars, while at the same time the whole starry system to which it belongs may have another proper motion totally different in quantity and direction." Herschel further says, "And should there be found in any particular part of the heavens a concurrence of proper motions of quite a different direction, we shall then begin to form some conjectures which stars may possibly belong to ours, and which to other systems."—Phil. Trans. 1783, pp. 276, 277.

* Mr. Proctor, speaking of these stars, says:—"Their drift is, I think, most significant. If, in truth, the parallelism and equality of motion are to be regarded as accidental, the coincidence is one of most remarkable character. But such an interpretation can hardly be looked upon as admissible when we remember that the peculiarity is only one of a series of instances, some of which are scarcely less striking."—Other Worlds than Ours, p. 269. See paper in Proc. Roy. Soc. vol. xviii. p. 170.

It scarcely needs remark that the difference in breadth of the line Hβ in different stars affords us information of the difference of density of the gas by which the lines of absorption are produced. A discussion of the observations in reference to this point, and other considerations on the physical condition of the stars and nebulæ, I prefer to reserve for the present.

TABLE II.—Stars approaching the Sun.

Star.	Compared with	Apparent motion.	Earth's motion.	Motion towards sun.
Arcturus	Mg	50	+ 5	55
Vega	H	40 to 50	+ 3·9	44 to 54
α Cygni	H	30	+ 9	39
Pollux	Mg	32	+17	49
α Ursæ majoris	Mg	35 to 50	+11	46 to 60
γ Leonis	Mg			
ε Boötis	Mg			
γ Cygni	H			
α Pegasi	H			
γ Pegasi ?	H			
α Andromedæ	H			

December 12.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

"Researches in Spectrum Analysis in connexion with the Spectrum of the Sun."—No. I. By J. Norman Lockyer, F.R.S.

The author, after referring to the researches in which he has been engaged since January 1869 in conjunction with Dr. Frankland, refers to the evidence obtained by them as to the thickening and thinning of spectral lines by variations of pressure, and to the disappearance of certain lines when the method employed by them since 1869 is used. This method consists of throwing an image of the light-source to be examined on to the slit of the spectroscope.

It is pointed out that the phenomena observed are of the same nature as those already described by Stokes, W. A. Miller, Robinson, and Thalen, but that the application of this method enables them to be better studied, the metallic spectra being clearly separated from that of the gaseous medium through which the spark passes. Photographs of the spark, taken in air between zinc and cadmium and zinc and tin, accompany the paper, showing that when spectra of the vapours given off by electrodes are studied in this manner, the vapours close to the electrode give lines which disappear from the spectrum of the vapour at a greater distance from the electrode, so that there appear to be long and short lines in the spectrum.

The following elements have been mapped on this method:—Na, Li, Mg, Al, Mn, Co, Ni, Zn, Sr, Cd, Sn, Sb, Ba, and Pb, the lines being laid down from Thalen's maps, and the various characters and lengths of the lines shown.

In some cases the spectra of the metals, enclosed in tubes and subjected to a continually decreasing pressure, have been observed. In all these experiments the lines gradually disappear as the pressure is reduced, the *shortest lines disappearing first, and the longest lines remaining longest visible.*

Since it appeared that the purest and densest vapour alone gave the greatest number of lines, it became of interest to examine the spectra of compounds consisting of a metal combined with a non-metallic element. Experiments with chlorides are recorded. It was found in all cases that the difference between the spectrum of the chloride and the spectrum of the metal was that under the same spark-conditions all the short lines were obliterated. Changing the spark-conditions, the final result was that only the very longest lines in the spectrum of the metallic vapour remained. It was observed that in the case of elements with low atomic weights, combined with one equivalent of chlorine, the numbers of lines which remain in the chloride is large, 60 per cent., *e. g.*, in the case of Li and 40 per cent. in the case of Na; while in the case of elements with greater atomic weights, combined with two equivalents of chlorine, a much smaller number of lines remain—8 per cent. in the case of barium, and 3 per cent. in the case of Pb.

The application of these observations to the solar spectrum, to elucidate which they were undertaken, is then given.

It is well known that all the known lines of the metallic elements on the solar atmosphere are not reversed. Mr. Lockyer states what Kirchhoff and Ångström have written on this subject, and what substances, according to each, exist in the solar atmosphere. He next announces the discovery that, with no exception whatever, *the lines which are reversed are the longest lines.* With this additional key he does not hesitate to add, on the strength of a small number of lines reversed, zinc and aluminium (and possibly strontium) to the last list of solar elements given by Thalen, who rejected zinc from Kirchhoff's list, and agreed with him in rejecting aluminium. It need scarcely be added that these lines are in each case the longest lines in the spectrum of the metal.

The help which these determinations afford to the study of the various cyclical changes in the solar spectra is then referred to.

GEOLOGICAL SOCIETY.

[Continued from vol. xliv. p. 543.]

May 22, 1872.—Prof. Morris, V.P., in the Chair.

The following communication was read:—

"Some observations on the Upper Greensand Formation of Cambridge." By W. Johnston Sollas, Esq., Assoc. Royal School of Mines, London.

The author supposes that the coprolites are in all cases the result of the fossilization of organic matter, or of the intermediate products of its decomposition—a large number being simply fossil sponges, and the rest merely the phosphatization of animal matter so far decomposed as to have lost almost every trace of its original structure. He said that it had been suggested that the decomposition of small fish must have furnished the Gault with a large proportion of that phosphatic matter which saturated the last-deposited clay of the formation, and which served to fossilize the decaying organisms imbedded in it. He supposes that the roughly cylindrical forms having a core of white chalk-marl with an outer annular portion of coprolitic material are sponges. He said that he had obtained from the calcareous portions of this deposit numerous specimens of Foraminifera belonging to genera of which he gave a list. He thought that the green grains are casts of Foraminifera. He stated that he has obtained several glauconitic casts still coated with the shells of the Foraminifera in which they were formed. In conclusion the author pointed out the unreliable nature of the analyses made of the green grains.

June 5, 1872.—J. Gwyn Jeffreys, Esq., F.R.S., in the Chair.

The following communications were read:—

1. "Notes on Sand-pits, Mud-volcanoes, and Brine-pits met with during the Yarkand Expedition of 1870." By George Henderson M.D., F.L.S.

The author described some very remarkable circular pits which occurred chiefly in the valley of the Karakash river. These pits varied in diameter from 6 to 8 feet, and were between 2 and 3 feet deep, the distances between the pits being about the same as the diameters. He accounted for the formation of the pits by supposing that the water, which sinks into the gravel at the head of the valley, flows under a stratum of clay, which prevents it from rising; the water in course of time, however, flowing in very varying quantities at different periods, gradually washes away small portions of the clayey band, when the sand above runs through into the cavity thus formed, leaving the pits described by the author. The mud-volcanoes at Tarl Dab he accounted for by supposing that after a fall of rain or snow the air contained in the water-bearing stratum would get churned up with water and mud, and be ejected as a frothy mud, sometimes to a height of 3 feet; while the brine-pits in the Karakash valley he believed to be formed by the excessive rise and fall in the level of that river at various times, which alternately fills and empties the bottoms of the pits, and the water left in the pits gets gradually concentrated by evaporation until a strong brine remains.

2. "On the Cervidæ of the Forest-bed of Norfolk and Suffolk." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The author described a new form of *Cervus* from the Forest-bed of Norfolk, which he based on a series of antlers, and named *C. verticornis*. The base of the antler is set on the head very obliquely; immediately above it springs the cylindrical brow-tyne, which suddenly curves downwards and inwards; immediately above the brow-tyne the beam is more or less cylindrical, becoming gradually flattened. A third, flattened tyne springs on the anterior side of the beam; and immediately above it the broad crown terminates in two or more points. No tyne is thrown off on the posterior side of the antler, and the sweep is uninterrupted from the antler-base to the first point of the crown. The antlers differ in curvature and otherwise from those of *Cervus megaceros*; but there is a general resemblance between the two animals, and the *verticornis* must have rivalled the Irish Elk in size. A second species of Deer, the *Cervus carnutorum*, which had been furnished by the strata of St.-Prest near Chartres, must be added to the fauna of the Forest-bed. The Cervidæ of the Forest-bed present a remarkable mixture of forms, such as the *Cervus polignacus*, *C. Sedgwickii*, *C. megaceros*, *C. carnutorum*, *C. elaphus*, and *C. capreolus*, seeming to indicate that in classification the Forest-bed belongs rather to an early stage of the Pleistocene than to the Pliocene age. This inference is strongly corroborated by the presence of the Mammoth, which is so characteristic of the Pleistocene age.

3. "The Classification of the Pleistocene Strata of Britain and the Continent by means of the Mammalia." By W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.

The Pleistocene deposits may be divided into three groups:—1st, that in which the Pleistocene immigrants lived, with some of the southern and Pliocene animals in Britain, France, and Germany, and in which no arctic mammalia had arrived; 2nd, that in which the characteristic Pliocene Cervidæ had disappeared, and the *Elephas meridionalis* and *Rhinoceros etruscus* had been driven south; 3rd, that in which the true arctic mammalia were the chief inhabitants.

This third or late Pleistocene division must be far older than any Prehistoric deposits, as the latter often rest on the former, and are composed of different materials; but the difference offered by the fauna is the most striking. In the Pleistocene river-deposits twenty-eight species have been found, the remains of man being associated with the Lion, Hippopotamus, Mammoth, Wolf, and Reindeer. On examining the fauna from the ossiferous caves, we find the same group of animals, with the exception of the Musk-sheep; and it is therefore evident that the cave-fauna is identical with that of the river strata, and must be referred to the same period. Some few animals, however, which would naturally haunt caves, are peculiar to them, as the Cave-bear, Wild Cat, Leopard, &c.

The magnitude of the break in time between the Prehistoric and late Pleistocene period may be gathered also from the disappearance in the interval of no less than nineteen species.

The middle division of the Pleistocene mammalia, or that from which the Pliocene Cervidæ had disappeared and been replaced by invading temperate forms, is represented in Great Britain by the deposits of the Lower Brick-earths of the Thames valley, and the older deposits in Kent's Hole and Oreston. The discovery, by the Rev. O. Fisher, of a flint-flake in the undisturbed Lower Brick-earth at Crayford proves that man must have been living at this time.

The mammalia from these deposits are linked to the Pliocene by the *Rh. megarhinus*, and to the late Pleistocene by the *Ovibos moschatus*. The presence of *Machærodus latidens* in Kent's Hole, and of the *Rh. megarhinus* in the cave at Oreston, tends to the conclusion that some of the caves in the south of England contain a fauna that was living before the late Pleistocene age. The whole assemblage of middle Pleistocene animals evinces a less severe climate than in the late Pleistocene times.

The fossil bones from the Forest-bed of Norfolk and Suffolk show that in the early Pleistocene mammalia there was a great mixture of Pleistocene and Pliocene species. It is probable also that the period was one of long duration; for in it we find two animals which are unknown on the continent, implying that the lapse of time was sufficiently great to allow of the evolution of forms of animal life hitherto unknown, and which disappeared before the middle and late Pleistocene stages.

The author criticised M. Lartet's classification of the Late Pleistocene or Quaternary period by means of the Cave-bear, Mammoth, Reindeer, and Aurochs, and urged that, since the remains of all these animals were intimately associated in the caves of France, Germany, and Britain, and, so far as we know, the first two appeared and disappeared together and the last two lived on into the Prehistoric age, they did not afford a basis for a chronology.

The latest of the three divisions of the British Pleistocene fauna is widely spread through France, Germany, and Russia, from the English Channel to the shores of the Mediterranean. The Middle Pleistocene is represented by a river-deposit in Auvergne, and by a cave in the Jura, in which the presence of the *Machærodus latidens*, and a non-tichorine Rhinoceros, and the absence of the characteristic arctic group of the late Pleistocene and of all the peculiar animals of the early Forest-bed stage, prove that that era must be Middle Pleistocene. The Early Pleistocene division is represented in France by the river-deposit at Chartres, being characterized by the precence of two non-Pliocine animals, *Trogontherium* and *Cervus carnutorum*.

The Pleistocene mammalia of the regions south of the Alps and Pyrenees present no trace of truly arctic species, the Mammoth being viewed as an animal fitted for the climatal conditions both of Northern Siberia and of the southern states of America. It contains *Elephas africanus* and *Hyæna striata*.

The fauna of Sicily, Malta, and Crete differs considerably from that described above, possessing some peculiar forms, such as *Hippopotamus Pentlandi*, *Myoxus melitensis* and *Elephas melitensis*.

The Pleistocene mammalia may be divided into five groups, each marking a difference in the climate:—the first embracing those which now live in hot countries; the second those which inhabit northern regions, or high mountains, where the cold is severe; the third those which inhabit temperate regions; a fourth those which are found alike in hot and cold; and a fifth, which are extinct.

There were three climatal zones, marked by the varying range of the animals:—the northern, into which the southern forms never penetrated, the latitude of Yorkshire being the boundary of the advance of the southern animals; the southern, into which the northern species never passed, a line passing through the Alps and Pyrenees being the limit of the range of the northern animals; and an intermediate area, in which the two are found mingled together.

Two out of the three zones are proved by the physical evidence of the Pleistocene strata.

We see by the discoveries of Dr. Bryce, Mr. Jameson, and others, that the Pleistocene mammalia must have invaded Europe during the first Glacial period before the submergence; for the Reindeer and the Mammoth have been found in Scotland under the deposits of the Boulder-clay. Dr. Falconer and others have also discovered the latter animal in the preglacial Forest-bed. The Glacial period can therefore no longer be looked on as a hard and fast barrier separating one fauna from another. If man be treated as a Pleistocene animal, there is reason to believe that he formed one of the North Asiatic group, which was certainly in possession of Northern and Central Europe in Preglacial times.

The Pleistocene mammalia may again be divided into three groups—those which came from Northern and Central Asia, those from Africa, and those which were living in the same area in the Pliocene age. Had not the animals which lived in Europe, during the Pliocene age, been insulated from those which invaded Europe from Asia, by some impassable barrier, the latter would occur in our Pliocene strata as well as the former. Such a barrier is offered by the northern extension of the Caspian up the valley of the Obi to the Arctic Sea. The animals of Northern and Central Asia could not pass westwards until the barrier was removed by the elevation of the sea-bottom between the Caspian and the Urals.

The same argument holds good as to the African mammalia, which could not have passed into Sicily, Spain, or Britain without a northward extension of the African mainland.

The relation of the Pleistocene to the Pliocene fauna is a question of great difficulty. If the Pliocene fauna be compared with that of the Forest-bed, it will be seen that the difference between them is very great. The Pliocene Mastodon and Tapir, and most of the Cervidæ, are replaced by forms such as the Roe and Red Deer, unknown until then; but many of the Pliocene animals were able to hold their ground against the Pleistocene invaders, although they were ultimately beaten in the struggle for existence by the new comers. The fauna which the author adopted as typically Pliocene is that furnished by the lacustrine strata of Auvergne, the marine sands of Montpellier, and the older fluviatile strata of the Val d'Arno.

XIX. *Intelligence and Miscellaneous Articles.*

THE INVENTION OF THE WATER AIR-PUMP. BY H. SPRENGEL.

[*Statement*[*].

A LETTER addressed to me by Dr. Sprengel under date of November 1st, 1872, in which he says, "Perhaps it will not have escaped your observation, that the invention of the water air-pump, which you have constructed after the principle of my mercury air-pump, according to your paper published in 1868 on the Washing of Precipitates, is almost everywhere attributed to you," induces me to make the following statement.

The interesting discovery, that by means of columns of liquids flowing downwards a more perfect vacuum can be produced than was possible by the air-pumps hitherto in use, belongs solely and only to Dr. Sprengel. He in his researches on the Vacuum (Journal of the Chemical Society, January 1865), brings prominently forwards that water is, from a practical point of view, the only liquid which could come into consideration as a substitute for mercury, used in the instrument described by him, and that it is not unlikely that such an instrument, adapted for water, might possess advantages which air-pumps of other constructions have not, particularly in hilly countries, where the large volume of a natural waterfall might be rendered available. In the theoretical considerations on the action of his instrument, which immediately follow the above, it is noticed that it is simply the reverse of the trompe, with this addition, that the supply of air is limited, while that in the trompe is unlimited.

If in the face of these facts, which are open to all, any one attributes to me, as I must conclude from Dr. Sprengel's letter, a share in his discovery, I can regret this only all the more keenly, as in my treatise on the new method of filtration I could not possibly have expressed myself with regard to Dr. Sprengel's claims more loyally and precisely than I have done. There I have stated expressly that I have constructed the pump, used for filtrations and described by me in detail, after the principle of Sprengel's mercury air-pump. It was the only apparatus of the kind which Dr. Sprengel described, consequently the one to which alone I could refer.

Heidelberg, November 5, 1872. (Signed) R. BUNSEN.]

Expressing my best thanks to Professor Bunsen for the above statement, I beg to add that since 1860 I have been using for laboratory purposes a water-trompe, as described by me in Poggendorff's *Annalen* for 1861 (vol. cxii.), which (by reversing the action) led me in 1863 to the new method of air-rarefaction. Water was the first liquid which I used in my first pump, constructed during the summer of 1863. But the fallacies arising from the tension of aqueous vapour and from the air absorbed in water, as well as the inconvenience of having to provide for the requisite fall, caused me to discontinue the use of water, and to substitute in its stead mercury as the most suitable liquid for establishing *the* truth, which I had recognized by means of a water air-pump with an insufficient fall. My paper of 1865 was written with reference to *all* liquids; in fact on p. 15 (rendered prominent by italics) I summed up thus:—

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[*] Translated from *Ann. Chem. Pharm.* vol. clxv. p. 159, by H. Sprengel, authorized by Professor Bunsen.

"The main fact which I have established in this paper may be shortly stated to be, *that if a liquid be allowed to run down a tube, to the upper part of which a receiver is attached by means of a lateral tube, and if the height at which the receiver is attached be not less than that of the column of the liquid which can be supported by the atmospheric pressure, a vacuum will be formed in the receiver,* minus *the tension of the liquid employed.*"

I regret that the obviousness of the matter led me to refrain from expressing myself in a more detailed manner, believing, as I still believe, that what I wrote sufficiently described the construction of the water air-pump.

In conclusion, Mr. Johnson's aspirator* for establishing a current of air ought to be mentioned here. It was recognized by Professor Hofmann† to act on the principle of the trompe, and, of course, might have served as an air-pump, had it been noticed at the time that the instrument would furnish the means of creating a vacuum. And I may also draw attention to the tube‡ of a vacuum-pan, through which the water is made to escape, which has served to condense the steam of the boiling liquid. This no doubt would in like manner have served as a complete water air-pump; but it does not appear that its use as such was discovered.

London, January 22, 1873.

REPORT ON THE RESEARCHES OF M. ARN. THENARD CONCERNING THE ACTIONS OF ELECTRIC DISCHARGES UPON GASES AND VAPOURS. BY EDM. BECQUEREL.

The publications of M. Arnould Thenard which the Academy commissioned us to examine, have reference to the decomposition effects produced by electric discharges on gases and vapours, especially carbonic acid.

The effects due to the action of the electric spark on compound gases are very complex; for if, on the one hand, decomposition may take place, on the other the separated elements, if they remain gaseous, tend to reconstitute the primitive compound. The final result, therefore, after an action of a certain duration, must differ according to whether one of the elements separated is solid, liquid, or gaseous at the surrounding temperature, and must depend on the more or less elevated temperature produced by the passage of the spark, as well as the recompositions which may be effected in the vicinity of the latter. M. Thenard placed himself in conditions such that the calorific action extended only the least distance possible around the electrified points. Instead of sparks bursting forth in a eudiometric tube, he made use of the electrical effluvium—that is to say, more or less obscure discharges produced from place to place among the gaseous particles themselves. For this purpose he had recourse to the simple and ingenious arrangement of apparatus devised by M. Houzeau for the production of ozone, as the conditions necessary to that allotropic transformation of oxygen appeared similar to those which he proposed to utilize. This arrangement permitted him, besides, to submit to the electric influence successively and in distinct portions any volumes whatever of gas or vapour.

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* Quarterly Journal of the Chemical Society, vol. iv. p. 186 (1852).

† *Vide* the same paper.

‡ Elements of Physics, by Neil Arnott, M.D. Longmans. 3rd edition, London, 1828.

Several important additions to and modifications of this mode of experimentation have been made by M. Arnould Thenard, and very carefully studied for the purpose of ascertaining the most favourable conditions for the production of ozone as well as for the decomposition of carbonic acid. His observations led him to perceive that it is preferable to produce the electric effluvium between smooth surfaces of glass instead of between metallic conductors. He likewise saw that the action of electricity disaggregates glass at its surface, covering it with a fine powder which ends by transforming little by little the effluvium into sparks—that is to say, gives to the discharge a form which not only does not produce the effects of the effluvium, but may even destroy them. By removing this powder the efficacious action of the smooth tubes is reestablished. In certain circumstances which he indicates, electrochemical deposits in the tubes may give rise to the same effects.

His researches relate particularly to carbonic acid, the partial decomposition of which has, from the end of the last century, been the subject of several investigations on account of the action of the spark upon this gas being opposite to its action upon a mixture of carbonic oxide and oxygen, the two latter gases being capable of reconstituting carbonic acid in a eudiometer. He has ascertained that, with a very gentle current of carbonic acid circulating in the special apparatus he makes use of, decomposition into carbonic oxide and oxygen may reach 26·5 per cent. of its volume; while, as De Saussure observed, if we operate by means of sparks, it does not exceed 7·5 per cent.

He likewise shows that the preceding mixtures, containing as much as 26·5 per cent. of decomposed carbonic acid, revert in the gaseous state to 7·5 per cent. in the eudiometer—the greatest elevation of temperature due to the sparks in the latter experimental conditions doubtless not rendering possible, as shown by M. Berthelot's experiments, an explosive mixture of carbonic oxide and oxygen in greater proportions than these. We will mention also that the oxygen proceeding from the decomposition of carbonic acid in the apparatus in question was sensibly ozonized. It must be remarked that the decomposition of carbonic acid by electricity in these circumstances is effected at apparently a very low temperature, and that this seems to be the first time that an approximation has been made to conditions analogous to those of the decomposition of that gas by green leaves under the influence of the light of the sun.

It would be desirable that these new experiments, which have been made with much care, and the principal results of which we have been able to verify, should be extended to other gases and vapours—the electric intensities being varied between wider limits, as well as the surrounding temperature and the velocity of the gaseous currents.

Physico-chemical researches directed to the modifications caused by electricity in simple substances and in compounds present great scientific interest; for they may elucidate the question, still so obscure, of the allotropy of simple bodies, and may lead to the explanation of the decomposition undergone by certain compounds in the organism.—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxv. pp. 1735–1737.

GREAT BAROMETRIC DEPRESSION OF JANUARY.

On the 16th of January, 1873, a fall of the barometer of no ordinary magnitude set in at several stations in the north-west of Europe. This fall became more general on the 17th, was fully established, except at Biarritz, on the 18th, and continued with such rapidity that at sixteen stations the fall exceeded 1 inch of mercury between 8 A.M. of the 18th and 8 A.M. of the 19th. In the evening of this day a violent gale from the south-west accompanied this rapid fall. Except at a few stations in France, the minimum reading (below 29 inches) occurred on the 20th. The following readings at Thurso, in the north of Scotland, will show the extent of the entire fall; while those at Toulon will indicate the extensive area over which the fall occurred.

Thurso.	in.	
Jan. 16.	29·89	0·42
17.	29·47	0·39
18.	29·08	0·95*
19.	28·13	0·02
20.	28·11	
Total fall	....	1·78

Toulon.	in.	
Jan. 16.	30·36	0·16
17.	30·20	0·17
18.	30·03	0·08
19.	29·95	0·60*
20.	29·35	0·05
21.	29·30	
Total fall	....	1·06

The corresponding portion of the great fall occurred a day later at Toulon than at Thurso, as shown by the asterisks.

It is noteworthy that in 1872 a somewhat similar depression occurred on January 17–18. The recorded minima were as follows:—Kew, 28·90; Falmouth, 28·75; Valencia, 28·50; Aberdeen, 28·09; Glasgow, 28·08; Stonyhurst, 28·07; Armagh, 28·06. In 1871 a similar depression occurred on January 15–16, the minima being as follows:—Kew, 28·878; Falmouth, 28·769; Stonyhurst, 28·592; Aberdeen, 28·411; Glasgow, 28·313; Valencia, 28·198; Armagh, 28·176. In 1870 a depression occurred on January 14, minima as follows:—Kew, 29·40; Falmouth, 29·30; Valencia, 29·15; Aberdeen, 29·00; Glasgow, 28·85; Stonyhurst, 28·80; Armagh, 28·75. In 1869 a depression occurred on January 14–15, minima as follows:—Kew, 29·55; Aberdeen, 29·50; Glasgow, 29·25; Falmouth, 29·15; Armagh, 29·05; Stonyhurst, 29·05; Valencia, 28·85. These depressions taking place so nearly at the same epoch in five consecutive years, appear to indicate that about this time in January there is a tendency to a great reduction of pressure over Western Europe.

The above may be interesting at the present time, especially as the last great depression is so recent.

January 24, 1873. W. R. Birt.

ON THE THERMAL EFFECTS OF MAGNETIZATION. BY J. MOUTIER.

The experiments of MM. Jamin and Roger have shown that the intermittent passage of a current in the wire of an electromagnet produces heat: heat is developed at the interruption of the circuit; it is due to the vanishing of the temporary magnetism of the electromagnet. M. Cazin has lately announced[*], after new experiments, that the heat thus produced is *proportional to the square of the intensity of the magnetism and to the polar distance.* I have sought to account for this simple law by theoretical considerations.

M. Clausius[†] has demonstrated the following theorem relative to the stationary motion of a system of points—that is to say, a motion in which the position and the velocity of each point do not continually change in one and the same direction, but remain comprised within certain limits:—*The mean* vis viva *of the system is equal to its virial.* The virial, which in questions of mechanics plays a part analogous to that of the potential, is, as is known, half the sum of the products obtained by multiplying the distance between any two points of the system by the force which acts between those two points.

This theorem conducts to peculiar consequences in the case of magnetization. Let us consider a lengthened bar of soft iron, and suppose magnetism developed by placing the bar in the centre of a coil which is traversed by a current.

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[*] *Comptes Rendus de l'Acad. des Sciences,* vol. lxxv. p. 1265.

[†] *Comptes Rendus,* vol. lxx. p. 1314; Phil. Mag. S. 4. vol. xl. p. 122.

This magnet may be regarded as consisting of an infinity of magnetic elements of constant and infinitely little thickness. The quantity of magnetism Y developed in each of the elements varies with its distance x from one of the extremities of the bar, and may be represented by $Y=\phi(x)$.

The quantity of free magnetism upon the element dx of the bar is the difference of the values of Y at the points which have for abscissæ x and $x+dx$; so that the quantity of free magnetism at the point situated at the distance x from the extremity of the bar is $y=\frac{dY}{dx}=\phi'(x)$. The quantity of free magnetism in one point is therefore proportional to the quantity of magnetism of the bar.

Supposing the bar composed of two symmetrical parts, let us consider one of them in particular. Starting from the extremity, the free magnetism diminishes till at a certain distance λ it becomes sensibly *nil*; beyond, the function ϕ retains a sensibly constant value $\phi(\lambda)$.

The pole of this portion of the bar is the centre of a system of parallel forces proportional to the quantities of free magnetism; the distance x from the pole to the extremity of the bar is determined by the theorem of the moments,

$$X\int_0^\lambda ydx=\int_0^\lambda xydx.$$

If we suppose the bar sufficiently long, so that the magnetism developed at the extremity may be neglected, we find easily

$$X\phi(\lambda)=\lambda\phi(\lambda)-\int_0^\lambda\phi(x)dx.$$

The magnetizing has given rise to attractive forces between the several magnetic elements, or, according to Ampère's theory, between the parallel currents circulating in the solenoid formed by the magnet. The element whose abscissa is x is solicited by forces exerted by the elements around it, forces proportional to the quantities of magnetism of the acting elements, and of which the intensity rapidly decreases in proportion as the distance augments.

The increment of the virial, relative to the point under consideration, which results from the magnetizing may be represented by $\mu\phi(x)$,—μ denoting a function of the distance, being at the same time proportional to the quantity of magnetism developed in the bar, and consequently to the free magnetism. Besides, $\phi(x)$ is a function proportional to the quantity of free magnetism of the bar; consequently every term $\mu\phi(x)$ of the virial is proportional to the square of that quantity.

Designating by l half the length of the bar, the increment of the virial which results from the magnetizing is, for the half of the bar,

$$\int_0^l\mu\phi(x)dx=\int_0^\lambda\mu\phi(x)dx+\int_\lambda^l\mu\phi(x)dx.$$

Remarking that $\phi(x)$ preserves the constant value $\phi(\lambda)$ in the interval from l to λ, and also taking account of the equation which determines the position of the pole, we find, lastly,

$$\int_0^l \mu\phi(x)dx=\mu\phi(\lambda)(l-X).$$

The increase of *vis viva* produced in the bar by the effect of the magnetizing is therefore proportional to the square of the intensity of the magnetism and to the polar distance. The effect of demagnetizing corresponds to an equal loss of *vis viva*—which is the measure of the thermal effect produced, if that effect is the only one which accompanies the demagnetization.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxv. pp. 1619–1621.

ENCKE'S COMET.

Appendix II. of the 'Washington Observations' for 1870 contains a very interesting report by Professors Hall and Harkness on observations of Encke's comet during its return in 1871. The report of Professor Hall is confined almost exclusively to the micrometrical observations for position made with the filar micrometer of the 9½-inch equatorial of the Naval Observatory. In the closing portion Professor Hall describes the appearances of the comet from the evening it was found, October 11, to December 7. The report is accompanied by four exquisite engravings, representing the comet as seen on October 17, November 17, December 1, and December 2. A note is added on the general fact of a condensation of the matter of the comet and formation of a nucleus as it approaches to, and an expansion of the matter and a disappearance of the nucleus as it recedes from, the sun.

Professor Harkness enters elaborately into the spectroscopic observations of the comet, and describes the instruments employed. The spectrum consisted of three bright bands of the following wave-lengths: 549·5, 510·6, and 455. By a process of interpolation, Professor Harkness finds that the wave-lengths of comet II. 1868, observed by Dr. Huggins, are so nearly identical with the above as to lead to the conclusion that the physical constitutions of the two comets are identical, and that both are composed of incandescent carbon in a gaseous state.

The investigation of the density of the supposed resisting medium in space is exceedingly interesting. Professor Harkness deduces the density in terms of the height in inches of the column of mercury which it will support, and finds that it is somewhere between $\frac{220}{10^{17}}$ and $\frac{285}{10^{20}}$ of an inch, which is "enormously greater than that of the atmosphere at the upper limit of auroras." Hence the probability that auroras are propagated in a medium which pervades all space, and that the spectrum of the aurora is in reality the spectrum of that medium.

Professor Harkness calls attention to the continual increase in the wave-length of the light emitted by the brightest part of the second band of the spectrum as a phenomenon hitherto unobserved. He refers it to an increase of the temperature of the comet as it approached the sun.

ON THE INTENSITY OF SOUND AND LIGHT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Glenville, Fermoy, Jan. 6, 1873.

The subject of Mr. Moon's paper (Phil. Mag. vol. xlv. p. 38) deserves consideration *physically* as well as mathematically. The formula for the intensity must necessarily embrace two factors, viz. one representing the *number* of waves (of a given length), and the other giving the *amplitude of the vibration* of the particle of æther under consideration in each particular wave.

In regard to the former, the Astronomer Royal's assumption as to intensity of light from two candles (supposed perfectly alike) is obviously legitimate; at the same time I conceive (Mr. Moon's view) that the "amplitude of the vibration" (*not* its square) truly represents the other factor of the intensity formula.

This point can be easily tested experimentally as regards *sound*. Thus a tense string with amplitude of vibration $=1$ ought to become inaudible at twice the distance at which it ceases to be heard with amplitude $=0{\cdot}70715$, if the simple power of the amplitude (not its *square*) be the correct assumption.

As regards the mathematical formula for the displacement of a particle of the æther in a wave, viz. $y=a\sin\frac{2\pi}{\lambda}(vt-x)$, I believe a represents the distance of the disturbed particle from its place of rest (not *necessarily*, therefore, the "*maximum vibration*," as stated by the Astronomer Royal, 'Undulatory Theory,' p. 7), and consequently that for an undulation comprising several waves of the same period but varying amplitudes, the formula should be

$$(a+a'+a''\ \&\text{c.})\sin\frac{2\pi}{\lambda}(vt-x), \text{ not } a^2\sin\frac{2\pi}{\lambda}(vt-x).$$

Of course when there are waves of *different periods*, the expressions for each undulation (or series of such waves) must be kept distinct, and be added together to obtain the intensity.

HENRY HUDSON, M.D., M.R.I.A.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1873.

XX. *On the Optics of Mirage. By* Professor EVERETT, *M.A., D.C.L., Queen's College, Belfast*[*].

I PROPOSE in the present paper to investigate some of the principles which govern the formation of images in a medium of continuously varying index of refraction, with special reference to the phenomena of mirage and atmospheric refraction.

I. I shall first establish the following proposition:—

In a medium in which the absolute index of refraction varies continuously, the path of a ray will in general be curved; and its curvature $\frac{1}{\rho}$ at any point is given by the formula

$$\frac{1}{\rho}=\frac{1}{\mu}\frac{d\mu}{dN}=\frac{d\log\mu}{dN},$$

N denoting distance measured along the normal towards the centre of curvature, and therefore $\frac{d}{dN}$ denoting rate of increase in the direction of the centre of curvature.

The simplest proof of this important proposition is derived from the principle that μ varies inversely as v the velocity of light.

Draw normal planes to a ray at two consecutive points of its path. Then the distance of their intersection from either point will be ρ, the radius of curvature. But these normal planes are tangential to the wave-front in its two consecutive positions.

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[*] Communicated by the Author.

Hence it is easily shown by similar triangles that a very short line dN drawn from either of the points towards the centre of curvature, is to the whole length ρ of which it forms part, as dv the difference of the velocities of light at its two ends is to v the velocity at either end. That is,

$$\frac{d\mathrm{N}}{\rho}=-\frac{dv}{v},$$

the negative sign being used because the velocity diminishes in approaching the centre of curvature. But, since v varies inversely as μ, we have

$$-\frac{dv}{v}=\frac{d\mu}{\mu}.$$

Hence the curvature $\frac{1}{\rho}$ is given by any one of the four following expressions:—

$$\frac{1}{\rho}=-\frac{1}{v}\frac{dv}{d\mathrm{N}}=-\frac{d\log v}{d\mathrm{N}}=\frac{1}{\mu}\frac{d\mu}{d\mathrm{N}}=\frac{d\log\mu}{d\mathrm{N}}. \quad . \quad . \quad (\mathrm{A})$$

This proof is due to Professor James Thomson, who gave it in a paper to Section A at the recent Meeting of the British Association at Brighton.

It is obvious that the osculating plane is the plane which contains the direction of most rapid increase of index at the point considered, and that it cuts at right angles the surface-of-equal-index drawn through the point.

Cor. 1. The curvatures of different rays at the same point are directly as the rates of increase of μ in travelling along their respective normals. If θ denote the angle which any ray makes with the surface-of-equal-index at the point, or which the radius of curvature of a ray makes with the direction of most rapid increase of index, the curvatures will be directly as the values of $\cos\theta$. In fact, if $\frac{d\mu}{dr}$ denote the rate at which μ increases in a direction normal to the surfaces-of-equal-index, we have

$$\frac{d\mu}{d\mathrm{N}}=\frac{d\mu}{dr}\cos\theta,$$

and therefore

$$\frac{1}{\rho}=\frac{1}{\mu}\frac{d\mu}{dr}\cos\theta=\frac{d\log\mu}{dr}\cos\theta. \quad . \quad . \quad . \quad (\mathrm{B})$$

Cor. 2. When θ is small, and is regarded as a small quantity of the first order, $\cos\theta$, being approximately $1-\frac{1}{2}\theta^2$, differs from unity by a small quantity of the second order. Hence $\cos\theta$ is sensibly constant when θ is small, and rays which are but slightly inclined to the surfaces-of-equal-index have sensibly the same curvature at the same point.

II. I now proceed to the solution of the following problem:—

Suppose a medium in which the index μ is a function of distance from a certain plane of reference which is itself a plane of maximum index. It is clear from (I.) that rays cutting this plane at any angle except a right angle will be bent back towards it, and may, under proper conditions, meet it again. Required the condition that rays cutting it at any small angle θ shall meet it again at a constant distance—that is to say, at a distance sensibly independent of θ. This is obviously the condition that rays of small inclination diverging from a point in the plane of reference, and lying in one and the same perpendicular plane, shall converge to a focus in the plane of reference.

Take rectangular axes of x and y in the common perpendicular plane, the axis of x being in and the axis of y perpendicular to the plane of reference. Let s denote distance measured along a ray; then $\frac{dx}{ds}$ or $\cos\theta$ is sensibly equal to unity. Also to the same degree of approximation we have

$$\theta \doteq \tan\theta = \frac{dy}{dx},$$

$$\frac{1}{\rho} = -\frac{d\theta}{ds} = -\frac{d\theta}{dx} = -\frac{d^2y}{dx^2}. \quad \dots\dots \quad \text{(C)}$$

The problem which we have to solve has therefore been reduced to the following:—find what function $\frac{d^2y}{dx^2}$ must be of y that the increment of x from $y=0$ to the next occurrence of $y=0$ shall be independent of the maximum value of y. Mathematically considered, this is precisely the problem of finding what law of acceleration for a particle executing vibrations about a position of equilibrium will render the vibrations isochronous, y denoting the distance of the particle at time x from the position of equilibrium. Its solution is well known to be

$$\frac{d^2y}{dx^2} = -\frac{y}{a^2}, \quad \dots\dots\dots \quad \text{(D)}$$

a being a constant; and the required law of index is therefore

$$\frac{d\log\mu}{dy} = -\frac{y}{a^2}. \quad \dots\dots\dots \quad \text{(E)}$$

The general equation of a ray in the plane of x, y will be the general integral of (D), namely

$$y=b\sin\frac{x-c}{a}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (F)$$

representing a curve of sines cutting the axis of x at points whose distances from the origin are

$$c, \quad c+\pi a, \quad c+2\pi a, \ldots$$

The values of b and c vary from one ray to another; but a is the same for all; and hence the distance πa between two consecutive intersections of a ray with the axis of x is independent of the amplitude b. This constant quantity πa is evidently the focal length; and rays of small inclination diverging in the plane x, y from a point in the axis of x will converge to a series of foci at this constant distance apart. The same reasoning which proves that *all* small vibrations are isochronous, proves that wherever a plane of maximum index occurs, the other surfaces of equal index in its neighbourhood being also parallel planes, rays of small inclination diverging from a point in this plane must have a constant focal length; and it can be shown that the smallness of the aberration of rays from this geometrical focal length is especially promoted by symmetry of the medium about the plane of maximum index.

In fact, if we suppose $\log\mu$ and its differential coefficients with respect to y to be continuous, the assumption that the surfaces of equal index are parallel planes gives

$$\log\mu=A+By+Cy^2+Dy^3+\ldots,$$

whence

$$\frac{d\log\mu}{dy}=B+2Cy+3Dy^2+\ldots$$

The assumption that y is measured from a plane of maximum index gives

$$B=0, \quad 2C \text{ negative } =-\frac{1}{a^2} \text{ suppose.}$$

Hence, when y^2 is negligible, we have approximately

$$\frac{d\log\mu}{dy}=-\frac{y}{a^2},$$

which is identical with equation (E). The further supposition that the medium is symmetrical about the plane from which y is measured makes $D=0$, because no odd powers of y can enter the expression for $\log\mu$.

III. The investigation in (II.) related to rays emanating from a point in the plane of maximum index; but the law of index there deduced involves similar consequences for rays emanating from any other point; for the substitution of $x+\pi a$ for x in equation (F) merely changes the sign of y. Hence rays diverging from a point (x, y) will converge to a focus at the point $(x+\pi a, -y)$, then to another focus at the point $(x+2\pi a, y)$, and so on.

It thus appears that in a medium in which the law (E) prevails, every object will yield a series of real images, alternately inverted and erect, πa being their common distance asunder in a direction parallel to the plane of maximum index; while, as regards distances measured normal to the plane of maximum index, each image in the series corresponds to the reflected image of its predecessor with respect to this plane. It is of course to be understood that the images are formed in one dimension only, like those formed by a cylindrical lens.

I may remark incidentally that in a medium in which the surfaces-of-equal-index are parallel planes, if one ray of small inclination to these planes is a curve of sines, all rays of the same or less amplitude must also be curves of sines; for equation (D) cannot hold for one ray unless (E) holds for all distances from the plane of reference not exceeding the amplitude of that ray.

I may also remark that a prism-like or lens-like arrangement of surfaces-of-equal-index produces less deviation in rays than an arrangement in which these surfaces are approximately parallel to the course of the rays. This is obvious from equation (B), which shows that, for a constant rate of variation of $\log \mu$ normally to the surfaces-of-equal-index, the curvature is proportional to $\cos \theta$.

The fact that rays emanating from *any* point in the medium converge to a focus, and that the focal length measured parallel to the axis of x has a constant value, corresponds to the self-evident proposition in cycloidal oscillation, that the time from *any* point to the symmetrically situated point on the other side, when one of the extreme positions is taken in the interval, is the half-period of oscillation.

If, instead of supposing the surfaces-of-equal-index to be parallel planes, we suppose μ to be a function both of y and x, then $\frac{d^2y}{dx^2}$ will be a function both of y and x, and the analogous supposition in cycloidal oscillation is that of gravity varying with time. On this supposition, it is clear that if two particles start at the same instant with different velocities from the *lowest points* of two equal cycloids, they will keep time with each other, however great and sudden the variations of gravity may be supposed to be*, unless these variations be such as to compel one of the particles to travel beyond the cusp of its cycloid and thus introduce discontinuity.

As velocity in the case of the particle corresponds to $\frac{dy}{dx}$ in the case of a ray, the corresponding inference is that, in the medium now supposed, rays which proceed from the same point in the axis of x at different inclinations to this axis will meet it again at the same distance. The conclusion cannot be extended to points above or below the axis of x.

IV. When the surfaces-of-equal-index are parallel planes, the deviation of a ray in passing from one of these surfaces to another can be expressed in terms of the angle of incidence at the first surface and the relative index from the first surface to the last—being entirely independent of the distance between the two surfaces, and of the character of the intervening layers.

For, since the axis of y is perpendicular to the planes-of-equal-index, equation (B) becomes

$$\frac{1}{\rho}=-\frac{d\log\mu}{dy}\cos\theta.$$

Hence

$$d\theta=-\frac{ds}{\rho}=\frac{ds}{dy}\cos\theta d\log\mu=\frac{\cos\theta}{\sin\theta}d\log\mu, \quad . \quad \text{(G)}$$

or

$$d\log\mu=\frac{\sin\theta d\theta}{\cos\theta}=-d\log\cos\theta.$$

Integrating from μ_1, θ_1 at the first surface to μ_2, θ_2 at the last, we have

$$\frac{\mu_2}{\mu_1}=\frac{\cos\theta_1}{\cos\theta_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(H)}$$

When the change of index is abrupt, this equation amounts to a statement of the "law of sines;" for $\cos\theta_1$ is the sine of the angle of incidence, $\cos\theta_2$ is the sine of the angle of refraction, and $\frac{\mu_2}{\mu_1}$ is the relative index from the first medium into the second. Instead of integrating between limits, we might have deduced the general integral

$$\mu\cos\theta=\text{constant},$$

which applies to the whole course of the ray.

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* For such changes will not disturb the equalities, Ratio of accelerations = Ratio of velocities = Ratio of distances from vertex = Constant.

V. Putting $\theta_2=0$ in equation (H), we have

$$\frac{\mu_2}{\mu_1}=\cos\theta_1.$$

Hence a ray entering at any angle θ_1 to the parallel planes-of-equal-index will become parallel to these planes when it has penetrated as far as the plane in which

$$\mu=\mu_1\cos\theta_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(K)}$$

It will then be bent back symmetrically, and will emerge again from the plane at which it entered, making the angle of emergence equal to the angle of incidence. This result can only occur when the original course of the ray is from greater index to less.

When there are two regions of constant indices μ_1, μ_2 (μ_1 being the greater) separated by a region in which μ diminishes continuously from μ_1 to μ_2 (the surfaces-of-equal-index being parallel planes), a ray entering this intermediate region from the side where μ is greatest will be able to get through if $\cos\theta_1$ is less than $\frac{\mu_2}{\mu_1}$. But if $\cos\theta_1$ is greater than $\frac{\mu_2}{\mu_1}$, there will be a plane in the intermediate region in which equation (K) will be satisfied, and the ray will be returned from this plane. When the change of index is abrupt, the above statement resolves itself into the usual formula for the "critical angle" of total reflection.

VI. Thus far we have been supposing the surfaces-of-equal-index to be plane. If we now suppose them to be horizontal surfaces parallel to the general surface of the earth, it will be necessary to modify the conditions of (II.) by making the axis of x not a straight but a horizontal line, which, if we regard the earth as a sphere, will be a circular arc described about the earth's centre; while the ordinates denoted by y will not be parallel to any one line, but will be vertical, and will therefore be everywhere perpendicular to the axis of x.

Putting R for the earth's radius, equations (C) will now stand thus:—

$$\theta=\tan\theta=\frac{dy}{dx},$$

$$\frac{1}{\rho}=\frac{1}{\mathrm{R}}-\frac{d\theta}{ds}=\frac{1}{\mathrm{R}}-\frac{d\theta}{dx}=\frac{1}{\mathrm{R}}-\frac{d^2y}{dx^2};$$

whence, putting for $\frac{1}{\rho}$ its value $-\frac{d\log\mu}{dy}$, we have

$$\frac{d^2y}{dx^2}=\frac{1}{\mathrm{R}}+\frac{d\log\mu}{dy}.$$

To obtain the convergence which the problem requires, we must have

$$\frac{d^2y}{dx^2}=-\frac{y}{a^2}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(D)}$$

for the general equation of the curved path of a ray will then have the same form as before, namely

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad y=b\sin\frac{x-c}{a}; \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(F)}$$

and it is upon this form that the convergence which the problem requires alone depends. For the same reason, the consequences deduced in (III.) will remain completely applicable.

Equation (D) now denotes the curve obtained by making equal algebraic additions to the curvatures at all points of a curve of sines, or by bending uniformly a rectangular rod in the plane of one of its faces which has a curve of sines drawn upon it.

The required law of variation of index is

$$\frac{d\log\mu}{dy}=-\frac{y}{a^2}-\frac{1}{\text{R}}.$$

At the point where a ray cuts the axis of x we have

$$\frac{d\log\mu}{dy}=-\frac{1}{\text{R}}.$$

The axis of x, therefore, does not lie in the surface of maximum index, but in that surface-of-equal-index which possesses the property that rays cutting it at a small inclination have, at the points of section, a curvature equal to that of the earth. In consequence of this property, rays can travel for any distance along that surface-of-equal-index which contains the axis of x; whereas rays above it have greater curvature and bend down to meet it, while on the other hand rays below it are less curved (or may even be curved in the opposite direction), and it accordingly bends down to meet them.

To find the level of no curvature, or of maximum index, we must put $\frac{d\log\mu}{dy}=0$, an equation which gives

$$y=-\frac{a^2}{\text{R}}.$$

The depth of the surface of maximum index below the axis of x is therefore a third proportional to the earth's radius and the parameter a. Rays above this level are curved in the same direction as the earth; those below it are curved in the opposite direction. The value of $\frac{a^2}{\text{R}}$ is about five feet when the value of

a is about two miles, or when the focal length πa is about six miles.

VII. We now proceed to the consideration of the physical circumstances on which the variation of index in the atmosphere depends.

The experiments of Biot and Arago have shown that, for variations of density due either to change of temperature or change of pressure, $\mu-1$ varies directly as the density; and it further appears from the experiments of Jamin, that at ordinary temperatures the value of $\mu-1$ is sensibly the same for dry as for saturated air at the same density. If α denote the coefficient of expansion ·00366 or $\frac{1}{273}$, and h the pressure expressed in millimetres of mercury, the formula for $\mu-1$ is

$$\mu-1=\frac{\cdot 0002943}{1+\alpha t}\cdot\frac{h}{760};$$

and this may also be regarded as the value of $\log\mu$, since the difference $\frac{1}{2}(\mu-1)^2-$&c. is too small to be appreciable. Hence, for horizontal or nearly horizontal rays, the curvature $\frac{1}{\rho}$ or $-\frac{d\log\mu}{dy}$ at any point is

$$\frac{1}{\rho}=\frac{\cdot 0002943}{760}\left\{-\frac{dh}{dy}\,\frac{1}{1+\alpha t}+\frac{dt}{dy}\,\frac{h\alpha}{(1+\alpha t)^2}\right\}.$$

But $-\frac{dh}{dy}$, being the fall of the barometric column per unit of ascent, is equal to $\frac{h}{\mathrm{H}}$, where H denotes the height of the homogeneous atmosphere, which height, if we neglect variations of gravity, is $26200\,(1+\alpha t)$ feet. We have therefore, if we make the foot the unit of measurement for y and ρ,

$$\frac{1}{\rho}=\cdot 0002943\,.\,\frac{h}{760}\cdot\frac{1}{(1+\alpha t)^2}\left\{\frac{1}{26200}+\frac{1}{273}\,\frac{dt}{dy}\right\}$$

$$=\frac{1}{89000000}\cdot\frac{h}{760}\cdot\frac{1}{(1+\alpha t)^2}\left\{1+96\frac{dt}{dy}\right\}.$$

This expression vanishes when $\frac{dt}{dy}=-\frac{1}{96}$. Hence, when the diminution of temperature per foot of ascent is $\frac{1}{96}$ of a degree Centigrade, the density of the air is uniform and rays are straight. When the decrease of temperature upwards is more rapid than this, the upper air is the denser, and rays are bent upwards, in other words, their curvature is opposite to that of the earth.

The rate of decrease usually assumed as an average is $\frac{1}{300}$ of a degree Fahrenheit, or $\frac{1}{540}$ of a degree Cent. per foot. This will give, at 0° C. and 760 millims.,

$$\frac{1}{\rho}=\frac{1}{89000000}\left\{1-\frac{96}{540}\right\},$$

whence $\rho=108000000$ feet, or 5·2 radii of the earth.

At 10° C., 760 millims., and the same rate of decrease as above, ρ is equal to about 5·6 radii of the earth, and the correction for refraction in levelling is therefore $\frac{1}{5 \cdot 6}$ of the correction for the curvature of the earth. Rankine (Rules and Tables, p. 131) says, "The correction for refraction to be added to the reading is very variable and uncertain. On an average, it may be taken at one sixth of the correction for curvature."

In order that the curvature of a ray may be the same as that of the earth at 0° C. and 760 millims., the expression $1+96\,\frac{dt}{dy}$ must be equal to 89000000 feet divided by the earth's radius—that is, to about 4·26. We shall therefore have

$$\frac{dt}{dy}=\frac{3 \cdot 26}{96}=\frac{1}{29 \cdot 4};$$

that is, the temperature must *increase* at the rate of 1° C. for 29·4 feet of ascent, or 1° F. for 16·3 feet. Any portion of the earth with such a state of things prevailing over it will appear plane, distant objects being no longer hidden by the intervening convexity. A still more rapid increase will make the surface of the earth appear concave.

VIII. I shall now take up in detail the principal phenomena of mirage, and indicate what I conceive to be their correct explanations.

1. An unusual extension of the range of vision, like that described by Latham in the Philosophical Transactions for 1798, when the coast of France from Calais to the neighbourhood of Dieppe was clearly visible from Hastings.

Explanation.—An increase of temperature with ascent, producing an exaggeration of the ordinary downward curvature of rays, as explained in the preceding section.

2. Distant objects seen inverted above their true positions. Some instances of this form of mirage are described by Vince in the "Bakerian Lecture," Phil. Trans. 1799; many more are described by Scoresby from his observations in the Arctic regions; and the phenomenon is extremely common across extensive sheets of calm water. Usually two images are seen, namely an erect image in the true or what appears to be the true position, and an inverted image above it. Sometimes, however, the

inverted image is visible when the erect image is hidden by the convexity of the intervening water.

Explanation.—A very rapid increase of temperature upwards in a stratum of air overhead. The inverted images are formed by rays incident from below upon this stratum at such an obliquity that (as explained in V.) they cannot get through, but are compelled to descend again, and thus undergo a kind of reflection. The upward increase is supposed to be more sudden here than in (1), and confined to a thinner stratum.

3. Multiple images seen above the true position of the object.

Explanation.—Either several strata of rapid upward increase of temperature, each of them, as in (2), fulfilling the office of a mirror, or a single such stratum of irregular shape, yielding reflections in different places.

4. An appearance as of architectural columns, obelisks, spires, or basaltic cliffs. Such appearances are said to be common in the illusions of the *Fata Morgana* at the Straits of Messina; and many instances are described, with illustrative plates, in Scoresby's 'Greenland.' Such appearances are always due to the vertical magnification of real objects.

Explanation.—In the arrangements of III. and VI. an object at a short distance in front of or behind a focus conjugate to the position of the observer's eye, will be greatly magnified in the vertical direction, its vertical diameter being seen under the same angle as if the eye were at this conjugate focus. If between the eye and the first conjugate focus, it will appear erect; if between the first and the second conjugate focus, inverted. I believe that, when vertical magnification is exhibited with any thing like regularity, an arrangement approximately resembling that described in (VI.) prevails in the body of air which lies between the observer and the objects magnified.

The same appearance is often seen on land (small bushes, for example, being magnified into tall trees), and is to be similarly explained.

5. A false appearance of water in a place actually occupied by hot and dry ground.

Explanation.—An increase of temperature downwards, within a few inches of the ground, at a rate considerably exceeding $\frac{1}{96}$ of a degree Cent. per foot. This will produce an upward bending of rays and a *quasi* reflection, as in (2); but the seeming mirror is now below the observer's eye instead of above it. The flickering movements of the reflected images thus seen, due to currents of hotter and colder air, greatly resemble the appearances produced by the rippling of waves on a lake; but probably the most irresistible feature in the illusion is the gleam of the reflected sky. The sky itself, and its reflection in water, so far exceed in brightness all other objects in an ordinary landscape, that when this gleam is seen in a place where the sky cannot be, the observer feels irresistibly compelled to ascribe it to water.

IX. In the transmission of rays through a medium of continuously varying index, no proper distinction can be taken between refraction and reflection. They shade insensibly into one another; or rather, I should perhaps say, both names are equally inappropriate in this application.

X. The following are some of the mistakes which have frequently been made by writers on mirage:—

1. The mistake of supposing that a ray in air can be bent at an angle—in other words, can have a point of infinite curvature. This would imply an absolutely abrupt change of index.

2. The mistake of supposing that a ray can pursue a straight course parallel to planes of equal index in a continuously varying medium. The contrary was pointed out so long ago as 1799 and 1800 by Vince and Wollaston in the Philosophical Transactions, but appears to have since dropped out of mind.

3. The mistake of supposing that rays which first ascend and then descend, or which first descend and then ascend, must produce inverted images, or an appearance as of reflection. If all the rays of a system are circular arcs in vertical planes, with the same radius of curvature, and everywhere nearly horizontal, the images which they present to the eye will be neither inverted nor distorted, but simply elevated or depressed; for such a system can be converted into a system of straight rays by a process of bending which will not alter their distances apart. Let such of them as lie in one vertical plane be represented by a diagram drawn upon one face of a prismatic rod; then if the rod be bent in the plane of this face with a uniform curvature opposite to that of the lines of the diagram, all these lines will become straight; and it is clear that this process does not sensibly alter the distances between the lines, nor the angles at which they intersect each other.

Some further consequences of the law of ray-curvature, of theoretical rather than practical interest, are reserved for a second paper.

XXI. *Correction to a Paper "On an Experimental Determination of the Relation between the Energy and Apparent Intensity of Sounds of different Pitch." By* R. H. M. BOSANQUET, *Fellow of St. John's College, Oxford**.

IN the paper above referred to it was sought to establish directly, by experiment, the following relation:—"The apparent intensity of a musical note is proportional to the mechanical energy expended in the production of the tone, and inversely as the wave-length or periodic time." At the end of the paper certain deductions were made from this law, with reference to the relations connecting amplitude, pitch, and periodic time with apparent intensity. The writer has to acknowledge with regret that the reasoning of this latter portion is erroneous. The error does not seem to be very obvious, as no one has yet pointed it out, to the writer's knowledge. It was intended to wait until some measures could be completed by an improved method, and to make a more complete communication. But as the performance of this has been unavoidably delayed, and the writer finds the erroneous result quoted as correct by Dr. Mayer, in his interesting paper, Phil. Mag. Feb. 1873, it is desirable that the mistake should be corrected at once.

The error committed was precisely of the nature against which a warning was given on the last page of the paper. It was assumed that the energy of the vibration of the plate of air considered was identical with the total energy transferred through the plate. The assumption was simply a *non sequitur.* The following is believed to be correct.

To find the total energy of the sound which crosses a given plane section in one second, in terms of the amplitude and wave-length.

Let the sound consist of a succession of plane waves generated at one end of a straight cylindrical tube having the given section. The waves then traverse the cylinder without sensible diminution of intensity, if we neglect the influence of the walls. Let the delivery of the sound commence at the beginning of a second and continue throughout the second, and suppose that the condition of the air in the tube at the expiration of the second is photographed. Then the sum of the potential and kinetic energy of the disturbed air in the tube, through a length equal to the velocity of sound per second, is the energy supplied from the source in one second.

First for the potential energy. This consists partly of compressions and partly of dilatations, both of which are supplied from the source: the sum of their absolute numerical values is therefore to be taken.

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* Communicated by the Author.

Consider a disk of air of section unity and thickness dx; let this be compressed, the section remaining the same, and its thickness becoming $dx-dy$. Then the increment of pressure is $\frac{dy}{dx}$ × atmospheric pressure × thermometric and specific-heat corrections. The atmospheric pressure × corrections for heat $=v^2\rho$; where v^2 is the coefficient of the equation of the transmission of sound, and ρ the mean density of the atmosphere. Hence the pressure exerted by the compressed disk over and above the atmospheric pressure is $\frac{dy}{dx}\,.\,v^2\,.\,\rho$.

As the compression proceeds through the small distance δdy, an element of work is done

$$=\frac{dy}{dx}\,.\,v^2\,.\,\rho\,.\,\delta dy;$$

and the sum of all the work done in the compression from $dy=0$ up to $dy=dy$ is

$$\int_0^{dy}\frac{dy}{dx}\,.\,v^2\,.\,\rho\,.\,\delta dy=\frac{1}{2}\,\frac{dy^2}{dx}\,.\,v^2\rho.$$

If we now assume that the sound in the tube consists of a simple periodic vibration

$$y=a\sin\frac{2\pi}{\lambda}(vt-x),$$

we have for the work stored in any disk dx, at a distance x from the end of the tube,

$$\frac{v^2\rho}{2}\,\frac{dy^2}{dx^2}\,.\,dx=\frac{v^2\rho}{2}\left(\frac{2\pi a}{\lambda}\right)^2\cos^2\frac{2\pi}{\lambda}(vt-x)dx.$$

And the work stored in all the disks in a quarter wave-length, or, if P be average potential work in unit of length,

$$\text{P}\,.\,\frac{\lambda}{4}=\frac{v^2\rho}{2}\left(\frac{2\pi a}{\lambda}\right)^2\int_0^{\frac{\lambda}{4}}\cos^2\frac{2\pi}{\lambda}(vt-x)dx,$$

$$\text{,,}\qquad\text{,,}\qquad\int_0^{\frac{\lambda}{4}}\frac{1}{2}\left(1+\cos 2\,.\,\frac{2\pi}{\lambda}(vt-x)\right)dx,$$

$$\text{,,}\qquad\text{,,}\qquad\frac{1}{2}\left\{\frac{\lambda}{4}-\frac{\lambda}{4\pi}\left[\sin 2\,.\,\frac{2\pi}{\lambda}(vt-x)\right]_0^{\frac{\lambda}{4}}\right\}.$$

And if we take $vt-x=0$ at the lower limit,

$$P=\left(\frac{\pi . a . v}{\lambda}\right)^2 \rho.$$

And the potential work in the length v traversed by the sound in one second $=v^3\rho\left(\frac{\pi a}{\lambda}\right)^2$.

This is expressed in foot-pounds; for $v^2\rho$ is in terms of the atmospheric pressure over the unit section, v is a number of feet, and $\frac{a}{\lambda}$ a number.

We have now to estimate the kinetic energy of each disk of the photographed column.

Kinetic energy of disk dx

$$=\frac{1}{2}\left(\frac{dy}{dt}\right)^2\rho dx$$

$$=\frac{1}{2}v^2\rho\left(\frac{2\pi a}{\lambda}\right)^2\cos^2\frac{2\pi}{\lambda}(vt-x)dx,$$

which is identically the same expression we had before. We have therefore simply to double the above result; and we get for the total energy delivered in one second,

$$2\rho v^3\left(\frac{\pi a}{\lambda}\right)^2.$$

If we apply to this result the experimental law, that the apparent intensity is directly as the energy and inversely as the wave-length or periodic time, we have

$$I=\text{const.}\frac{a^2}{\lambda^3}.$$

And instead of the laws given in the writer's communication of last November, we have the following:—

The apparent intensities of sounds of different pitch are proportional to the squares of the amplitudes, and inversely as the cubes of the wave-lengths.

In sounds of the same apparent intensity, the squares of the amplitudes vary as the cubes of the wave-lengths.

XXII. *On the Effect of Internal Friction on Resonance.*

By J. Hopkinson, *D.Sc., B.A.*[*]

As a typical case which may be taken as illustrating the nature of the phenomena in more complex cases, let us consider the motion of a string, of a column of air, or an elastic rod vibrating longitudinally, one extremity being fixed, whilst the other is acted on so that its motion is expressed by a simple harmonic function of the time.

Let l be the length of the string, a the velocity with which a wave is transmitted along it, ξ the displacement of a point of the string distant x from the fixed extremity at the time t. In the hypothetical case, in which there is no friction, no resistance of a surrounding medium, and the displacements are indefinitely small, the equation of motion is

$$\frac{d^2\xi}{dt^2}=a^2\frac{d^2\xi}{dx^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

with the conditions that at the extremities $\xi=0$ when $x=0$, and $\xi=\mathrm{A}\sin nt$ when $x=l$, also that at some epoch ξ shall be a specified function of x.

If we start with the string straight and at rest, we have the condition $\xi=0$ for all values of x from zero to very near l when $t=0$, and we readily find

$$\xi=\frac{\mathrm{A}}{\sin\frac{nl}{a}}\sin nt\,.\sin\frac{nx}{a}+\Sigma\mathrm{C}_p\sin\frac{p\pi x}{l}\,.\sin\frac{p\pi at}{l}, \quad . \quad . \quad (2)$$

where $\mathrm{C}_p=(-1)^p\dfrac{2nal}{n^2l^2-p^2\pi^2a^2}$.

When $\frac{nl}{a}$ is very nearly a multiple of π (*i. e.* when the note sounded by the forcing vibration at the extremity is almost the same as one of the natural notes of the string), we have two notes sounded with intensity, viz. one the same as the forcing vibration, the other native to the string. That this is the case may be readily seen with a two-stringed monochord, the strings being nearly in unison: one string being sounded, the motion of the other is seen by the eye to be intermittent, the period of variation being the same as that of the beats of the two strings sounded together. But should $\frac{nl}{a}$ be an exact multiple of π, two terms in the value of ξ become infinite, and our whole method of solution is invalid. A somewhat similar difficulty, of course, occurs in the lunar and planetary theories, but with this difference: there the difficulty is introduced by the method of solving the differential equation, and is avoided by modifying the first approximation to a solution; here it is inherent in the differential equation, and can only be avoided by making that equation express more completely the physical circumstances of the motion. One or more of the assumptions on which the differential equation rests is invalid. We must look either to terms of higher orders of smallness, to resistance of the air, or to internal friction. With the modifications due to the last cause we are now concerned.

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[*] Communicated by the Author.

The approximate effect of internal friction is probably to add to the stress $E\frac{d\xi}{dx}$, produced by the strain $\frac{d\xi}{dx}$ when the parts of the body are relatively at rest, a term proportional to the rate at which the strain is changing; so that the stress when there is relative motion will be $E\left(\frac{d\xi}{dx}+k\frac{d^2\xi}{dx\,dt}\right)$, and our equation of motion becomes

$$\frac{d^2\xi}{dt^2}=a^2\left(\frac{d^2\xi}{dx^2}+k\frac{d^3\xi}{dx^2\,dt}\right). \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The solution of this equation will contain two classes of terms. First, a series corresponding to those under the sign of summation in (2), which principally differ from (2) in the coefficients decreasing in geometrical progression with the time, the highest fastest, and in the total absence of the notes above a certain order as periodic terms; these terms we may consider as wholly resulting from the initial conditions, and as having no permanent effect on the motion. Second, a term corresponding to the first term of (2), and which expresses the state of steady vibration when work enough is continually done by the forced vibration of the extremity to maintain a constant amplitude. The investigation of this term is a little more troublesome, because the motion is periodic, the effect of friction being to alter the motion in a manner dependent on the position of the point, not on the time, and equation (3) cannot be satisfied by a sine or a cosine alone of the time.

Assume $\xi=\phi(x)\sin mt+\psi(x)\cos mt,$

or a series of such terms, if possible, each pair satisfying equation (3). Substitute in the equations of motion, and equate coefficients of $\sin mt$ and $\cos mt$,

$$\left.\begin{aligned} a^2(\phi''-km\psi'')&=-m^2\phi,\\ a^2(\psi''+km\phi'')&=-m^2\psi.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (4)$$

Assume

$$\left.\begin{aligned}\phi &= c_1 \sin \lambda x,\\ \psi &= c_2 \sin \lambda x\end{aligned}\right\} \quad \dots\dots\dots \quad (5)$$

where c_1, c_2, and λ may be imaginary, but ϕ and ψ are real: this form is indicated as suitable, because ξ must change sign with x.

We obtain

$$\left.\begin{aligned}a^2(c_1 - c_2 km)\lambda^2 &= m^2 c_1,\\ a^2(c_2 + c_1 km)\lambda^2 &= m^2 c_2\ ;\end{aligned}\right\} \quad \dots\dots \quad (6)$$

whence

$$c_1^2 = -c_2^2, \quad c_2 = \pm c_1\sqrt{-1}\ ; \quad \dots \quad (7)$$

and

$$\lambda = \pm\mu\left(1 \pm \sqrt{-1}\tan\frac{\theta}{2}\right),$$

where $\tan\theta = km$,

$$\mu = \frac{m}{a}\frac{\cos\frac{\theta}{2}}{\sqrt[4]{1+k^2m^2}}.$$

The most general real expression for ϕ is then

$$\frac{A_1 + B_1\sqrt{-1}}{2}\sin\mu\left(1 + \sqrt{-1}\tan\frac{\theta}{2}\right)x$$

$$+\frac{A_1 - B_1\sqrt{-1}}{2}\sin\mu\left(1 - \sqrt{-1}\tan\frac{\theta}{2}\right)x\ ;$$

or, as it may be written,

$$\left.\begin{aligned}\phi &= A_1 \sin\mu x\,.\,\frac{\epsilon^{\mu\tan\frac{\theta}{2}.x} + \epsilon^{-\mu\tan\frac{\theta}{2}.x}}{2}\\ &\quad + B_1\cos\mu x\,.\,\frac{\epsilon^{\mu\tan\frac{\theta}{2}.x} - \epsilon^{-\mu\tan\frac{\theta}{2}.x}}{2}.\end{aligned}\right\} \quad \dots \quad (8)$$

Similarly

$$\begin{aligned}\psi &= A_2 \sin\mu x\,.\,\frac{\epsilon^{\mu\tan\frac{\theta}{2}.x} + \epsilon^{-\mu\tan\frac{\theta}{2}.x}}{2}\\ &\quad + B_2\cos\mu x\,.\,\frac{\epsilon^{\mu\tan\frac{\theta}{2}.x} - \epsilon^{-\mu\tan\frac{\theta}{2}.x}}{2}.\end{aligned}$$

The constants will be connected by the relations

$$\left\{\begin{aligned}A_1 + B_1\sqrt{-1} &= A_2\sqrt{-1} - B_2,\\ A_1 - B_1\sqrt{-1} &= -A_2\sqrt{-1} - B_2\ ;\end{aligned}\right.$$

that is,

$$A_1 = -B_2 \text{ and } B_1 = A_2. \quad \dots\dots \quad (9)$$

Let

$$\left.\begin{aligned} P &= \sin \mu l \, . \, \frac{\epsilon^{\mu \tan \frac{\theta}{2} . l} + \epsilon^{-\mu \tan \frac{\theta}{2} . l}}{2}, \\ Q &= \cos \mu l \, . \, \frac{\epsilon^{\mu \tan \frac{\theta}{2} . l} - \epsilon^{-\mu \tan \frac{\theta}{2} . l}}{2}. \end{aligned}\right\} \quad . \quad . \quad (10)$$

If possible, let m be other than n; when $x=l$, we have $\phi=0$ and $\psi=0$, or

$$\begin{cases} A_1P+B_1Q=0, \\ B_1P-A_1Q=0; \end{cases}$$

therefore, since A_1, B_1 must be real, they must vanish, and we conclude that the only steady vibration is of the same period as that impressed on the extremity.

Let $m=n$; when $x=l$, $\phi=A$ and $\psi=0$; hence

$$\left.\begin{aligned} PA_1+QB_1&=A, \\ PB_1-QA_1&=0; \end{aligned}\right\}$$

$$\left.\begin{aligned} A_1 &= \frac{AP}{P^2+Q^2}, \\ B_1 &= \frac{AQ}{P^2+Q^2}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This completely determines the steady vibration of the string.

Suppose a change to take place in the forcing vibration, it is easy to see that the result will be that momentarily all the notes natural to the string with both ends fixed will be sounded. This conclusion could readily be tested by graphically describing the motion of a point of a string moving in the manner supposed, the motion being produced by a tuning-fork actuated by an electromagnet. If this be verified, an attempt might be made to determine the value of k for various strings or wires by comparing the amplitude of vibration at the points of greatest and least vibration; and at the different points of least vibration true nodes will not occur. The curve having x for abscissa, and the maximum value of ξ at each point for ordinate, might possibly be portrayed by photographing a vibrating string. The calculations would be much facilitated by the fact that $\mu=\frac{n}{a}$ if small quantities of the second order are neglected. Suppose that $\mu l=2\pi$, a case of strong resonance; then $P=0$ and $Q=\pi kn$ very nearly; we have $A_1=0$ and $B_1=\frac{A}{\pi kn}$, and the motion is expressed by the equation

$$\xi = \frac{A}{\pi k n}\left\{\frac{kn^2x}{a}\cos\frac{nx}{a}\sin nt + \sin\frac{nx}{a}\cos nt\right\}.$$

Let the amplitudes observed at the node and middle of ventral segments of the string be α, β; we have

$$\left.\begin{aligned}\alpha &= \frac{Anl}{2\pi a},\\ \beta &= \frac{A}{\pi k n};\end{aligned}\right\} \quad \text{. . . . . (12)}$$

therefore

$$k = \frac{2\alpha}{\beta}\frac{a}{n^2 l} = \frac{\alpha}{\beta}\frac{1}{\pi n},$$

the result being expressed in seconds. It is worth noticing that the vibrations throughout the ventral segments in this case are nearly a quarter of vibration behind the extremity in phase. If the theory of friction here applied be correct, many important facts could follow from a determination of the value of k in different substances—for example, the relative duration of the harmonics of a piano-wire.

Let us now calculate what is the work done by the force maintaining the vibration of the extremity. The force there exerted is

$$E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right),$$

and the work done in time dt is

$$E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right)\frac{d\xi}{dt}dt,$$

x being put equal to l after differentiation. We have then work done from time 0 to time t

$$= \int_0^t \left\{E\left(\frac{d\xi}{dx} + k\frac{d^2\xi}{dx\,dt}\right)\frac{d\xi}{dt}\right\}_{x=l} dt.$$

In estimating the work done in any considerable period, we may exclude the periodic terms as unimportant. Hence work done on extremity of string

$$= \frac{nEt}{2}\left\{\left(\frac{d\psi}{dx} + kn\frac{d\phi}{dx}\right)\phi - \left(\frac{d\phi}{dx} - kn\frac{d\psi}{dx}\right)\psi\right\}_{x=l}$$

$$= \frac{nEt}{2}\left\{\frac{d\psi}{dx} + kn\frac{d\phi}{dx}\right\}_{(x=l)} A.$$

An expression for this could of course be at once written down without approximation; but the case where k is small is most important; then we have

$$\begin{cases} P=\sin\dfrac{nl}{a}, \\[2ex] Q=\cos\dfrac{nl}{a}\cdot\dfrac{n^2lk}{2a}, \end{cases}$$

$$\begin{cases} A_1=\dfrac{A}{\sin\dfrac{nl}{a}}, \\[3ex] B_1=\dfrac{A\cos\dfrac{nl}{a}}{\sin^2\dfrac{nl}{a}}\cdot\dfrac{n^2lk}{2a}, \end{cases}$$

unless $\sin\dfrac{nl}{a}$ becomes very small,

$$\begin{cases} \phi=A\dfrac{\sin\dfrac{nx}{a}}{\sin\dfrac{nl}{a}}, \\[3ex] \psi=\dfrac{A}{\sin^2\dfrac{nl}{a}}\cdot\dfrac{kn^2}{2a}\left\{l\cos\dfrac{nl}{a}\cdot\sin\dfrac{nx}{a}\right. \\[2ex] \qquad\qquad\qquad\qquad\left.-x\cos\dfrac{nx}{a}\sin\dfrac{nl}{a}\right\}. \end{cases}$$

Work done on the string

$$=\frac{n^3Etk}{4a\sin^2\dfrac{nl}{a}}A^2\left\{l\cos^2\frac{nl}{a}\cdot\frac{n}{a}-\cos\frac{nl}{a}\sin\frac{nl}{a}\right.$$

$$\left.+l\frac{n}{a}\sin^2\frac{nl}{a}+2\sin\frac{nl}{a}\cos\frac{nl}{a}\right\}$$

$$=\frac{n^3EtkA^2}{4a\sin^2\dfrac{nl}{a}}\left\{\frac{nl}{a}+\sin\frac{nl}{a}\cos\frac{nl}{a}\right\}.$$

If

$$\sin\frac{nl}{a}=0,\quad \mathrm{Q}=\pm\frac{n^2lk}{2a},$$

$$\mathrm{A}_1=0,\ \text{and}\ \mathrm{B}_1=\pm\frac{2a}{n^2lk}\mathrm{A},$$

$$\begin{cases}\phi=\pm\dfrac{\mathrm{A}}{l}x\cos\dfrac{nx}{a},\\[2ex] \psi=\pm\dfrac{2a\mathrm{A}}{n^2lk}\sin\dfrac{nx}{a}.\end{cases}$$

Work done $=\dfrac{\mathrm{A}^2}{lk}\mathrm{E}t.$

We infer that the energy imparted to the string varies as the square of the amplitude of vibration of the extremity, that it rapidly increases as the period approaches that of the string, that, if these periods differ materially, the work is directly proportional to the friction and increases rapidly with the number of vibrations—but that if the periods are identical, the work varies *inversely* as the friction, the diminishing of the friction being more than counterbalanced by the increased amplitude.

It is interesting to examine how this energy is distributed over the string. This is easily done by writing down the work done by one portion of the string from x to l, on the remainder from 0 to a, and then taking the differential; we readily find that work absorbed by portion dx of string

$$=\frac{n^2k\mathrm{E}t}{2}\left(\left|\frac{d\phi}{dx}\right|^2+\left|\frac{d\psi}{dx}\right|^2\right)dx.$$

Substituting, we obtain, when the string does not resonate,

$$\text{work}=\frac{n^4k\mathrm{E}t}{2a}\,\frac{\cos^2\dfrac{nx}{a}}{\sin^2\dfrac{nl}{a}}\,\mathrm{A}^2\,dx;$$

when the string resonates,

$$=\frac{\mathrm{E}t}{l^2k}\cos^2\frac{nx}{a}\,.\,\mathrm{A}^2dx.$$

In either case the absorption of energy, and therefore the heating-effect, is greatest at the nodes, and, omitting squares of k, vanishes at the middle of the ventral segments. Directly the contrary will result from the friction of the string against the air.

Glass Works, near Birmingham.

XXIII. *On the Action of Solid Bodies on* [*Gaseous*] *Supersaturated Solutions.* *By* F. C. Henrici*.

No. 52 of the *Naturforscher* (1869) gave a short account of numerous experiments by Mr. Tomlinson on the action of solids on supersaturated solutions, as brought before the Chemical Society. The views which Mr. Tomlinson has founded thereon, and which in their exposition called forth some objections, do not appear to me to follow from the phenomena; and as some experiments of my own strengthened my objections, I determined to investigate the matter fully. I have limited my numerous experiments to gas-impregnated water, a limitation which can have no injurious effect on the inquiry.

It is necessary first of all to define accurately what we mean by a supersaturated solution. If we consider how difficult it is to free water from air, even by boiling or by the action of the air-pump, there must evidently be between air and water an attraction, or so-called *adhesion*. The volume of air in solution must depend on this adhesion, and also on the atmospheric pressure at the time. The temperature has also considerable influence, since heat diminishes the adhesion and increases the expansive force of the air-molecules. There is also under the given conditions an equilibrium of pressure between the exterior and the dissolved air, which does not exist when these conditions are wanting. Hence, if the water contain too little air, it will absorb more, and in the opposite case part with it. In the latter case, therefore, the water is supersaturated with air, which is nearly always the case with spring-water. As respects gases which are not found in the atmosphere, or only in minute quantities (such as hydrogen, carbonic acid, and ammonia), the equilibrium of pressure does not obtain. Each of these gases, with respect to the quantity brought into contact with water, produces supersaturation more or less.

With respect to equality of pressure, the air particles in a gas-holding liquid are under the same conditions in all directions. If supersaturated, air particles continually escape from the surface, and others follow from the interior, their tendency to expand being thereby assisted. But this condition is entirely changed when the continuity of the liquid is interrupted by a solid body, as indeed it already is by the boundary walls of the containing vessel. In such case, in addition to the attraction between the liquid and the air or gas, there is also an attraction between both of these and the solid sides; and it is a question which of these two attractions will prevail. There are three cases to be considered:—

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* Translated by Charles Tomlinson, F.R.S., from Poggendorff's *Annalen*, No. 12 (1872). The translator has made a number of critical remarks on this interesting paper, which he proposes to send in time for the April Number of the Philosophical Magazine. In the mean time he begs to refer to two papers by him on the same subject in the Philosophical Magazine for August and September 1867.

1. The attraction between a liquid and air or gas has the preponderance; in this case the solid sides produce no change, and there is no separation of gas.

2. The attraction between the solid and the gas has the preponderance; in such case gas attaches itself to the solid, but does not escape.

3. The attraction between the solid and the liquid has the preponderance, in which case there is a condensation of gas on the solid surfaces, and, if the liquid is sufficiently supersaturated, an escape of gas (the quantity depending on circumstances).

The last case applies to Mr. Tomlinson's experiments, and also to my own. The first condition in such an inquiry is doubtless the most perfect cleanliness of the solid surfaces employed; for with unclean surfaces definite results cannot be obtained*. I have adopted the following method for obtaining clean surfaces—namely, rubbing them with fine pumice-stone powder sprinkled on soft leather. Surfaces so cleaned are well adapted to galvanic experiments; and their purity may be tested by the facility with which water wets them. The solids used were metal, glass, and bone: the supersaturated liquid, to begin with the simplest, was freshly drawn spring-water in small cylindrical glasses, and also in ordinary test-glasses. The observations were assisted by a double convex lens. The first experiments were with newly cleaned wires of different thicknesses of platinum, silver, brass, copper, plated copper, zinc, and steel, a strip of platinum, glass and bone rods. These were usually attached to a cork, and so sunk in the water to the depth of about two inches. No sooner were the solids in contact with the water, than they, as well as the side of the glass, were immediately dotted over with minute air-bubbles, which constantly increased in number and size until, after some time, the surfaces were more or less covered with them, occasionally quitting hold and ascending. No difference of action worth mentioning was to be observed between the different solids. With highly impregnated water, the bubbles reappeared more or less numerously next day on immersing the solids. These results were obtained, however often the experiments were repeated.

My next experiments were with water supersaturated with carbonic acid. The gas was generated by means of common effervescing powders, which enabled me to impregnate the water to the required strength, and made me independent of the variations which constantly occur in spring-water*. By the use of small quantities of the powder and the before-mentioned solids immersed in the water, as soon as it became clear the action was very decided, so that decreasing quantities of the powder were sufficient to cause the solids to be covered with innumerable bubbles as soon as they were immersed. A fuller impregnation of the water produced a lively effervescence. The results are so beautiful as to leave nothing to be desired. Moreover the carbonic acid remains a remarkably long time in the water. In less than two cubic inches of carbonic-acid water, brass and silver wires acted after twenty-four hours and produced an abundant separation of bubbles.

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* I have not made use of the terms "active" and "passive," since they are calculated to excite false ideas as to the mode of action.

Since, by frequent repetitions of these experiments, the same results were always obtained, and as I had bestowed the greatest care on the cleansing of the submerged solids, I am fully convinced that solids made perfectly clean, coming into contact with gas-impregnated water, is a necessary condition for the unequivocal production of the phenomena in question. This is in direct opposition to the hitherto received view, according to which *unclean*, and especially dusty surfaces, produce the separation of the bubbles in question. This view does not seem to have been submitted to rigid proof, but rather to have been supported by a solitary experiment. If a given surface, such as a silver wire, a glass rod, &c., be submitted to the action of a small flame of spirits of wine, such a surface will scarcely act, or not act at all, in separating gas. Hence it has been concluded that all kinds of organic substances contracted by exposure to the air were thus burnt off from the surface so as to leave it perfectly clean. This conclusion, however, follows so little from the premises, that the flame produces only a change in the covering and converts it into carbon or ash. This consideration has led me to inquire further into the matter; and the following are the results:—

A carefully cleaned glass rod was for some time moved over a small flame of spirits of wine, after which nothing could be seen upon its surface; but when it was drawn with slight pressure between the folds of a clean linen cloth, a frictional impediment was plainly felt, even producing a faint noise when the pressure was increased, and the film could only be removed from the surface by a tolerably strong pressure.

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* These changes are really very great. My experiments with spring-water were for the most part carried on in summer; and I was very much surprised, on resuming them in autumn, to find the same water of no use. In summer the fermentation of the moist constituents of the soil is most active; we might therefore presume that the gaseous contents of spring-water at that season consist chiefly of carbonic acid; only the water drawn in autumn gave a precipitate with baryta-water. The solid contents of the water of this place are exceedingly small.

The same effect was produced in the case of all kinds of metal. A clean silver teaspoon had its hollow held over the flame, after which this inner surface had no particular appearance, and became wetted with water much in the same way as the simply rubbed surfaces; nevertheless it cost me trouble to get rid of the frictional impediment in wiping out the hollow.

At the extremity of a strip of milk-white glass which had been held in the small flame, there could be seen, with the assistance of a lens, minute specks which could only be removed by very strong pressure.

We must also refer to the deposit of carbon on the bottom of flasks &c. heated by a spirit-flame.

Hence I must confidently conclude that, by the action of a spirit-flame, glass and other solids become covered with a scarcely recognizable (carbon?) film, which is itself sufficient to prevent the separation of gas-bubbles from aërated water, probably by absorption. The dust-particles which cover all bodies exposed to the air are most readily removed by rubbing with a clean duster, or rinsing with clean water. Surfaces which after previous cleaning are long exposed to the air and then so treated, act in fact like freshly cleaned ones. A glass rod so treated, a silver wire which had stood twenty-four hours and upwards in water, were covered abundantly with bubbles when put into aërated supersaturated water. The most convincing proof is the action of a pure quicksilver surface. In a small glass cylinder 5 centims. in height and $1\frac{3}{4}$ in width, a drop of pure mercury was poured sufficient to cover the bottom, and carbonic-acid water was carefully agitated with it. The mercury was immediately covered with rapidly swelling bubbles, which ascended and others formed in their place. As by the shaking of the glass the bubbles escaped from the mercury, this was immediately covered with new ones; and this result, notwithstanding the small quantity of gas in solution, could be repeated many times with scarcely any diminution, even in but slightly impregnated water, in which other surfaces did not act—thus showing that mercury, separating numerous bubbles, possesses a surpassing activity. Pure mercury forms indeed the most perfect surface that can be used; and it is completely wetted by water.

The choice of solids for these researches is somewhat limited. Thus pure metallic surfaces cannot be compared with oxidized ones, since the mechanical condition of the surface has the most powerful influence on the results, and is often entirely different in the two cases. If a clean polished brass wire be compared with one that has been well rubbed with sand paper, the latter will be found to contain minute furrows, and if plunged into aërated water will be immediately covered with innumerable scarcely visible bubbles which arrange themselves in the furrows; while on the first wire less numerous and, on that account, more quickly growing bubbles appear. In the same way behaves a strongly oxidized zinc wire that has long been exposed to the air, the minute roughnesses of which are easily detected by rubbing with the finger; only in this case the furrows are absent. An oxidised brass wire, as compared with one free from oxide, shows, on the contrary, no great difference.

It has been already remarked that the temperature has a great influence on the quantity of gas held in solution. If of two volumes of equally impregnated water one be warmed, there is a diminution of attraction between the air and water particles, and also in the expansive force of both; supersaturation sets in; and if this already existed, it is increased. Hence in air- or carbonic-acid-impregnated water, it may happen that no bubbles can be separated, but if gently heated by a small flame, or a warm metal plate, the action sets in at once. By this simple means soda-water or spring-water that has long been exposed to the air and apparently of no further use, may again be brought into activity. In water kept until the next day, in which no bubbles can be separated (as is almost always the case), a gentle heating causes them to appear anew and swell out in large numbers.

The experiments with oxidized zinc wire and with scratched brass wire plainly show how important is the mechanical condition of the surface on the liberation of gas. It was very interesting to notice the action of the parts of these sap-containing plants the surface of which is slightly rough, such as the stems of the strawberry, milfoil or common yarrow, birch leaves with their peduncles, &c. When these were immersed in the air- or carbonic-acid-impregnated water, their minute projecting points were quickly covered with numerous bubbles, which took up an enormous quantity. The phenomenon was very interesting in water weakly impregnated with air and carbonic acid, when this was gently warmed. As the bubbles gradually formed, they were from the first easily seen; the smallest appeared on the finest hairs of the pine or Scotch fir, which were entirely covered with them, and in sunshine displayed a brilliant show of colours.

These oft-repeated experiments convinced me that the air-bubbles at the moment of their appearance are so small as to be invisible, and by slow growth attain a visible size. How the cohesion of the liquid at any given point within it (as is required by the origin of a bubble) is overcome, cannot be made a matter of observation; but that this takes place by means of the slight roughnesses of the surface, fine points, &c., cannot, according to my experiment, be doubted. Once admit the origin of a bubble, an enclosing liquid surface is thus created, into the hollow space of which other bubbles can penetrate, as continually happens at the free outer surface of the liquid, where the air particles can follow the bent of their force of expansion upwards.

The bubbles have, as a rule, so far as I have been able to make out, a spherical form; but they assume, in consequence of rapid growth, especially when thick together, an elongated form. In this case I have not remarked that they coalesce (a plain proof of the tenacity of the boundary walls), but they rather, when they have attained a certain size, become detached and ascend. Nor are the bubbles in actual contact with the operating surfaces, which may be accounted for by the adhesion of the liquid to the wet surfaces, whereby the fluid particles lose to a great extent their mobility; and hence it is intelligible why the bubbles cling with such remarkable tenacity to the surfaces: the smaller ones can scarcely be loosened by striking the vessel, and it is not till they have attained a certain size that they acquire ascensional force sufficient to detach them; so that on perpendicular or oblique surfaces they often ascend with diminished speed*. The nature of the surface seems from these results to have an influence. The bubbles do not attain so large a size on glass as on metal; the attraction between glass and water must be less than between metal and water, and hence the water particles would have less adhesion to glass.

If, as I doubt not, the condensation of the air-impregnated water on the immersed surfaces is the cause of the separation of the bubbles, such condensation acting in another way will produce the same result. The following experiments were made with water so slightly impregnated that an immersed silver wire produced no bubbles; and these experiments illustrate the well-known fact, that aqueous solutions under like conditions do not take up so much gas as pure water. By adding a little sulphuric acid, or portions of concentrated solutions of different readily soluble salts, to the impregnated water, there was a greater or less separation of gas-bubbles on the glass rod or other immersed surface. The following form of experiment gave the most striking results:—A copper wire hanging in a test-glass was twisted at the centre so as to hold some fibres of linen for the reception of the body that was to enter into solution and to retain it at the surface level of the liquid in the test-glass. Fragments of carbonate of soda, saltpetre, common salt, &c. descended in concentrated streams visible to the eye. During this descent and before the solution had time to accumulate at the bottom of the jar and diffuse through the liquid, numerous bubbles separated on introducing a glass rod or silver wire; and this continued long in action, although only very small fragments of the salt were used. On using caustic alkali on air-impregnated water, there arose an opaque cloud of the finest bubbles as the saline streams descended; and this cloud slowly ascended and disappeared. The further course of the experiment was as with the other salts. This experiment was tried with water that had been boiled; but it is needless to add that nothing followed.

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* Hence doubtless arises the remarkable phenomenon which I have observed in using the effervescing powders; namely, the ascending columns of the smallest bubbles were continually reflected from the under side of the upper surface, while many single ascending bubbles bounded repeatedly therefrom ere they could break through the thin surface.

The well-known phenomenon will be remembered, where an effervescent wine, in which, after standing some time, bubbles no longer rise, foams when the glass is struck or agitated, and slight condensations of gas appear on the surface. If a blow be struck on the side of the glass with a rod of wood, bubbles immediately appear at the point struck.

De Luc referred these phenomena to the film of air which covers all bodies exposed to the atmosphere. There certainly are cases in which, on immersing solids in air-impregnated water, bubbles are produced in this way—as, for example, when the surfaces are such as not to be wetted by the water. I have made many experiments with dry, green grass halms, slightly rough on the surface, which show this result. On immersing them in supersaturated gaseous solutions, they became covered with innumerable bubbles, especially in the more extended air-spaces, such as the fine furrows &c.; such air-spaces remain longer in water exposed to the air; while such water may not give a single bubble to a metal surface. The air is evidently derived from that which is carried down during the immersion of the halm. All porous bodies act in the same way when immersed in liquids. Ripe grass halms with smooth surfaces behave like solids generally; they produce only a few bubbles.

It would doubtless be very interesting to carry out an exhaustive experimental inquiry into the cases 1 and 2, since they do not involve insuperable difficulties. In the meanwhile I cannot help offering a few remarks. Let us begin with the second case, and consider the phenomena which arise when the attraction between a solid surface and the gas in an aqueous solution prevails. In this case the first effect can only be the condensation of the gases on such surface—the very opposite to a separation. The adhesive attraction is but a preliminary step to the chemical, with which a condensation of the less dense constituents is indeed always connected. Only in the case in which the gas in solution unites chemically with the solid so as to form a gaseous body, could there be a separation of gas-bubbles.

There are many bodies which have for gases a strong condensing force or power of absorption, such, for example, as charcoal for carbonic acid. But their great porosity and numerous points and roughnesses (which soon exhaust their absorbing powers) render them scarcely available for experiments of this kind. I have endeavoured to prepare a piece of charcoal by plunging it red-hot into previously boiled water and keeping it immersed some days with occasional heating. On plunging it into highly impregnated water, only a few solitary bubbles appeared on its surface, while a silver wire similarly immersed was completely covered with them. The graphite from a common lead pencil or roughly dressed strips of the same material prepared as above, and plunged into the same highly impregnated solution of carbonic acid, produced no bubbles, notwithstanding the roughness of the surface. The non-appearance of the bubbles can only be ascribed to the condensation of the gas within the pores of the graphite.

I have prepared the charcoal in another way, namely by triturating it while thoroughly wet. The wet powder was then shaken into a test-glass; and after standing some days, the supernatant water was drawn off and soda-water carefully poured upon it. The result was as before, scarcely a bubble was to be seen; while in another test-glass, which instead of charcoal-powder contained the finest quartz sand, numerous bubbles appeared on it; and these being released by shaking, others took their place. Pieces of stone-coal with brilliant surfaces and one piece with polished sides were covered with bubbles.

The remarkable condensation of the mixed gases on a clean platinum surface, the condensation of oxygen alone, and even the less marked condensation of hydrogen on such a surface, may be made the subject of experiments with water sufficiently impregnated with these gases. A small glass cylinder 9 centims. in height and 1½ centim. in width, was one evening filled with boiled water, and five or six drops of strong sulphuric acid added; a strip of zinc was then put in and the glass left in a cool place. The gas came off so quietly that on the following day it was still escaping. What was left of the zinc was taken out, and a well-cleaned platinum and silver wire inserted. The platinum had scarcely any action; but numerous bubbles appeared on the silver wire, the number and size of which increased, especially after warming the glass, while on the platinum wire not a bubble was to be seen.

For the investigation of the first case (preponderance of attraction between water and gas) a sufficient number of examples may be found. In an experiment with a strong solution of ammoniacal gas, in which a silver wire and a strip of platinum were immersed, no bubbles appeared on either surface, although innumerable bubbles were set free by the action of heat. Strong hydrochloric acid behaved in the same way with platinum; only on heating it more strongly fewer bubbles were produced, showing the much greater attraction between water and hydrochloric acid gas. By long exposure to the air both solutions became much weaker; and on gently warming them, the immersed wires became covered with bubbles, arising doubtless from the liquid having absorbed atmospheric air.

In conclusion, some experiments on fatty bodies may be referred to. Large drops of olive-oil, oil of almonds, and linseed-oil on the surface of water well impregnated with carbonic acid produced no separation of bubbles. The attraction of these oils for water is therefore not sufficiently energetic for the purpose; and it remains to be seen whether they absorb the gas to a slight extent. Stearic acid melted at the bottom of a small glass cylinder, and after becoming solid covered with soda-water, gave off an extraordinary display of bubbles. After some time the stearine loosened its hold and rose to the surface, separating from the fluid fat, a chemical action having taken place. With water previously boiled no bubbles were produced.

Freiburg, im Breisgau, January 1871.

XXIV. *On Arithmetical Irrationality.*

By J. W. L. Glaisher, *Fellow of Trinity College, Cambridge*[*].

It is rather curious that, although very many of the numerical quantities with which the mathematician is constantly concerned are generally believed to be irrational (*i. e.* not to terminate or circulate[†] when expressed as decimals), yet the fact of such irrationality has been demonstrated in only a few cases; and it is still more remarkable that so little attention seems to have been given to the matter at all. To take an example, I suppose no one has any doubt (using the words in the sense that there is no one who would not be very much surprised if the contrary were proved) that all the sines in an ordinary Briggian or hyperbolic logarithmic canon, in which the arguments are commensurable with a right angle, are irrational; but, as far as I know, no one has attempted to prove this; and the same may be said of many similar properties.

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[*] Communicated by the Author.

[†] In the rest of this paper the word *circulate* will be supposed to include the case where the decimal terminates, as is indeed the fact; for a terminating decimal is merely one in which the circulating period consists entirely of zeros. Thus every numerical quantity either circulates and is rational, or does not circulate and is irrational.

The most general theorems of this class with which I am acquainted were proved by Lambert in the Berlin Memoirs for 1761; he has there shown that the tangent of every rational arc (viz. arc commensurable with the unit arc, equal to radius) is irrational, and that the tangents of all angles commensurable with a right angle, except tan 45°, are irrational. From the first of these results it follows that π is irrational (since $\tan\frac{\pi}{4}=1$); and to establish this was the main object Lambert had in view in undertaking his investigation. It also follows, though not quite so simply, that π^2 is irrational; but I believe no one has ever shown that π^3 or any higher power of π is so too; so that, as far as proof is concerned, π might be the nth root of a rational quantity, though, as Legendre has remarked, there is very little doubt that it is not the root of any algebraical equation with rational coefficients.

Lambert's principle consists in developing the quantity which is to be proved irrational into a continued fraction; and his result, stated in what is apparently the most generalized form it admits of, viz. that given to it by Legendre in the notes to his 'Geometry,' is that if in the continued fraction

$$\cfrac{\beta_1}{\alpha_1+\cfrac{\beta_2}{\alpha_2+\cfrac{\beta_3}{\alpha_3+\&\text{c.}}}}$$

(extended to infinity) $\frac{\beta_1}{\alpha_1}, \frac{\beta_2}{\alpha_2}\ldots$, regarded as fractions ($\alpha_1, \alpha_2, \ldots$ $\beta_1, \beta_2, \ldots$ all integers), be all less than unity, then, whether $\beta_1, \beta_2, \ldots$ be all positive or all negative, or some positive and some negative, the value of the continued fraction is irrational.

It is clear that the expansion of the quantity into a continued fraction is the most natural way of attacking the question, as the process is identical in principle with that of finding the greatest common measure in arithmetic.

Besides the theorems cited above with regard to tangents, Lambert showed that the hyperbolic logarithm (viz. logarithm to base 2·7182818...) of every rational number was irrational; and the corresponding theorem when the base of the system is a rational number is evident; for, to take the common base, we see at sight that $10^x=\mathrm{N}$ (an integer) can only be satisfied by a rational value of x when N is a power of 10.

There is another method by which the irrationality of a series can be proved; but it is of exceedingly limited application. I refer to the way in which e is usually shown to be irrational, viz. that if $e=\frac{n}{m}$ (m and n integers), then

$$\frac{m}{n}=\frac{1}{1.2}-\frac{1}{1.2.3}\dots\pm\frac{1}{1.2\dots n}\mp\frac{1}{1.2\dots(n+1)}+\dots,$$

so that

$$m(1.2\dots n-1)=\text{integer}\mp\left(\frac{1}{n+1}-\frac{1}{(n+1)(n+2)}+\dots\right),$$

or integer = integer + fraction;

so that m and n cannot be finite integers.

This method can only be applied when the denominators involve all numbers (or multiples of them), and when each denominator includes all the preceding ones, the numerators being constant.

There cannot be much doubt that in an ordinary natural canon sin 30° is the only rational sine, though I believe this has not been proved; ($\sin\frac{1}{x}$ and $\cos\frac{1}{x}$ are easily seen to be irrational; for x an integer, the unit being the arc equal to radius, by the sort of reasoning applied above to e^{-1};) and many other constants are in the same state, as, *e. g.*, $\log\pi$, e^{π}, &c., or Euler's constant ·577215....

Lambert's principle will be found to be not often applicable, as the conditions requisite are but seldom fulfilled; it is far more common for $\frac{\beta_1\beta_2\dots}{\alpha_1\alpha_2\dots}$ to be infinite than zero.

The method of proving the irrationality of certain quantities which I now proceed to explain, although very simple, does not seem to have been noticed; at all events Eisenstein, who was occupied with the irrationality of some of the quantities to which it is directly applicable, certainly did not perceive it. An example will make the principle of the method clear. Consider the series $1+q+q^4+q^9+q^{16}+\dots$ which occurs in the theory of Elliptic Functions; it follows at once that the value of this series is irrational whenever q is the reciprocal of an integer greater than unity; for if $q=\frac{1}{r}$, then in the scale of radix r the value of the series would be written

$$1{\cdot}1001000010000001000000001\dots,$$

which, as the intervals between the 1's consist of 2, 4, 6, 8, 10... ciphers, does not circulate. In the scale of radix r, therefore, the series cannot be expressed in the form $\frac{\text{M}}{\text{N}}$, M and N being finite integers. And as M and N are not integers in the scale of radix r, neither are they integers in the scale of radix 10, as a number expressible as an integer in one scale must clearly also be so expressible in any other scale, both the radixes being integers.

It is evident that the same kind of reasoning applies to all series of the form $1+q^{\phi(1)}+q^{\phi(2)}+q^{\phi(n)}+\ldots$, where $\phi(n)$ is a rational non-linear function of n such that, when n is an integer, $\phi(n)$ is so too; so that all such series are irrational when q is the reciprocal of an integer greater than unity. We see also that the method is susceptible of being still further generalized, and gives a result in a great number of cases where the coefficients are not unity nor all positive; thus

$$1-q+q^4-q^9+\ldots=\cdot900099999000000099\ldots$$

(q being, as throughout the rest of this paper, $\frac{1}{r}$, and the symbol 9 denoting the digit $r-1$) cannot circulate; and the same is the case with $1+q+2q^4+3q^9+\ldots$, $1+q+4q^4+9q^9+\ldots$, &c., and generally with $1+\psi(1)q+\psi(2)q^4+\psi(3)q^9+\ldots$, where $\psi(n)$ is such that the number of figures constituting it in the scale of radix r always bears a ratio less than unity to $2n-1$ (which is the difference between the number of ciphers preceding the first significant figures in $q^{(n-1)^2}$ and q^{n^2}),—as then the numbers of figures in the groups of 0's or 9's continually increase, so that the decimal cannot circulate.

The number of figures that any number u will occupy when expressed in the scale of radix r is equal to the integer next above $\log_r u$; so that if we apply the above reasoning to the series $1+xq+x^2q^4+x^3q^9+\ldots$, we see that so long as x is an integer such that $\log_r x^{n+1}<2n$ (that is, if x be an integer less than r^2), its value is irrational.

That the number of zeros in a group is in this case ultimately infinite is apparent, as the number in question when $x=r^2-1$ is approximately $2n-\log_r(r^2-1)^{n+1}$, which

$$=(n+1)\log_r\frac{r^2}{r^2-1}-2,$$

and is infinite with n; that the series is irrational when $x=r^2$ is easily seen otherwise.

The most general case to which the method is applicable is that of the series $1+\psi(1)q^{\phi(1)}+\psi(2)q^{\phi(2)}+\psi(3)q^{\phi(3)}+\ldots$, which is irrational if $\log_r\psi(n)+1<\phi(n)-\phi(n-1)$, so that when n is infinite, $\phi(n)-\phi(n-1)-\psi(n)$ is infinite too,—$\phi(n)$ being as above, and $\psi(n)$ a rational function of n, which is an integer when n is an integer.

It is not essential that the exponents of the powers of q should be given by an algebraical formula $\phi(n)$; the reasoning is equally successful when they are the series of prime numbers, or of their squares, cubes, &c.

I need scarcely remark that the sole condition for the success of the method is that the groups of 0's or 9's do not circulate; so long as this is the case the signs $\pm$ may occur in the terms in any order. A good example is afforded by the product $(1-q)(1-q^2)(1-q^3)\dots$, which Euler proved to be equal to

$$1-q-q^2+q^5+q^7-q^{12}-\dots,$$

the general term being $(-)^n q^{\frac{1}{2}(3n^2\pm n)}$, so that the product, when q is the reciprocal of an integer, is evidently irrational.

It will thus have been seen that the method is applicable to a considerable number of series, the irrationality of which is not seen otherwise. Lambert's principle, however, can be applied to a good many cases by means of the following formulæ:—

$$\frac{1}{a_1}-\frac{1}{a_1a_2}+\frac{1}{a_1a_2a_3}-\dots=\cfrac{1}{a_1+\cfrac{a_1}{a_2-1+\cfrac{a_2}{a_3-1+\&c.}}}, \quad (1)$$

$$\frac{1}{a_1}+\frac{1}{a_1a_2}+\frac{1}{a_1a_2a_3}+\dots=\cfrac{1}{a_1-\cfrac{a_1}{a_2+1-\cfrac{a_2}{a_3+1+\&c.}}}, \quad (2)$$

which are proved at once, since

$$\frac{1}{a_1}-\frac{1}{a_1a_2}=\cfrac{1}{a_1+\cfrac{a_1}{a_2-1}},$$

so that the nth convergent to the fraction is identically equal to the first n terms of the series.

From (2) we have

$$1+\frac{1}{r^\alpha}+\frac{1}{r^\beta}+\frac{1}{r^\gamma}+\dots 1+\cfrac{1}{r^\alpha-\cfrac{r_\alpha}{r^{\beta-\alpha}+1-\cfrac{{}^{r}r^{\beta-\alpha}}{r^{\gamma-\beta}+1-\&c.}}}; \quad (3)$$

since $\qquad r^\beta=r^\alpha.r^{\beta-\alpha}, \quad r^\gamma=r^\alpha.r^{\beta-\alpha}.r^{\gamma-\beta}$, &c.;

so that if $\alpha, \beta, \gamma, \dots$ are such that the differences $\beta-\alpha$, $\gamma-\beta, \dots$ after some point always increase, Lambert's principle is applicable and the series is irrational—the same result as was obtained above when the series was written $1+q^{\phi(1)}+q^{\phi(2)}+\dots$ The same reasoning also applies when any of the signs are negative;

which by (1)

$$=\frac{1}{r-1+}\,\frac{r-1}{r^2-2+}\,\frac{r^2-1}{r^3-2+\&c.},$$

to which also Lambert's principle applies.

I may mention that Eisenstein (whose chief results have been incidentally reproduced above) has enunciated his theorems without demonstration, and evidently intended to return to the subject, though after an examination of his subsequent memoirs I feel pretty certain he never did do so. He states that he was in possession of a more general theorem; and I think it likely that the result he alludes to was that marked (3) above, or some other continued fraction of kindred scope, from which the irrationality of a good many series could be deduced. Eisenstein must have been aware of the irrationality of $1+q+q^3+q^6+\dots$, as appears from his theorems with reference to series of a similar kind given two years previously; but, curiously enough, he has omitted to state it as a conclusion to be deduced from the continued fraction into which he transformed it; and at the time of writing the paper of which the abstract appeared in the British-Association Report, I had not seen his previous papers, and in fact did not know he had considered the subject of irrationality at all; so that I drew the inference which it was probably merely an accidental omission that Eisenstein had not himself pointed out. The method explained in this paper, however, is far more simple and appropriate in such cases.

I may mention that Eisenstein also gave another continued fraction for the series with squares as exponents, viz.

$$1+\frac{1}{r}+\frac{1}{r^4}+\frac{1}{r^9}+\dots\frac{1}{1-}\,\frac{1}{r-}\,\frac{1-r^2}{r^2-}\,\frac{1}{r^3-\&c.},$$

resembling that given by him for $1+\frac{1}{r}+\frac{1}{r^3}+\frac{1}{r^6}+\dots$

It is interesting, in conclusion, to note that the method explained above gives the proper result for the geometrical series $1+q^\alpha+q^{2\alpha}+q^{3\alpha}+\dots$, which would be written in the scale of radix r $1{\cdot}00\dots100\dots1$ ($\alpha-1$ ciphers in each group), which *does* circulate, as of course it ought to do, since the sum is $\frac{1}{1-q^\alpha}$.

Cambridge, February 11, 1873.

XXV. *On a Method of Testing Submarine Telegraph Cables during Paying-out.* By THOMAS T. P. BRUCE WARREN, *Electrician to Hooper's Telegraph Works, Limited; Member of the Society of Telegraph Engineers* *.

IT is a singular circumstance that although within the last few years we have become, as it were, inundated with new appliances for testing submarine telegraph-cables during their manufacture, so little has been effected towards the improvement of electrical testing during the paying-out.

Apart from the uncertainty which has lately been shown to exist in galvanometric measurements themselves, there are many little difficulties to encounter when testing on board ship, which at times are so embarrassing as to make one forego a very large share of confidence in the results. The electrical condition of the cable must consequently be a matter of great anxiety, until a steady observation enables the electrician to decide that every thing is all right.

The practice has hitherto been to make the duties at the shore station too subsidiary a matter in the system of testing, instead of relying upon the results obtained on shore as of primary importance. Considering the facilities offered when the instruments are perfectly at rest and consequently admit of much more accurate adjustment and indication than is possible on the ship, it is evident that we should look to the shore observations with an equal, if not greater interest.

In the present systems of testing, the shore operations have been usually so formulated that any separate or independent observation is quite inadmissible, and, unless distinctly and definitely preconcerted by the ship, would lead to serious results.

Any system of testing on the ship should be capable of being synchronously carried on at the shore station without impeding any modified operation required on the ship; and either the ship or shore should be capable of being made the controlling station, and in such a way as not to interfere with each other.

The electrician has to provide for two kinds of faults, one of which is brought about by the rupture of the conductor, and the other by a flaw or defect in the dielectric.

The rupture of the conductor involves the cessation of signals from one end to the other, and may be either accompanied by or without defective insulation on one or both sides of the broken conductor.

As the shore must transmit its results to the ship telegraphically, it is evident that in the case of "no continuity"† the ship must follow out its own system of testing for the position of the rupture in the conductor; the only provision which can be made by the shore station, in helping the ship to determine the position of such a fault, is in sending "continuity"-signals as often as possible, so that the ship, at any time failing to receive them, can pronounce upon the position of the rupture without any loss of time.

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* Communicated by the Author.

† The technical expression for a broken conductor.

The following system of "continuity"-signals, suggested by the author in an article which appeared in 'Engineering,' April 24, 1868, serves at the same time to inform the ship and shore station whether the conductor is perfect or broken. The great advantage of this system is that it leaves the electrified condition of the cable undisturbed during the intervals of signalling.

The shore end of the cable being landed, the conductor, or a prolongation of it, is attached to an insulated pin; and at a suitable distance from it is fixed another insulated pin which is connected through a delicate galvanometer to earth; between these two pins an insulated pendulum is made to vibrate.

The pendulum on touching the pin connected with the cable, takes from it a certain proportion of its charge, and during its oscillation empties it into the pin which is connected to the galvanometer.

The capacity of the pendulum is first fixed upon for the length and electrostatic relation of the cable. Increased capacity may be given to the pendulum by attaching to it a condenser of suitable electrical dimensions.

It will be sufficient to insulate from the other portions of the pendulum that part which is concerned in making the contacts.

On contact with the end of the cable by the pendulum, a momentary increase in the deflection will be observed on the galvanometer in the ship, arising from the sudden increase of capacity; and this will continue as a constant range of variation in the angular deflection due to leakage on the cable itself. By means of these slight motions, a "test of continuity" is kept up without interfering with the test of insulation which is always kept on in the ship. The observer on shore is enabled to know with positive certainty the electrical condition of the cable by allowing the abstracted charges to flow to earth through the galvanometer, occasionally noting the deflection.

If the conductor parted, and at the same time each end remained insulated, the observer at the shore end would continue to get indications on his galvanometer; but the intermittent rise and fall in the galvanometer deflection in the ship would immediately cease, and the engineer would know the exact moment that the continuity ceased.

The observer on shore would very soon observe, by the deflection on his galvanometer becoming gradually but very regularly lessened, that the conductor had parted, and the end on the shore side was enclosed in the insulator. If, on the other hand, the deflections suddenly ceased, he would further know that the conductor had parted and was exposed.

In practice it is found preferable, instead of automatically connecting the condenser with the cable and galvanometer, to use an ordinary charging and discharging key, and to perform this test at intervals of every five minutes. According to Mr. Latimer Clark (Electrical Measurement, &c.) this was carried out on the Atlantic cables in 1866, although it was perfectly unknown to the author at the time this article appeared.

In the present method it is proposed to obtain "continuity" indications on the shore, and at the same time to measure the tensions of the end at the shore by means of Sir William Thomson's quadrant-electrometer.

Now, since the conductor of a cable may be parted with insulation still perfect, the electrometer should be capable of showing the fall of tension in 20 or 30 seconds on the highest degree of insulation; but an electrometer with this degree of sensibility could not be used in the ordinary way, when the cable is connected on the ship with 100, or any number of cells usually employed; for the tension of the cable at the shore end would repel the needle in one direction completely beyond the range of being read.

In order to obviate this, the end of the cable is to be connected to one pair of quadrants, and to the other pair a battery in all respects similar to the testing battery on the ship. If the tension of the ship's battery, as found at the shore end of the cable, be such as to balance the opposing battery on the shore, the electrometer will maintain its zero position; and if we could be sure of setting up any ordinary testing battery in two places and at different times alike, we should be able at once to obtain a most accurate comparison of the tensions at the two ends of the cable.

The shore-electrometer being used in this way, the slightest flaw which could not be possibly recognized on the ship's galvanometer would be easily detected, and in deep water a long time before it could possibly reach the bottom.

In this way, by one single observation, the shore obtains a continuity test, an insulation test, and at the same time, should any thing go wrong, the adjustment required to bring the electrometer-needle to its zero position supplies the data for localizing any defect which might arise. Should the ship wish to open communication with the shore, the same instrument is at once ready to receive a "call" signal, or a complete but simple message.

Not to rely solely upon the electrometer for the measurements of tensions, a modification of the condenser method previously described, and which will serve equally well to convey continuity-signals to the ship, will supply a check-measurement of these tensions, and in a way so as to give at once continuity-indication to the ship and the tension in volts to the shore observer.

The charge abstracted from the cable in one condenser is sent through a delicate galvanometer in one direction, simultaneously that a condenser of similar capacity, charged to the same tension from a constant battery, flows in the other direction; a current will be perceptible on the galvanometer flowing from one condenser to the other, should they not be charged to the same tension. The changes due to the galvanometer itself will not interfere; and thus not only will a considerable amount of irksome testing and calculating be avoided, but the results themselves be more accurate.

These tensions will be telegraphed to the ship at stated times. The tension of the ship's battery will be carefully measured on a delicate portable electrometer on board ship for comparison with the results sent from the shore station. The fall of tension on the cable can then be simultaneously noted on the shore as well as on the ship; and during this time the continuity-signals need not be interrupted, as the sensibility of the ship's electrometer will allow the small impulses to be noted, but, instead of in a current form, either by regular and equal abstractions of charge, or by the shore attendant at stated times restoring the full tension. This, in addition to forming a perfect continuity-signal, will at once indicate to the ship the loss observed on shore during any specified space of time. In the same way the ship may inform the shore observer; and both will be able to verify or compare their results.

Any difference of tension can thus be instantly communicated from shore to ship, or *vice versâ*, without the necessity of signalling the results as noted by the sliding resistances or by the condenser method; the signal itself will convey all the data required by the ship, should this difference of tension arise from the existence of a fault.

The reduction and comparison of these tensions should in every case be made with the constant cells of Mr. Latimer Clark. Now the ship and the shore doing this independently of each other, should still arrive at the same result, so long as every thing remains right; but in the event of a slight fault occurring, the two results will not coincide, and the difference observed will correspond to the magnitude of the fault; and as the precise tension at the fault can be measured from either end, the difficulty should be considerably diminished in localizing small faults.

The balancing of the ship's battery on the shore-electrometer by a similar battery stationed on shore, should be done either with a set of ordinary resistance-coils, or, preferably, by a set of Sir William Thomson's resistance-slides.

The points for consideration are, that the battery which has the highest potential should be bridged over with a resistance sufficiently high, so that it may be impossible to produce any appreciable alteration whilst the test is being applied*. The same remark applies to the constant cells employed to balance the discharge from the condenser used for the continuity-signals; for this purpose, too, it will be useful occasionally to take the condenser-tensions on the quadrant-electrometer with the resistance-slide and constant cells in opposition. The insulation of the condensers should be so perfect that no loss can be perceptible during the time of making these tests; and they should also, on this account, be properly and completely charged, whether from the cable or batteries. When using the condensers with the ship's electrometer for continuity-signals to ship, it is equally important that they should be completely discharged. Any tendency in the condensers to give too great residual discharges should be carefully studied beforehand, and their effect on the portable electrometer well noted.

The constant cells used on the ship and on shore, and also the electrometers, should all be very carefully compared by testing against each other a short time before the paying-out commences.

The method here given for measuring the discharges from the condenser will serve as an efficient test for comparing batteries; it involves, however, that their resistances should be ascertained. A great advantage in favour of this plan is, that when the electromotive forces of the cells to be compared are nearly the same, the resistances with which the stronger cell is bridged over will be quite incapable of producing any alteration in its electromotive force.

Tamworth House,
Mitcham Common.

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* The battery on shore should contain a sufficient number of cells above that on the ship, so as to be capable of being balanced against any increase of tension from the ship's battery.

XXVI. *On the Pressure required to give Rotation to Rifled Projectiles. By* Captain Noble, *F.R.S. &c.* (*late Royal Artillery*)*.

[With a Plate.]

1. IN a paper published in the Philosophical Magazine for 1863 (vol. xxvi.), and subsequently in the *Revue de Technologie Militaire,* I gave some investigations on the ratio between the forces tending to produce translation and rotation in the bores of rifled guns.

2. My object in these investigations was to show, 1st, that in the rifled guns with which experiments were then being made the force required to give rotation was generally only a small fraction of that required to give translation; 2ndly, that in all cases (and this was a point about which much discussion had taken place) the increment of gaseous pressure (that is, the increase of bursting force) due to rifling was quite insignificant.

3. In the paper referred to, although the formulæ were sufficiently general to embrace the various systems of rifling then under consideration in England, they did not include the case of an increasing twist, which has since been adopted for the 8-inch and all larger guns of the British service; neither was our knowledge of the pressure of fired gunpowder sufficient to enable me to place absolute values on either of the forces I was considering.

4. Since the date at which I wrote, an extensive series of experiments has been made in this country; and the results of these experiments enable me to give with very considerable accuracy both the pressure acting on the base of the projectile and the velocity at any point of the bore. I am therefore now able not only to assign absolute values where in my former paper I only gave ratios, but also to show the amount by which the studs of the projectiles of heavy guns have been relieved by the introduction of the accelerating twist known as the parabolic system of rifling.

5. Very little consideration will satisfy any one conversant with the subject, that in the ordinary uniform spiral or twist the pressure on the studs or other driving-surface is a maximum when the pressure on the base of the shot is a maximum, and becomes greatly reduced during the passage of the shot from its seat to the muzzle of the gun. In my former paper I showed, in fact, that in a uniform twist the pressure on the studs was a constant fraction of the pressure on the base of the shot, the value of the fraction of course depending on the angle of the rifling; and as it is evident that the tension of the powder-gases at the muzzle is very small when compared with the tension of the same gases

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* Communicated by the Author.

at the seat of the shot, it follows that in such a system of rifling the studs may have scarcely any work to do at the muzzle, while they may be severely strained at the commencement of motion.

6. If then, the defect of the ordinary or uniform system of rifling be that the studs are severely strained at the first instants of motion and are insignificantly strained at the instant of quitting the gun, it is obvious that it is possible to remove this inequality and at the same time allow the projectile to leave the bore with the same angular velocity by reducing the twist at the seat of the shot and gradually increasing it until it gains the desired angle at the muzzle. In fact, if we know the law according to which the pressure of the powder varies throughout the bore, it is theoretically possible to devise a system of rifling which shall give a uniform pressure on the studs throughout the bore.

7. These reasons doubtless led the late Ordnance Select Committee, to whom the application of the increasing twist to the service guns is due, to propose its introduction; and they selected as the simplest form of an increasing spiral the curve which, when developed on a plane surface, should have the increments of the angle of rifling uniform. This curve is, as is well known, a parabola; and as considerable advantages have been claimed for the parabolic system of rifling, I propose in this paper to examine and evaluate them.

I may add that I should not have given the results I now give, before the full experiments made by the Committee of Explosives, as well as some investigations undertaken by Mr. Abel and myself are published, were it not that several groundless assertions concerning the Woolwich rifling have recently appeared, and have led to much discussion and very unnecessary uneasiness.

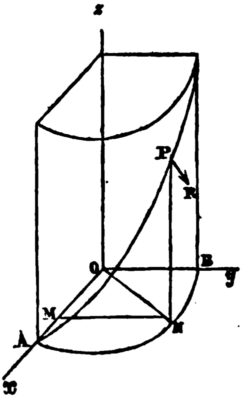
8. The argument commonly advanced against an accelerating twist is based upon the fact of the shot moving slowest at first, it being supposed that while moving slowest the shot will require less force to make it rotate; but there is a fallacy in this argument, which lies in confounding velocity with rate of acceleration. The shot undoubtedly moves slowest at first, but it acquires velocity most rapidly at first, and it is the *gain* of velocity that determines the strain upon the stud.

9. The first question, then, which I propose is, to determine the pressure on the studs of a projectile fired from a gun rifled on a parabolic or uniformly increasing twist; and in this investigation I shall adopt the notation used in my former paper.

10. Take, then, as the plane of xy a plane at right angles to the axis of the gun. If the angle of rifling commence at zero, increasing to, say one turn in n calibres, let the plane of xy pass through the commencement of the rifling; but if the rifling do not com-

mence at zero, it will be found more convenient to make the plane of xy pass through the point where the twist would be zero were the grooves sufficiently prolonged. Let the axis of x pass through one of the grooves; and, for the sake of simplicity, we shall suppose the rifling to be given by one groove only. Let the axis of z be coincident with that of the gun; let A P (see fig. 1) be the groove or curve described by the point P, and let P (x, y, z) be the point at which the resultant of all the pressures tending to produce rotation may be assumed to act at a given instant. Let the angle $AON = \phi$.

Fig. 1.



11. Now the projectile in its passage through the bore is acted on by the following forces:—

1st. The gaseous pressure G, the resultant of which acts along the axis of z.

2nd. The pressure tending to produce rotation. Calling this pressure R, and observing that it will be exerted normally to the surface of the groove, we have for the resolved parts of this pressure along the coordinate axes, $R\cos\lambda$, $R\cos\mu$, and $R\cos\nu$—λ, μ, and ν being the angles which the normal makes with the coordinate axes.

3rd. The friction between the stud or rib of the projectile and the driving-surface of the groove. This force tends to retard the motion of the projectile; its direction will be along the tangent to the curve which the point P describes. If μ_1 be the coefficient of friction, and if α, β, γ be the angles which the tangent makes with the coordinate axes, the resolved portions of this force are $\mu_1 R \,.\, \cos\alpha$, $\mu_1 R \,.\, \cos\beta$, $\mu_1 R \,.\, \cos\gamma$.

12. Summing up these forces, the forces which act

$$\left.\begin{array}{lll} \text{parallel to } x & \text{are} & X = R\,.\,\{\cos\lambda - \mu_1\cos\alpha\}, \\ \quad\text{,,} \quad y & \text{,,} & Y = R\,.\,\{\cos\mu - \mu_1\cos\beta\}, \\ \quad\text{,,} \quad z & \text{,,} & Z = G + R\,.\,\{\cos\nu - \mu_1\cos\gamma\}; \end{array}\right\} \quad . \quad (1)$$

and the equations of motion are

$$M\,.\,\frac{d^2z}{dt^2} = G + R\{\cos\nu - \mu_1\cos\gamma\}, \quad . \quad . \quad . \quad (2)$$

$$M\,.\,\frac{d^2\phi}{dt^2} = \frac{Yx - Xy}{\rho^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

ρ being the radius of gyration. Equations (1), (2), and (3) are identical with those I formerly gave.

13. Now, in the case of a uniformly increasing twist, the

equations to the curve which when developed on a plane surface is a parabola may be put under the form

$$x=r\cos\phi;\quad y=r\sin\phi;\quad z^2=kr\phi. \quad . \quad . \quad (4)$$

Hence

$$dx=-r\sin\phi\,.\,d\phi;\quad dy=r\cos\phi\,.\,d\phi;$$

$$dz=\frac{kr}{2z}\,.\,d\phi;\quad ds=\frac{r}{2z}\sqrt{4z^2+k^2};$$

and we have, to determine the angles which the tangent to the curve described by P makes with the coordinate axes, the equations

$$\left.\begin{aligned}\cos\alpha&=\frac{dx}{ds}=\frac{-2z\,.\sin\phi}{\sqrt{4z^2+k^2}},\\ \cos\beta&=\frac{dy}{ds}=\frac{2z\,.\cos\phi}{\sqrt{4z^2+k^2}},\\ \cos\gamma&=\frac{dz}{ds}=\frac{k}{\sqrt{4z^2+k^2}}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

14. In the Woolwich guns the driving-surface of the groove may be taken, without sensible error, as the simpler form of surface where the normal to the driving-surface is perpendicular to the radius, the surface itself being generated by that radius of the bore which, passing perpendicularly through the axis of z, meets the curve described by the point P; but in the first instance I shall examine the more general case, where the normal makes any assigned angle with the radius.

Assume then that on the plane of xy the normal makes an angle δ with the radius of the gun. The driving-surface of the groove is then swept out by a straight line which, always remaining parallel to the plane of xy, intersects the curve described by P, and touches the right cylinder whose axis is coincident with that of z, and whose radius $=r\,.\cos\delta$.

Now, the equations to the director being given by (4), and that to the cylinder, which the generator always touches, being

$$x^2+y^2=(r\cos\delta)^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

it is easily shown that the coordinates x_1, y_1 of the point of contact of the tangent to the cylinder drawn from P parallel to the plane xy, are

$$\left.\begin{aligned}x_1&=r\,.\cos\delta\,.\cos(\phi-\delta),\\ y_1&=r\,.\cos\delta\,.\sin(\phi-\delta),\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (7)$$

and that the equation to the driving-surface is

$$x\,.\cos\left\{\frac{z^2}{kr}-\delta\right\}+y\,.\sin\left\{\frac{z^2}{kr}-\delta\right\}=r\,.\cos\delta. \quad . \quad (8)$$

pressure on the studs for parabolic rifling, with the equations subsisting where a uniform twist is used.

For a uniform twist we have, as I formerly showed,

$$R=\frac{2\pi\rho^2}{\frac{\mu_1(2\pi\rho^2k-rh)}{\sqrt{1+k^2}}+\frac{(2\pi\rho^2+rhk)\sin\delta}{\sqrt{k^2+(\sin\delta)^2}}}\,.\,G,\ . \quad (16)$$

where h is the pitch of the rifling, k the tangent of the angle which the groove makes with the plane of xy, the other constants bearing the meaning I have already assigned to them in this investigation.

20. In the Woolwich guns, where $\delta=90°$, (16) becomes

$$R=\frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1)+2\pi\rho^2(\mu_1k+1)}\,.\,G,\ \ .\ \ . \quad (17)$$

21. I proceed to apply these formulæ, and propose to examine what are the pressures actually required to give rotation to a 400-lb. projectile, fired from a 10-inch gun with battering charges, under the following conditions:—1st. If the gun be rifled with an increasing twist as at present. 2nd. If it be rifled with a uniform pitch, the projectile in both cases being supposed to have the same angular velocity on quitting the gun. As the calculations for the uniform pitch are the simpler, I shall take this case first.

22. I have before remarked that with a uniform twist the pressure on the studs of the projectile is a constant fraction of that on the base of the shot, and represents, so to speak, on a reduced scale, the pressure existing at any point in the bore of the gun. Calling the fraction in equation (17) C, we have

$$R=C\,.\,G,\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ . \quad (18)$$

where

$$C=\frac{2\pi\rho^2\sqrt{1+k^2}}{hr(k-\mu_1)+2\pi\rho^2(\mu_1k+1)}=\cdot04426,\ \ .\ \ . \quad (19)$$

the values of the constants in (19) being in the case of the 10-inch gun as follow:—

$\rho=\cdot312$ ft., $k=12\cdot732$, $h=33\cdot333$ ft., $r=\cdot417$ ft., $\mu_1=\cdot167$.

Hence

$$R=\cdot04426\,.\,G.\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ . \quad (20)$$

23. But the values of G are known with very considerable exactness from the investigations of the Explosive Committee under the presidency of Colonel Younghusband. The following Table gives the value of (G that is, the total pressure in tons acting on the base of the projectile) for a charge of 85 lbs. of

pebble-powder at various points of the bore, and the corresponding values of R. It will be remarked how high the pressure on the studs is when that on the base of the shot is a maximum, and how rapidly the strain decreases as the shot approaches the muzzle.

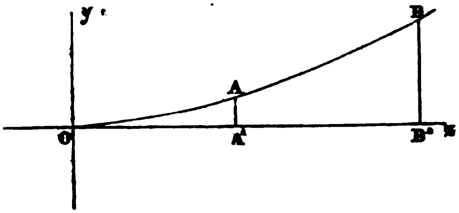
Table showing the pressure on the studs in a 10-*inch British-service gun rifled with a uniform twist, calculated from* (17).

Travel of shot, in feet.	Total pressure G on base of shot, in tons.	Value of C.	Value of R, or total pressure on studs, in tons.
0·000	0	·04426	0
0·333	1547	„	68·5
0·945	1077	„	47·7
1·834	781	„	34·6
2·723	621	„	27·5
3·612	510	„	22·6
4·500	424	„	18·7
5·389	356	„	15·8
6·278	305	„	13·5
7·167	268	„	11·8
8·055	240	„	10·6
8·944	220	„	9·7
9·833	205	„	9·1

24. The results in the Table show the pressures required to give rotation, if the 10-inch gun be rifled on a uniform twist. I turn now to the rifling as it actually exists, and which is defined to be a parabolic twist, commencing with one turn in 100 calibres and terminating at the distance of 9·833 feet with a twist of one turn in 40 calibres; and first to determine the equation to the parabola.

Let the origin be at the point where the twist vanishes when the curve A B is sufficiently prolonged—that is, at the vertex of the parabola. Let O z and O y' be the axes of coordinates; let

Fig. 2.



O $A'=z_1$, O $B'=z_2$; let tan θ_1 be the tangent of the angle which the curve makes with O z at A, and tan θ_2 be the corresponding tangent at B.

Then, from the definition of the parabolic twist,

$$\frac{d^2y'}{dz^2} = \text{constant} = c, \text{ suppose};$$

$$\therefore \frac{dy'}{dz} = cz; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and

$$y' = \frac{c}{2}z^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

But, from (21),

$$\tan\theta_2 = cz_2, \text{ and } \tan\theta_1 = cz_1;$$

$$\therefore c = \frac{\tan\theta_2 - \tan\theta_1}{z_2 - z_1} = \cdot047925.$$

Comparing (22) with the form of this equation given in (4), $z^2 = kr\phi$, we have $y' = r\phi$ and $k = \frac{2}{c} = 417\cdot3$.

Hence the equation to the development of the parabolic rifling is

$$z^2 = 417\cdot3 r\phi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

and z_1 the distance of the origin from the commencement of the rifling $= \frac{\tan\theta_1}{c} = 6\cdot555$ feet.

25. As in the last case, I place in the form of a Table the results given by (15) for different values of z. The values of the constants are, $r = \cdot417$ feet, $k = 417\cdot3$, $\rho = \cdot312$ feet, $\mu_1 = \cdot167$, $M = \cdot00555$.

Table showing the pressure on the studs in a 10-*inch British-service gun rifled with a parabolic twist commencing at one turn in* 100 *calibres and terminating at one turn in* 40 *calibres, calculated from* (15).

Value of z, the distance from the origin, in feet.	Corresponding travel of the shot in the bore, in feet.	Corresponding velocity of shot, in feet.	Total pressure on base of shot, in tons.	Value of R, or total pressure on studs, in tons.
6·555	0·000	0	0	0
6·888	0·333	411	1547	31·2
7·500	0·945	675	1077	28·7
8·389	1·834	873	781	29·0
9·278	2·723	992	621	30·2
10·167	3·612	1078	510	31·4
11·055	4·500	1146	424	32·3
11·944	5·389	1200	356	33·0
12·833	6·278	1245	305	33·8
13·722	7·167	1282	268	34·5
14·610	8·055	1311	240	35·2
15·499	8·914	1333	220	35·8
16·388	9·833	1349	205	36·3

26. From an examination of the values of R given in this Table, it will be seen that the total pressure on the driving-surface reaches about 31 tons shortly after the commencement of motion, and the projectile quits the bore with a pressure of about 36 tons. With the view of making the variations which the pressures undergo more readily comparable, I have laid down in Plate VI. the curves derived from equations (15) and (17) for the battering charge of pebble-powder.

From these diagrams the pressures on the driving-surface at any point of the bore, both for the uniform and parabolic twists, can be seen by simple inspection. The line of abscissæ gives the travel of the shot, and the ordinates give the corresponding *total pressure* on the studs.

The curves show that with the uniform spiral the pressure on the studs reaches nearly 70 tons after a travel of ·3 feet, rapidly falling to about 9 tons at the muzzle, while with the parabolic rifling the pressure at ·3 feet of travel, corresponding to the point of maximum pressure, is only 31 tons. The pressure then falls slightly and amounts to 28 tons at about 1 foot travel; thence it gradually increases to a maximum of 36 tons at the muzzle.

By way of comparison, I have added in the Plate a curve showing the pressures required to give rotation to a 400-lb. projectile fired from the 10-inch gun with uniform twist when R. L. G. instead of pebble-powder is used.

The curve in this case is of the same nature as that derived from the pebble-powder; but the variation is greater, the maximum pressure being much higher and the muzzle-pressure, owing to the smaller charge, somewhat less.

27. To one more point it is worth while to call attention.

If the gun were a smooth-bore gun, the equation of motion would be

$$M\,.\,\frac{d^2z}{dt^2}=G'; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (24)$$

and comparing this equation with (10), we have, on the supposition * that the velocity increments in both cases are equal,

$$G=G'+R\,.\left\{\frac{2z\,.\,\sin\delta}{\sqrt{4z^2(\sin\delta)^2+k^2}}+\frac{\mu_1 k}{\sqrt{4z^2+k^2}}\right\}, \,. \qquad (25)$$

or, in the case of the Woolwich gun, where $\delta=90°$,

$$G=G'+R\,.\left\{\frac{2z+\mu_1 k}{\sqrt{4z^2+k^2}}\right\}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (26)$$

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* Were the velocity increments not supposed equal, the reduction of pressure due to the suppression of rifling would be less than that given in the text.

and the interpretation of these equations is that the gaseous pressure in the rifled guns (rifled with the parabolic twist) is greater than that in the smooth-bored gun by the second term of the right-hand member of the equation.

28. The corresponding equations for a uniform twist are

$$G=G'+R\left\{\frac{\sin\delta}{\sqrt{k^2+(\sin\delta)^2}}+\frac{\mu_1 k}{\sqrt{1+k^2}}\right\}. \qquad (27)$$

or, if $\delta=90°$,

$$G=G'+R\left\{\frac{\mu_1 k+1}{\sqrt{1+k^2}}\right\}. \qquad (28)$$

29. I shall now put these results in actual figures, and shall again take for illustration the 10-inch gun, supposed (as before) to be rifled, 1st, on the uniform, 2nd, on the parabolic or service twist.

With the uniform twist, G (see Table) = 1547 tons; and using equation (28) and the values of the constants given in 22,

$$G'=G-{\cdot}245R$$

$$={\cdot}989G. \qquad (29)$$

Hence the decrement of pressure due to the suppression of rifling is only about 1 per cent.; that is, the total pressure on the base of the shot is reduced from 1547 tons to 1530 tons, or the bursting pressure is reduced from 19·7 tons per square inch to 19·5 tons per square inch.

At the muzzle of the gun in the same manner we find that the total pressure is reduced from 205 tons to 202·8 tons, and the pressure per inch in a corresponding proportion.

30. Similarly, from equation (26) and the values of the constants given in 25, the values of G' at the point of maximum pressure and at the muzzle of the gun are obtained; and I find that with the parabolic twist the pressure on the base of the shot would be reduced from 1547 tons to 1541 tons, or the bursting pressure would be reduced from 19·7 tons to 19·62 tons per square inch.

At the muzzle the corresponding reductions are from 205 tons total pressure, to 196 tons, or from 2·61 tons to 2·49 tons per square inch.

31. For the sake of clearness, I recapitulate the results at which I have arrived. They are as follow:—

1st. That the pressures actually exerted at all points of the bore to give rotation to the 10-inch British-service projectile, compared with the pressures which would be exerted were the gun rifled on a uniform twist, are very approximately exhibited in the diagrams on Plate VI.

2nd. That in the 10-inch gun (and other guns similarly rifled) the pressure on the studs due to rifling is but a small fraction (about 2¼ per cent.) of the pressure required to give translation to the shot.

3rd. That the substitution of the parabolic for the uniform rifling has reduced by about one half the maximum pressure on the studs.

4th. That the increment of the gaseous pressure, or the pressure tending to burst the gun, due to rifling is exceedingly small*, both in the case of the uniform and parabolic rifling. This result is entirely confirmed by the experiments of the Explosive Committee, who have found no sensible difference of pressure in the 10-inch gun fired in the rifled and unrifled states.

5th. That, small as the increment in gaseous pressure due to rifling is, it is still less in the parabolic than in the uniform system of rifling.

Elswick Works, February 15, 1873.

XXVII. *Note on the Measure of Intensity in the Theories of Light and Sound.* By R. H. M. Bosanquet, *Fellow of St. John's College, Oxford*†.

MR. MOON, and more recently Dr. Hudson, have, in the Philosophical Magazine, controverted the position that the energy of the forms of motion, which constitute light and sound, with factors depending on the wave-length or periodic time, is to be regarded as the measure of the intensity of these impressions on our senses. The matter is one for experimental evidence. Now direct evidence bearing on the point at issue is not wanting, in the case of either sound or light.

The question is whether the intensity is measured by the square of the amplitude, or by the amplitude, for given periodic times.

Mr. Moon has not offered any answer to the remark made at the end of my paper of last November, although, if he understood it, it is conclusive in the case of light. It may be worth while to mention the point again. There exists an experimental law known as the law of Malus, which connects the intensities of the two rays, polarized in planes at right angles to each other, into which a plane-polarized ray is decomposed by a doubly refracting crystal. The intensities of these two rays are proportional to the squares of the sine and cosine of the angle which the principal plane of the crystal makes with the original plane of polarization. The verification is stated to have been made photometrically by Arago; but it is easy to obtain a verification for one's self. If the ray that has passed through a Nicol fall through a hole on a crystal of Iceland spar of suitable thickness so that the emerging rays overlap, it is quite easy to recognize that, as the crystal is turned round, the intensity of the overlapping part is constant. Now the amplitudes of the polarized pencils will be

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* Although the increase of strain due to rifling is inconsiderable, yet the decrease of the strength of the structure of a gun inseparable from rifling may be, and in many systems is, considerable; but the discussion of this question is outside of the scope of my paper.

† Communicated by the Author.

$$a \cos \alpha \text{ and } a \sin \alpha,$$

if a be the amplitude of the original ray, and α the inclination of the principal plane of the prism to the plane of polarization of the Nicol. But if these amplitudes were the measures of the intensities, the intensity of the overlapping part would be represented by $a(\sin \alpha + \cos \alpha)$, and would vary with α. The question is here submitted to direct experiment; and we see that it is only by taking the intensity to be measured by the square of the amplitude that the experimental result can be accounted for; we have then, of course, $a^2(\sin^2 \alpha + \cos^2 \alpha) = a^2$.

In the case of sound, the law of Töpfer, which is quoted in my paper in the Philosophical Magazine, November 1872, shows that in organ-pipes of equal intensity the wind consumed is proportional to the wave-length. As I have remarked, this is equivalent to saying that the work done is proportional to the wave-length. Töpfer's law is beyond dispute. It might well have been left to rest on his authority, except that, for once, he gives no measures or evidence for it; but he is generally so accurate that his enunciation of the law carries weight. The evidence in the case of sound does not at present amount to actual proof of the representation of intensity by mechanical energy, but to a high degree of probability. I have devised a better method of observation, which requires special arrangements, and hope to throw some additional light on the subject when I have an opportunity of carrying this out.

As one of Mr. Moon's suggestions is sometimes felt as a difficulty by learners, I will just touch upon it. It seems specious to say that, if we superpose two luminous or sonorous vibrations of the type $y = a \sin \frac{2\pi}{\lambda}(vt - x)$, the amplitude of the resulting vibration will be $2a$, and the intensity four times that of either vibration. But if we draw from this the conclusion that from twice the energy proportional to a^2 we obtain energy proportional to $4a^2$, we clearly make a mistake. In the first place, if the two vibrations are to be superposed they must be in the same phase; this requirement prevents the occurrence of the superposition in nature in many cases. Thus, if two similar organ-pipes are sounded near each other in exact unison, they always arrange their vibrations in opposite phases, and the two tones destroy each other. No doubt also the explanation given by Airy of the doubling of intensity by use of two candles points in the correct direction. As, however, it takes no account of the alteration of the plane of polarization in common light, a matter of which we know scarcely any thing, it can only be regarded as an illustration. The complete explanation cannot be given or recognized till we know more about the alteration of the plane of polarization. We must note, however, that the doubled light of two candles is not to be regarded as an axiom, but as a fact ascertained in the every-day processes of photometry.

Still it may often happen that two such vibrations may be superposed with coincident phase; it happens, for instance, at the loudest part of the beats given by two organ-pipes nearly in unison. In these cases we must not treat each vibration as a cause in itself, invariable under all conditions. If we regard the two vibrations as unaltered by the superposition, we shall in general be wrong. We must go back to the sources of energy; and we shall in general find that the delivery of the vibrations is materially affected by their superposition. By the superposition of the vibrations we increase the work to be done by each, if considered to remain unaltered. Take an illustration from electricity; then it is like interposing an additional resistance in an electric circuit, only that heat is developed instead of sound. If the battery be arranged for quantity, as it is called, the current may be materially affected by the new resistance; if for tension, the new resistance may have a small influence only. So with an organ-pipe. Let two pipes be nearly in unison, and at the swell of the beat, when the vibrations are superposed with the same phase; then the state of things may vary a good deal, but necessarily lies between the following extreme cases.

First, let the wind be very light, passing with freedom through the windway, and wasting little of its pressure there on the overcoming of friction, imparting velocity to the external air, and so on; then most of the pressure at the windway is due to the work converted into sound. The pressure at the orifice, and thence the rate of issue, are then different as more or less work is converted into sound; when the work to be done is increased, the pressure at the windway is increased and the velocity lessened. Thus on superposition the amplitude of each vibration will be diminished, until the work of the compound vibration is such as the sources are able to furnish; and in the extreme case each amplitude would be diminished in the ratio of $1 : \sqrt{2}$. This is analogous to the introduction of a resistance into an electrical circuit of small resistance, or where the battery is arranged for quantity.

Secondly, if the organ have a heavy wind, and the pipes have narrow windways, so that the greater portion of the pressure at the windway is due to friction, velocity communicated to the external air, &c., and only a small part to the reaction of the sound-impulses, then even considerable variations of the work transformed into sound will not alter perceptibly the pressure at the windway or the velocity of exit: thus, on superposition, the two vibrations would retain their form, and the intensity of the swell of the beat would be in the extreme case four times that of the single vibration. This case is analogous to the introduction of a resistance into an electric circuit whose resistance is already great, or where the battery is arranged for tension.

XXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 148.]

December 5, 1872.—Rear-Admiral G. H. Richards, C.B., Vice-President, in the Chair.

THE following communication was read:—

"Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass." By James Stuart, M.A., Fellow of Trinity College, Cambridge.

From investigations given by Ampère, we can deduce an expression for the potential U at an external point Q of a closed circular galvanic current carried by a wire of indefinitely small section. Let a be the radius of the circle; let the distance of Q from C, the centre of the circle, be r; and let the line C Q make an angle θ with the normal to the plane of the circle. Then it can be shown that when r is less than a,

$$U=2\pi k\left\{-1+\frac{r}{a}P_1-\frac{1}{2}\frac{r^3}{a^3}P_3+\frac{1.3}{2.4}\cdot\frac{r^5}{a^5}\cdot P_5-\ldots\right\};$$

and when r is greater than a,

$$U=2\pi k\left\{-\frac{1}{2}\frac{a^2}{r^2}P_1+\frac{1.3}{2.4}\cdot\frac{a^4}{r^4}P_3-\frac{1.3.5}{2.4.6}\cdot\frac{a^6}{r^6}P_5+\ldots\right\},$$

where k depends only on the intensity of the current, and where P_1, P_2, P_3 are defined by the equation

$$\frac{1}{\sqrt{1-2x\cos\theta+x^2}}=1+P_1x+P_2x^2+P_3x^3+\ldots.$$

If, therefore, X represents the resolved part perpendicular to the plane of the circle and towards it of the force exerted by the current on a unit of magnetism placed at Q, and if Y represent the resolved part of that force parallel to the plane of the circle and directed from its centre outwards, then

$$X=\frac{dU}{r\,.\,d\theta}\sin\theta-\frac{dU}{dr}\cos\theta,$$

$$Y=\frac{dU}{r\,.\,d\theta}\cos\theta+\frac{dU}{dr}\sin\theta.$$

To calculate these quantities, we know that

$$P_1=\cos\theta,$$

$$P_3=\frac{5}{2}\left(\cos^3\theta-\frac{3}{5}\cos\theta\right),$$

$$P_5=\frac{63}{8}\left(\cos^5\theta-\frac{10}{9}\cos^3\theta+\frac{15}{63}\cos\theta\right).$$

We shall only consider the case of those points for which r is greater than a. Substituting these values in the expression which in such instances holds for U, we have

$$\begin{aligned}U=2\pi k\Big\{&-\frac{1}{2}\cdot\frac{a^2}{r^2}\cos\theta+\frac{15}{16}\cdot\frac{a^4}{r^4}\left(\cos^3\theta-\frac{3}{5}\cos\theta\right)\\&-\frac{315}{128}\cdot\frac{a^6}{r^6}\left(\cos^5\theta-\frac{10}{9}\cos^3\theta+\frac{15}{63}\cos\theta\right)\\&+\ldots\Big\}.\end{aligned}$$

From which, after some reduction, we obtain

$$\begin{aligned}\frac{X}{2\pi k}=&-\frac{1}{2}(-1+3\cos^2\theta)\,.\,\frac{a^2}{r^3}+\frac{1}{16}\,.\,(9-90\cos^2\theta+105\cos^4\theta)\frac{a^4}{r^5}\\&-\frac{1}{128}(-75+1575\cos^2\theta-4725\cos^4\theta+3465\cos^6\theta)\frac{a^6}{r^7}\\&+\ldots\ldots\ldots\quad(1)\end{aligned}$$

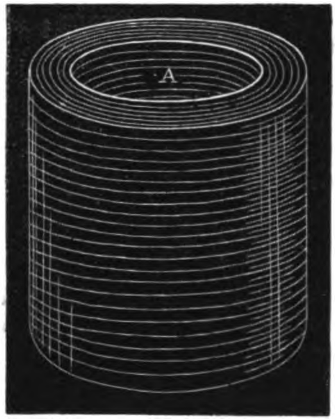
$$\begin{aligned}\frac{Y}{2\pi k}=\sin\theta\,.\,\Big\{&+\frac{3}{2}\cos\theta\,.\,\frac{a^2}{r^3}-\frac{1}{16}(-27\cos\theta+105\cos^3\theta)\frac{a^4}{r^5}\\&+\frac{1}{128}(525\cos\theta-3150\cos^3\theta+3465\cos^5\theta)\frac{a^6}{r^7}\\&-\ldots\ldots\Big\}\ldots\ldots\ldots\quad(2)\end{aligned}$$

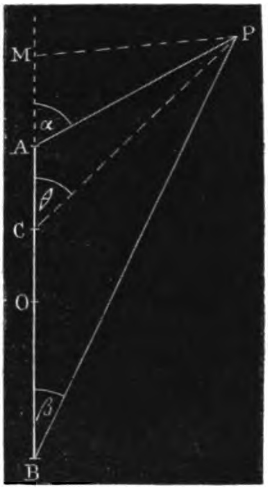
Each of these expressions consists of a series of terms in ascending powers of $\frac{a}{r}$ which will be converging.

We shall now seek to find X and Y for a galvanic current traversing a wire coiled into the form of a hollow cylinder, of which the internal radius is b, the external radius $b+c$, and of which the length is $2f$. We shall suppose the individual turns of the wire to lie so close that each may be regarded as an exact circle.

Let A B be the axis of the coil, so that A and B are the centres of its two faces; then A B$=2f$. Let O be the middle point of A B.

Let P be the attracted point, P M its perpendicular distance p from A B. Let P A M$=\alpha$, P B M$=\beta$.





Let C be the centre of any turn of the wire regarded as a circle of radius a, CP$=r$, PCM$=\theta$, OC$=x$; then it is readily seen that for the whole cylindrical bobbin the forces X, Y are given by

$$\frac{\mathrm{X}}{\mu}=\int_{-f}^{+f}\int_{b}^{b+c}\mathrm{L}\,dx\,da,$$

$$\frac{\mathrm{Y}}{\mu}=\int_{-f}^{+f}\int_{b}^{b+c}\mathrm{M}\,dx\,da,$$

where L and M stand for the expressions on the right-hand side of (1) and (2) respectively, and where μ depends on the strength of the current.

To perform the integrations for the length of the bobbin in these expressions, we have the formulæ

$$p=r\,.\sin\theta,$$

$$\delta x\,.\sin\theta=-r\,.\,\delta\theta\,;$$

$$\therefore\ \delta x=\frac{-p\,\delta\theta}{\sin^2\theta},$$

and

$$r=\frac{p}{\sin\theta}.$$

Making these substitutions for δx and r, the integrals with respect to x become integrals with respect to θ, which can be easily evaluated by a continued application of the method of integration by parts, the limits being from $\theta=\alpha$ to $\theta=\beta$. If we then integrate the result thus obtained with respect to a, from the limit b to the limit $b+c$, we finally obtain

$$\begin{aligned}\frac{\mathrm{X}}{\mu}=&\frac{\overline{b+c}^3-b^3}{6p^2}\{-(\cos\beta-\cos\alpha)+(\cos^3\beta-\cos^3\alpha)\}\\ &+\frac{\overline{b+c}^5-b^5}{80p^4}\{-9(\cos\beta-\cos\alpha)+33(\cos^3\beta-\cos^3\alpha)\\ &\qquad-39(\cos^5\beta-\cos^5\alpha)+15(\cos^7\beta-\cos^7\alpha)\}\\ &+\frac{\overline{b+c}^7-b^7}{896p^6}\{-\ \ 75(\cos\beta-\cos\alpha)\ +\ 575(\cos^3\beta-\cos^3\alpha)\\ &\qquad-1590(\cos^5\beta-\cos^5\alpha)+2070(\cos^7\beta-\cos^7\alpha)\\ &+\ldots\ldots,\qquad-1295(\cos^9\beta-\cos^9\alpha)+\ 315(\cos^{11}\beta-\cos^{11}\alpha)\}\end{aligned}$$

$$\begin{aligned}\frac{\mathrm{Y}}{\mu}=&\frac{\overline{b+c}^3-b^3}{6p^2}\{+(\sin^3\beta-\sin^3\alpha)\}\\ &+\frac{\overline{b+c}^5-b}{80p^4}\{-12(\sin^5\beta-\sin^5\alpha)+15(\sin^7\beta-\sin^7\alpha)\}\\ &+\frac{\overline{b+c}^7-b^7}{896p^6}\{+120(\sin^7\beta-\sin^7\alpha)-420(\sin^9\beta-\sin^9\alpha)\\ &+\ldots\ldots\qquad+315(\sin^{11}\beta-\sin^{11}\alpha)\}\end{aligned}$$

These expressions for X and Y will be converging for all points situated at a greater distance than $b+c$ from any point of the axis A B, inasmuch as they are composed by adding together corresponding terms of series which are then all convergent. Among other points, these expressions hold for such as are situated on the axis external to the bobbin, and not nearer A or B than by the distance $(b+c)$. For such points, however, the expressions become illusory, assuming the form $\frac{0}{0}$. They may, however, be evaluated by the methods for the evaluation of vanishing fractions. Y is clearly zero. X may be more readily obtained directly from the expression for U. From that expression we find that for a single circular current the attraction on such points is

$$\mathrm{X}=2\pi k\left\{+\frac{a^2}{r^3}-\frac{3}{2}\frac{a^4}{r^5}+\frac{15}{8}\frac{a^6}{r^7}-\ldots\ldots\right\}.$$

Hence, in the case of a bobbin, if x be the distance of the attracted point from O, the middle point of the axis of the bobbin, we have

$$\begin{aligned}\frac{\mathrm{X}}{\mu}=&\int_{x+f}^{x-f}\int_{b}^{b+c}dr\,da\left(+\frac{a^2}{r^3}-\frac{3}{2}\frac{a^4}{r^5}+\frac{15}{8}\frac{a^6}{r^7}-\ldots\ldots\right)\\ =&-\frac{\overline{b+c}^3-b^3}{6(x^2-f^2)^2}(\overline{x+f}^2-\overline{x-f}^2)\\ &+3\frac{\overline{b+c}^5-b^5}{40(x^2-f^2)^4}(\overline{x+f}^4-\overline{x-f}^4)\\ &-5\frac{\overline{b+c}^7-b^7}{112(x^2-f^2)^6}(\overline{x+f}^6-\overline{x-f}^6)\\ &+\ldots\ldots,\end{aligned}$$

which gives X for points situated on the axis for which x is not less than $(b+c+f)$.

January 9, 1873.—William Sharpey, M.D., Vice-President, in the Chair.

The following communication was read:—

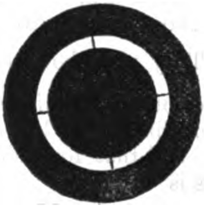
"On a new Method of viewing the Chromosphere." By J. N. Lockyer, F.R.S., and G. M. Seabroke.

The observations made by slitless spectroscopes during the eclipse of Dec. 11, 1871, led one of us early this year to the conclusion that the most convenient and labour-saving contrivance for the daily observation of the chromosphere would be to photograph daily the image of a ring-slit, which should be coincident with an image of the chromosphere itself.

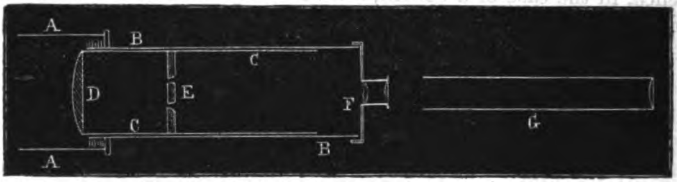
The same idea has since occurred to the other.

We therefore beg leave to send in a joint communication to the Royal Society on the subject, showing the manner in which this kind of observation can be carried out, remarking that, although the method still requires some instrumental details, which will make its working more perfect, images of the chromosphere, almost in its entirety, have already been seen on several days during the present month and the latter part of last month.

The adaptation of this method to a telespectroscope will be seen at a glance from the accompanying drawing.



Diaphragm showing annulus, the breadth of which may be varied to suit the state of the air.



The annulus is viewed and brought to focus by looking through apertures in the side of the tubes.

A. Sliding eye-tube of telescope. B. Tube screwing into eye-tube. C. Tube sliding inside B, and carrying lens D and diaphragm E. F. Lenses bringing image of diaphragm to a focus at the place generally occupied by the slit of the spectroscope. G. Collimator of spectroscope.

The image of the sun is brought to focus on a diaphragm having a circular disk of brass (in the centre) of the same size as the sun's image, so that the sun's light is obstructed and the chromospheric light is allowed to pass. The chromosphere is afterwards brought to a focus again at the position usually occupied by the slit of the spectroscope; and in the eyepiece is seen the chromosphere in circles corresponding to the "C" or other lines. The lens D is used to reduce the size of the sun's image, and keep it of the same size as the diaphragm at different times of the year; and the lenses F are used in order to reduce the size of the annulus of light to about ⅛ inch, so that the pencils of light from either side of the annulus may not be too divergent to pass through the prisms at the same time, and that the image of the whole annulus may be seen at once. There are mechanical difficulties in producing a perfect annulus of the required size, so one ½ inch in diameter is used, and can be reduced virtually to any size at pleasure.

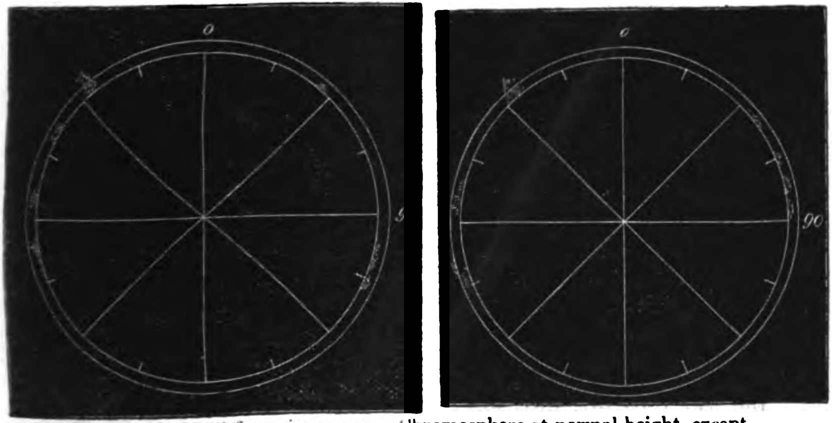
The proposed photographic arrangements are as follows:—

A large Steinheil spectroscope is used, its usual slit being replaced by the ring one.

A solar beam is thrown along the axis of the collimator by a heliostat, and the sun's image is brought to focus on the ring-slit by a 3¾-inch object-glass, the solar image being made to fit the slit by a suitable lens.

By this method the image of the chromosphere received on the photographic plate can be obtained of a convenient size, as a telescope of any dimensions may be used for focusing the parallel beam which passes through the prisms on to the plate.

The size of the image of the chromosphere obtained by the method adopted will be seen from the accompanying photograph, taken when the ring-slit was illuminated with the vapours of copper and cadmium.



December 6, 1872, at 11.30.

December 7, 1872, at 11.30.

Outer circle 100'' from inner one. Chromosphere at normal height, except where prominences marked.

As this photograph is not reproduced, it may be stated that the ring-images have an internal diameter of nearly ¾ of an inch.

The accompanying solar profiles are copies of drawings made, on the dates stated, by means of the new method, which were exhibited by the authors at the Meeting.

[Since reading the above paper, it has come to our knowledge that Zöllner had conceived the same idea unknown to us, but had rejected it. Prof. Wenlock in America has tried a similar arrangement, but without success.—J. N. L., G. M. S., January 17, 1873.]

January 16.—T. Archer Hirst, Ph.D., Vice-President, in the Chair.

The following communication was read:—

"A new Formula for a Microscope Object-glass." By F. H. Wenham.

A pencil of rays exceeding an angle of 40° from a luminous point cannot be secured with less than three superposed lenses of increasing focus and diameter, by the use of which combination rays beyond this angle are transmitted, with successive refractions in their course, towards the posterior conjugate focus: until quite recently, each of these separate lenses has been partly achromatized by its own concave lens of flint glass, the surfaces in contact with the crown glass being of the same radius, united with Canada balsam; the front lens has been made a triple, the middle a double, and the back again a triple achromatic. This combination therefore consists of eight lenses, and the rays in their passage are subject to errors arising from sixteen surfaces of glass.

In the new form there are but ten surfaces, and only *one* concave lens of dense *flint* is employed for correcting *four* convex lenses of *crown* glass: as this might at first sight be considered inconsistent with theory, a brief retrospect of the early improvements of the microscope object-glass will help to define the conditions. The knowledge of its construction has been entirely in the hands of working opticians; and the information published on the subject being scanty, this has probably prevented the scientific analyst from giving that aid which might have been expected.

Previous to the year 1829 a few microscopic object-glasses were made, composed of three superposed achromatic lenses; but this combination appears to have been used merely with the intention of gaining an increase of power, in ignorance of any principle, and without even a knowledge of the value of angular aperture.

At this time the late J. J. Lister tried a number of experiments, and discovered the law of the aplanatic focus, and proved that, by separating lenses suitably corrected, there were one or two positions in which the spherical aberration was balanced. This was explained in a paper read before the Royal Society in 1829. In the year 1831 Mr. Ross was employed to construct the first achromatic object-glass in accordance with this principle, which performed with "a degree of success never anticipated."

Mr. Ross then discovered that, after he had adjusted the interval of his lenses for the aplanatic focus, that position would no longer be correct if a plate of thin glass was placed above the object; this focus had then to be sought in a different plane, and the lenses brought closer together, in order to neutralize the negative aberration caused by covering-glass of various thickness. From this period the "adjustment" with which all our best object-glasses are now provided became established. Fig. 1 is the form of object-glass used at this time, consisting of three plano-concave achromatics, whose foci were nearly in the proportion of 1, 2, 3.

No greater angle than 60° could be obtained with this system in a $\frac{1}{8}$-inch objective (the highest power then made), for reasons apparent in the diagram. The excessive depth of curvature of the contact-surfaces of the front pair is unfavourable for the passage of the marginal rays; the softness of the flint glass forming the first plane was also objectionable. In the year 1837 Mr. Lister gave Mr. Ross a diagram for an improved "eighth," having a *triple front lens* in the form shown in fig. 2. By this the passage of extreme rays was facilitated; and in order to diminish the depth of curvature, a very dense glass was used, having a specific gravity of 4·351. Faraday's glass, having a density of 6·4, had been previously tried, but was abandoned on account of a difficulty in working it. The polished surfaces of both these qualities of dense glass speedily became tarnished by exposure to the air; and thus the dense flint concave could only be employed in a triple combination, that is, when cemented between two lenses of crown glass: this form of front was kept a trade secret, and was not published in any work treating of the optics of the microscope. The front incident surface of the flint of the middle pair was made concave, in order to reduce the depth of the contact; and for this reason only, as that surface has but little influence in correcting the oblique pencils, or in producing flatness of field, and may be a plane with an equally good or better result. "Eighths" of this form with angles of 80° were made, and remained unaltered till the year 1850, when larger apertures were called for, and Mr. Lister introduced the *triple back lens*.

The necessity for this will be seen by the diagram (fig. 2) which shows that the contact-surfaces of the back achromatic are too deep, thus giving great thickness to the lens and limiting its diameter: dense flint would have remedied this to some extent; but its liability to tarnish rendered its use in a *pair* objectionable. The highest density at this time known, quite free from this defect, was 3·686. By means of the triple back, the final corrections were rendered less abrupt, a greater portion of the marginal rays could be collected, and the aperture of an "eighth" was at once brought up to 130° or more.

At this time the author had been making some experiments in the construction of an object-glass in the form of fig. 2. Mr. Lister having favoured his "eighth" with an examination, was

Fig. 1.

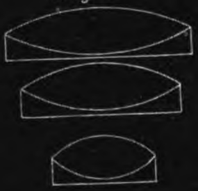


Fig. 2.

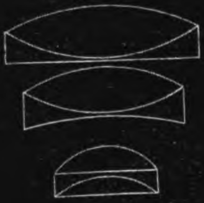
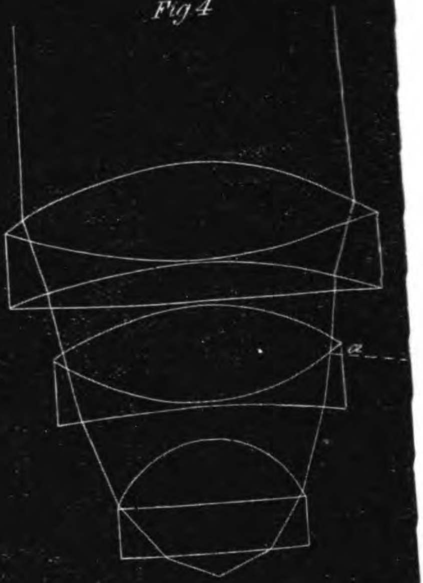


Fig. 4



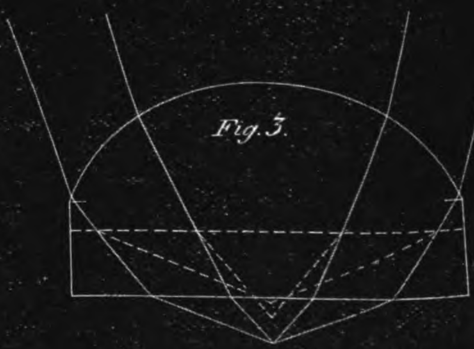


Fig. 3.

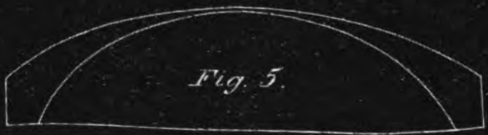


Fig. 5.

good enough to communicate his late improvement of the triple back. No time was lost in giving this a trial, the result of which proved that excessive negative aberration or over-correction could readily be commanded with lenses of shallow contact-curves. During these trials all chromatic correction was obtained by alterations in the triple *back*; for it was found that the colour-correction could not be controlled by a change in the concave surface of the triple *front*, as the negative power of the flint here appeared to be feeble, requiring a great difference in radius to give a trifling result. For this reason the front concaves were formed of very dense and highly dispersive flint; the cause of this was analyzed by a large diagram, with the passage of the rays projected through the combination, starting from the longest conjugate focus at the back. This proved that the rays from that focus passed through the concave flint of the front nearly as a radius from its centre, or in such a direction that its negative influence was almost neutralized. It is well known that a lens may be achromatic for parallel rays, and under-corrected for divergent ones. The utmost extent of this condition was apparent in the object-glass under consideration.

This led the author to the idea of the *single front* lens of crown glass, which gave a fine result at the first attempt, as the back combinations to which it was applied happened to have a suitable excess of negative or over-correction existing in the triple back alone, the middle being neutral or nearly achromatic. Still there was a defect remaining as positive spherical aberration; and this was afterwards cured by giving *additional thickness* to the *front* lens, which is now recognized as a most essential element of correction. In a "fifteenth," for instance, a difference of thickness of only ·002 of an inch will determine the quality between a good and an indifferent glass. Fig. 3 represents a front lens suitable for bringing the back rays to a focus. The dotted lines indicate the effect of this difference, showing that with a lens of less thickness the marginal rays fall within the central, producing positive aberration as the result.

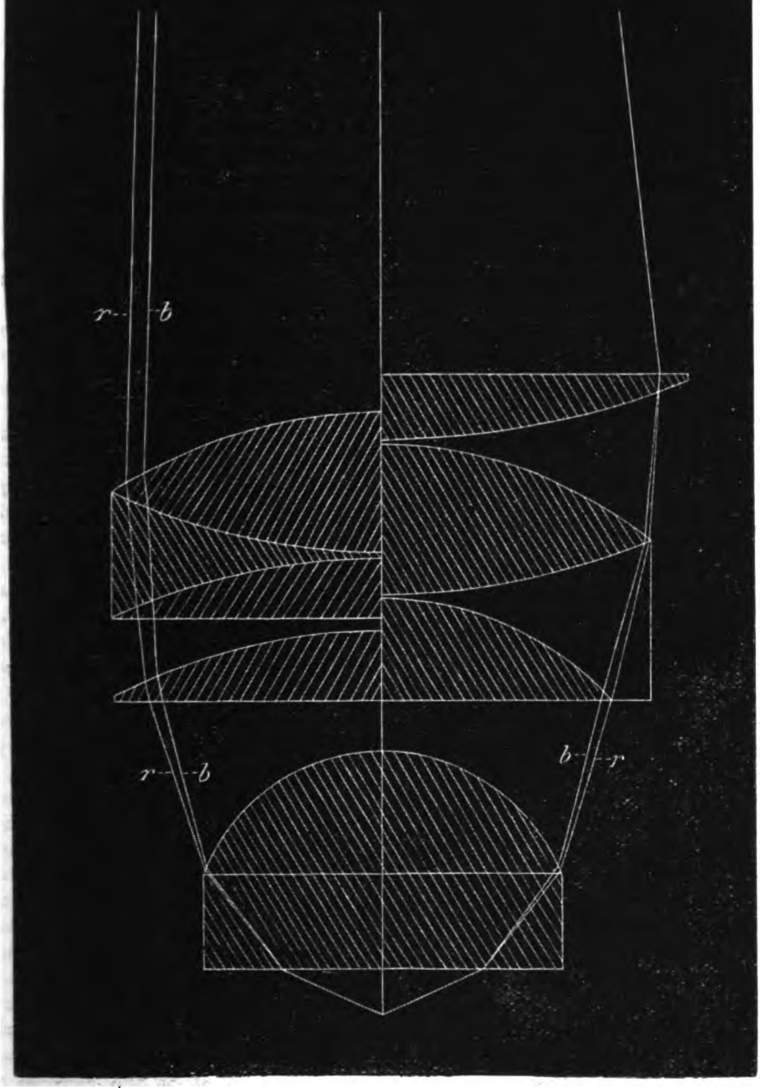
The single front introduced by the author is now used by every maker; for several years he could not induce the leading opticians to change their system, though challenged by a series of high powers constructed on this formula, for the purpose of proving its superiority. Fig. 4 represents the curves of the first successful "eighth" on this system, having an aperture of 130°, enlarged ten times. On tracing the passage of the marginal rays through the combination, it will be seen that, though the successive refractions are nearly equalized, the contact-surfaces of the middle pair are somewhat deep, though no over-correction existed or was needed here, for this would have required a shorter radius still (the density of the flint in this was 3·686). If this pair of lenses were not cemented with Canada balsam, total reflection would take place near the circumference of the contact flint surface, cutting off the marginal rays at *a*, and limiting the aperture. It might be argued that practically this would be no disadvantage, as these surfaces are united with Canada balsam, whose refraction is higher than the crown; so that the rays in this case must proceed with very little deviation. But incidences beyond the angle of total reflection may be considered detrimental, as they imply excessive depth of curvature; this can be discovered by looking through the front of an object-glass held close to the eye, any air-films in the balsam near the edge of the lens appearing as opaque black spots.

At the commencement of the present year the author caused a few object-glasses to be made, with a middle of the form of fig. 5, the performance of which was very satisfactory. In this the extreme rays pass at more favourable incidences, and within the angle of total reflection. The upper lens is of dense flint.

When the experiments on the single front were concluded, and the remarkable corrective power of the triple back in conjunction therewith had been proved, the next attempt was to make the *middle* also a single lens, leaving the entire colour-correction to be performed by the one biconcave flint in the back. After numerous trials it was found that though something like over-correction or negative aberration could be obtained with the back, in the degree requisite for balancing the under-correction of the single middle and front when set at the prescribed distance of the aplanatic focus, yet by trial on the mercury globule all the results invariably displayed two separated colour-rings: these could not be combined by any alteration in the radius of the lenses. By projecting the blue and red, or visible rays of greatest and least refrangibility through the system, the cause became apparent. The left-hand section of this object-glass is shown in fig. 6. The rays from the focus are slightly divided by the first front surface. On emerging from the back the separation is increased; the red ray (*r*) is outwards, and the more refrangible or blue ray (*b*) inwards. Next, the divergence of these two rays is extended by the middle single lens. The following crown lens extends the angle of divergence so far that the flint lens of the back triple cannot recombine them; and they emerge at two distinct zones, shown by the practical test of the "artificial star" or light-spot reflected from a mercury globule, viewed within and without the focus.

It might be supposed that these rays at their final emergence can be so refracted as to project the blue outwards. A crossing point would then occur at a fixed conjugate focus in the body of the microscope, at which all rays would be combined; and if this focus was adjusted to that of the eyepiece, achromatism and final correction would be the result. But to meet the various conditions occurring in the use of the microscope, the conjugate focus constantly alters in position, this being affected by every change of eyepiece, length of tube, or adjustment for thickness of cover; therefore a correction for a fixed point cannot be maintained. Achromatism in the microscopic object-glass, like that of other perfectly corrected optical combinations, must be the reunion of the rays of the spectrum close to the final emergent surface of the system. The remedy suggested by these experiments appeared to be in a transposition—that is, in placing the over-corrected triple

Fig. 6.



in the *middle* of the entire object-glass; this would at once cause a convergence of the blue and red rays. A single lens of longer focus at the back would then bring these rays parallel at the point of final emergence.

By projection in a diagram this condition was apparently realized. The dispersive power of the flint (density 3·686) was taken by the refractive index 1·76 of line H in the blue ray of the spectrum, and 1·70 of line B in the red ray. The refraction of the corresponding rays in the crown (density 2·44) was 1·53 H and 1·51 B. With these indices the rays are traced in fig. 6. The radii in the right-hand half section are those of an "eighth" of the new form drawn twenty times the size of the original. The *single front* is of the usual form, as this is much alike in all cases. The radius or focus of the *single plano-convex back* is about four and a half times that of the front, and the focus of the *middle* (triple) three times. The passage of the blue and red rays at the extreme of the pencil is shown in contrast with the preceding, the separation from the same front being alike.

The inner and outer, or blue and red rays, after passing the first surface of the triple middle, meet the concaves of the flint, which refract the blue rays to a greater extent than the red, and cause them to converge (instead of diverging, as in the opposing half diagram), so that at their exit from the triple they meet and would cross, effecting what is known as "over-correction;" but this is so balanced and readjusted by the single back of crown glass, that the rays are finally united, and emerge in a state of parallelism. This form of object-glass is suitable for the high powers, or such as have a cover adjustment, viz. from the "$\frac{1}{8}$-inch" upwards; perfect colour-correction is equally to be obtained in all of them.

It may be asked by some who have devoted their attention to the higher branches of optical mathematics, why the above result should have been worked out entirely by diagrams. But it has been found such a difficult task to calculate the passage of the two rays of greatest and least refrangibility through a combination having sixteen surfaces of glass of three different densities and refractions, that even first-class mathematicians have hitherto shrunk from the attempt.

Diagrams, however, are surprisingly accurate in their capability of indicating causes and results in the microscope and object-glass; for these lenses are minute, with deep curves and abrupt refractions; so that if the projection is worked out some fifty times the size of the original, small errors can be detected. The work should be commenced at the back from a long conjugate focus, which, not being a constant distance, may be taken as very near to parallelism. The high powers all have the means of correction within this distance, and perform better with a long posterior focus than with a very short one. The relative indices for the two or more rays should be marked on a large pair of proportional compasses, the long limb representing the sine of the angle of incidence, and the short one that of refraction. Both the sines ought to be set off in the diagram *behind*, and neither of them in front of the ray in course of projection; this leaves the way clear, with the least confusion of lines.

At the same time a second or counterpart diagram should be at hand, to which the rays only are transferred as soon as their direction is ascertained; with these precautions a mistake is scarcely possible.

Now it is hoped that some improvements may be effected by this investigation, on account of the simplicity attained in the combination, in which we have *two single lenses* of crown, whose foci bear a definite proportion to each other; while all the corrections are performed by *one concave of dense flint*, the acting condition of which is not altered by the influence of any other concaves acting in the combination, and hitherto taking a share of the duty. This one flint is now to be considered singly as the heart and centre of the system in reference to the correction of the rays entering and leaving.

This memoir is of necessity incomplete, for want of definite information concerning the optical properties of various kinds of glass. Data obtained from working them into small lenses furnish only a rough approximation to the mean dispersive power of the combined flint and crown having the best apparent effect. Of the intermediate rays, little can be known beyond the mere appearance of more or less of a secondary spectrum.

Nothing of importance has been published since Fraunhofer's Table, containing the refractive indices for each of the seven primary colour-lines of the spectrum for ten kinds of glass: great advance has been effected since that date in the manufacture of optical glass, a most complete collection of which of every variety has been made by the Rosses up to the present date. Selected specimens from this will be worked into prisms, and the relative spectra mapped out by the Fraunhofer lines, leading, it is hoped, to the discovery of a combination of crown and flint glass which shall be free from secondary spectrum or absolutely achromatic. The result of this investigation will be the subject of a future communication.

GEOLOGICAL SOCIETY.

[Continued from p. 152.]

June 19, 1872.—Prof. Ramsay, V.P.G.S., in the Chair.

The following communications were read:—

1. "On *Trochocyathus anglicus*, a new species of Madreporaria from the Red Crag." By P. Martin Duncan, M.B., F.R.S., V.P.G.S., Professor of Geology in King's College, London.

The author described a Coral of which a single specimen had been found in the Red Crag, in the grounds of Great Bealings Rectory, Norfolk. He stated that it belonged to the genus *Trochocyathus*, and was distinguished from the other species of that genus by its dense epitheca, its small and prominent columella, and its inverted calicular margin. He proposed to name it *Trochocyathus anglicus*, and stated that its nearest alliance is with the Australian Upper Tertiary form described by him under the name of *T. meridionalis*.

2. "On the Discovery of Palæolithic Implements in association with *Elephas primigenius* in the High-terrace Gravels at Acton and Ealing." By Col. A. Lane Fox, F.G.S.

The gravels in the neighbourhood of Acton have been divided by Mr. Prestwich into two principal groups, viz. the high-level gravels, on the hills above the valley, and the valley-gravels, on the sides and bottom of the valley itself. The valley-gravels have been again divided by Mr. Whitaker into three terraces, viz. a high terrace, between 50 and 100 feet above the Ordnance datum, a mid terrace, between 20 and 40 feet high, and a low terrace, at an average height of 10 feet, occupying the low ground in the bends of the river. On both sides of the river the high terrace is separated from the mid terrace by a strip of the London Clay, which is laid bare at an average level of 50 feet. The London Clay is also laid bare on the sides of the tributary streams running into the valley on both sides of the river, thus dividing the high-terrace gravel into patches. The mid terrace is continuous, and follows the sinuosities of the valley on both sides up to the strip of London Clay. The author accounts for this distribution of the gravels by supposing that a large body of water must at one time have stood at the 50-feet level, and the denudation of the high terrace have been caused by the waves beating on the sides of the valley, and by drainage into this body of water. The mid terrace he conceives may have been caused in part by accumulations beneath this body of water.

The position of the high-terrace gravel at Acton corresponded so closely to that of the implement-bearing gravels of the Somme and the Ouse, that the author was led to examine carefully the excavations made in it for the construction of houses. He discovered a number of implements of the drift-type, together with flakes and cores, and a few roughly formed scrapers; all these were found in close contact with the London Clay, and beneath the gravel. Fragments of fern (*Osmunda regalis*) and of wood (*Pinus sylvestris*) were also found with the implements at the same level. Two implements were found at Ealing Dean, 2 miles westward, on nearly the same level as those of Acton, viz. 90 feet; and these also came from the bottom of the gravel. Another implement was found south of the river at Battersea Rise, in the same position above the strip of London Clay as at Acton, and at about 60 feet above the Ordnance datum. The implements are of the pointed and oval types. The only animal remains discovered in the high terrace consisted of a tooth of *Elephas primigenius* in the Acton gravel. The position of this the author believes to be reliable, although he did not discover it himself *in situ*.

In the mid-terrace gravel a number of pits were examined between Shepherd's Bush and Hammersmith, and in the neighbourhood of Turnham Green, which resulted in the discovery, at the latter place, of a large quantity of animal remains (noticed by Mr. Busk in the following paper), all of which, like the implements of the high terrace, were at the bottom of the gravel; but no evidence of human workmanship was found in the mid terrace.

All these were found together, in the same seam of gravel, 12 feet beneath the surface; and all appeared to have been deposited at the same time. The surface was here 25 feet above the Ordnance datum, and consequently about 50 feet lower than the implements of the high terrace, 1½ mile to the north. The section across the valley, taken through the two places, here shows the strip of the London Clay intervening between the two terraces.

The chief points of interest which the author submitted to the judgment of geologists, consisted in:—the presence of drift implements in the high terrace, their absence in the mid terrace, and reappearance in the existing bed of the Thames; the great rarity or absence of animal remains in the high terrace, and their abundance in the mid terrace; and the occurrence of both implements and animal remains at the bottom of the gravel in both terraces. The writer concluded by adducing proofs of the great antiquity of the present river-bed, which it was shown must have run in its present meandering course in the bottom of the valley for at least 2000 years.

3. "On the Animal Remains found by Col. Lane Fox in the High- and Low-level Gravels at Acton and Turnham Green." By George Busk, Esq., F.R.S., F.G.S.

The author described the mammalian bones referred to in the preceding paper.

The remains from the High-level Gravels at Acton belong to the genera *Bos*, *Ovis*, *Equus*, and *Elephas*? The greater part belong to the first-named genus, and are probably modern, as are also those of *Ovis*. The remains of *Equus* may be of greater antiquity. The other bones found may belong either to Elephant, Rhinoceros, or Hippopotamus; they include a large portion of an Elephant's molar, and are much rolled.

The remains from the mid-level gravel at Turnham Green generally present the characters of great antiquity. They include bones of *Rhinoceros hemitœchus*, *Equus caballus*, *Hippopotamus major* (one of them the left frontal of a very young animal almost unworn), *Bos* (probably *B. primigenius*, and some perhaps *Bison priscus*), *Cervus* (*C. clactonensis*, Falc. = *C. Browni* Dawk., *C. elaphus*, and *C. tarandus*), *Ursus ferox priscus*, and *Elephas primigenius*.

4. "On the Evidence for the Ice-sheet in North Lancashire and adjoining parts of Yorkshire and Westmoreland." By R. H. Tiddeman, Esq., M.A. Oxon, F.G.S., of the Geological Survey of England and Wales.

The country of which the earlier glacial phenomena were described in this paper lies between the Lake-district on the north and the plains of South Lancashire and Cheshire on the south, and extends from the great watershed of England to the Irish Sea.

On the west is a sea-side plain rising to levels of less than 200 feet. On the north-east is a portion of the Pennine chain, comprising Ingleborough, Pennigent, and other Fells, rising to heights of from 2000 to 2400 feet. Between these, from south to north, we pass over:—1, a range of moorlands from 1000 to 1500 feet high, called the Rossendale Anticlinal, which forms the watershed between the basins of the Mersey and the Ribble; 2, the valley of the Burnley and Blackburn Coal-field, which drains north through gorges in (3) the Pendle chain of hills into (4) the broad valley of the Ribble; 5, a group of Fells rising to a general level of 1800 feet, between the valleys of the Ribble and the Lune, called, for the purposes of this paper, "The Central Fells;" 6, north of this the valley of the Lune and the estuary of the Kent. The main direction of all these features, between the sea-side plain and the Pennine chain, is from north-east to south-west.

The paper was illustrated by a map of the district on the scale of 1 inch to a mile, coloured to represent elevations, the level-contours having been reduced from the 6-inch scale. Upon this all the ice-scratches found on the solid rocks were inserted. A diagram illustrating the proportional number of scratches in different directions showed that 20 per cent. of them were due south, although the general direction of the valleys was to the south-west.

An instance was mentioned of a ridge 1400 feet in height, which had scratches at the top running directly across it to the south, although no land of equal height occurred north of it within a distance of 7 miles. A similar instance was shown to exist on the ridge north-east of Pendle Hill. A *roche moutonnée* in the gorge of the Calder at Whalley was shown to have been formed by ice working from the north, although the river drains from the south. Other systems of scratches were mentioned in detail. All these tended to show that, though the general slope and drainage of the district is to the south-west, the movement of the ice at the period of maximum cold was to the S. or S.S.E., or nearly parallel to the watershed.

The author goes on to describe certain disturbances at the surface of the rocks which dip at high angles to the south, they having been overturned by some force coming from the north. Such surface-disturbances are not found on rocks dipping to the north; and this fact may be explained by an illustration: in one case the brushing was with the nap, in the other against it. It was shown that these phenomena could not be attributed to any other agent but a great ice-sheet pushing on from its northern gathering-grounds, recruited by the greater elevations on its course, but overriding the lesser, grinding down and smoothing by its friction rocks presenting but a gentle incline, tearing up and turning over the basset edges confronting its approach.

The author next described the arrangement of the Till as to colour and material, and endeavoured to show that all the facts which he has observed are in favour of the existence of an ice-sheet travelling south in this district.

Mr. Cumming's observations in the Isle of Man were considered to confirm these views. He describes the general glaciation of the island as being from the E.N.E. or Lake-country, and describes many large blocks of granite which had been carried from their parent rock up the high hill of South Barruh and down the other side. This was referred by Mr. Cumming at the time to a great "wave of translation;" but the facts are quite easily explained by an ice-sheet. Other observations of Mr. Cumming upon the drifts of the Isle of Man were taken by the author as confirmatory of his views. Mr. Morton's observations on the glaciation of the Mersey basin were touched upon; and it was suggested that the glaciation of that district was produced by an ice-sheet, not coming from the south-east, as Mr. Morton holds, but working to the south-east from the Lake-country, and across a part of what is now the Irish Sea.

Professor Ramsay's observations on the glaciation of Anglesey being to the S.S.W. instead of from the Snowdon group, as might have been expected, were considered by the author to be confirmatory of his views of a great ice-sheet having filled what is now the Irish Sea, and emptied itself by St. George's Channel on the one hand, and by the Cheshire plain on the other, as well as by some of the passes in the Pennine Chain.

5. "On the Mammalia of the Drift of Paris and its Outskirts." By Prof. Albert Gaudry, F.C.G.S. (In a letter to W. Boyd Dawkins, Esq., M.A., F.R.S., F.G.S.)

In this paper the author briefly indicated those mammals the remains of which have been discovered in the Pleistocene or Quaternary deposits of Paris and its vicinity. His list includes flint implements as evidences of the existence of man, and bones of the following species:—*Canis lupus*, *Hyæna crocuta* (*spelæa*), *Felis leo* (*spelæa*), *Castor trogontherium* and *fiber*, *Elephas primigenius* and *antiquus*, *Hippopotamus amphibius*, *Rhinoceros tichorhinus* (a Rhinoceros of doubtful species), *Sus scrofa*, *Equus asinus* and *caballus*, *Bos primigenius*, *taurus*?, and *indicus*?, *Bison priscus* and *europæus*, and *Cervus tarandus*, *Belgrandi*, *megaceros*, *canadensis*?, *elaphus*, and a small species.

XXIX. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF A CONDUCTOR ARRANGED SYMMETRICALLY ROUND AN ELECTROSCOPE. BY CH.-V. ZENGER.

I HAVE the honour to address to the Academy the result of some fresh experiments on the electric inertia of a conductor arranged symmetrically round an electroscope.

Ruhmkorff found that if static electricity exercises no action on the electroscope disposed as I have indicated, it is not so with dynamic electricity or the electricity of induction.

This result is only a confirmation of my theory of electric inertia, since the condition of equal distribution (equal superficial tension), and symmetrical, is not fulfilled when induction-apparatus is used. In fact the tension of the current after the opening and after the closing of the inducing current is not the same, and the charge of the symmetrical conductor is successively positive and negative; the superficial tension cannot be none, nor even equal, since a certain time is required for the two electricities to combine after two alternate unequal discharges, considering the tension and the nature of the electricity. The tension at the part of the symmetrical conductor furthest from the point of discharge will be quite different from that at the part nearest to the conductor of the Ruhmkorff; and the condition of equal superficial tension at every point of the symmetrical conductor is not fulfilled. Failing this essential condition, there will be an action nearly equal to the difference of tension of the sparks of opening and of closing.

To show the influence of the symmetrical distribution, I put symmetrically round a gold-leaf electroscope a rectangular copper wire; the electroscope and the wire are placed upon the brass plate of another electroscope (with straws instead of gold leaves), larger and less sensitive. From the conductor of an electrical machine strong sparks go to one of the angles of the wire; the upper electroscope shows not a trace of tension, while the straws of the large electroscope below are strongly affected.

If the experiment be modified by placing the knob of the upper electroscope not symmetrically in relation to the middle points of the sides of the conductor, there will be seen a movement of the gold leaves at every discharge from the conductor of the machine. The greater this defect of symmetry, the more sensible will be the action.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxv. p. 1765.

ON THE HEAT OF TRANSFORMATION. BY M. J. MOUTIER.

A substance may present itself at the same temperature in two different states, which we will call M and M′. In passing from M to M′ a kilogramme of the substance absorbs a quantity of heat Q, which we will call *heat of transformation*. Let us suppose that in both conditions the substance can be vaporized, and that the tensions p and p' of the vapours at the same temperature are unequal; we propose to determine a relation between the heat of transformation Q and the tensions p and p'.

Let u and u' be the specific volumes of the substance in the states M and M′, v and v' the specific volumes of the vapours given by M and M′, L and L′ the heats of vaporization.

Let us conceive the following cycle of operations effected at a constant temperature:—

(1) The substance passes from the state M to M′ under a constant pressure π; it absorbs the quantity Q of heat. The quantity of heat consumed by external work is $A\pi(u'-u)$, A being the heat-

equivalent of the work; the heat expended on internal work is $Q-A\pi(u'-u)$.

(2) The body M′ is reduced to saturated vapour. The heat consumed by external work is $Ap'(v'-u')$; that consumed by internal work is $L'-Ap'(v'-u')$.

(3) The temperature being kept constant, the volume of the vapour formed is changed so that the pressure becomes equal to p; the internal work consumes the quantity q of heat.

(4) We condense the vapour under the constant pressure p; the body returns to the initial state M. The heat disengaged is L, of which the portion corresponding to the internal work is $L-Ap(v-u)$.

The cycle is closed; the change of internal heat is *nil*:

$$Q-A\pi(u'-u)+L'-Ap'(v'-u')+q-[L-Ap(v-u)]=0;$$

$$Q=L-L'+A\pi(u'-u)+Ap'(v'-u')-Ap(v-u)-q. \quad . \quad . \quad (1)$$

If we neglect the volumes u and u', if we assume that the internal work in the transformation of the vapour during the second operation is insensible, and, further, that the vapour follows Mariotte's law

$$pv=p'v', \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

we obtain the relation, in general sufficiently approximate,

$$Q=L-L'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The heat of transformation is equal to the difference of the heats of evaporation.

According to Carnot's theorem, if T denotes the absolute temperature of the body,

$$L=AT(v-u)\frac{dp}{dT}, \quad L'=AT(v'-u')\frac{dp'}{dT}. \quad . \quad . \quad . \quad (4)$$

Taking into consideration relation (2), and neglecting u and u' in comparison with v and v', we get another expression—

$$Q=ATvp\frac{d}{dT}\log\left(\frac{p}{p'}\right), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where log denotes a Napierian logarithm.

Let us apply these relations to a few examples.

Solution.—Let us consider a kilogramme of water and a sufficient weight of salt to give a saturated solution at the temperature T. Pure water has a vapour-tension p; that of the saturated solution is p'. The quantity of heat absorbed by the solution of the salt in the water is given by the preceding relations. Relation (5) was first pointed out by Kirchhoff[*].

Fusion.—We owe to M. Regnault a series of "Researches undertaken for the purpose of deciding if the solid or liquid state of bodies exercises an influence on the elastic force of the vapours emitted by them *in vacuo* at the same temperature." The conclusion deduced from M. Regnault's experiments is the following:—"that the molecular forces which determine the solidification of a substance do not exercise any sensible influence on the tension of its vapour *in vacuo*; or, more exactly, if an influence of this kind exists, the variations it produces are so slight that they could not be certainly established in our experiments." It must, however, be added that monohydrated acetic acid forms an exception; Regnault attributes this anomaly to impurity of the acid.

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[*] *Journal de Physique théorique et appliquée*, vol. i. p. 30.

Let us see what the preceding theory indicates in this case; we have to do with very small differences. We will take the exact formula*. Suppose the vapour-tensions equal in the two conditions, $p=p'$; then $v=v'$, $q=0$, and, taking into consideration equations (4),

$$Q=(u'-u)A\left(T\frac{dp}{dT}+\pi-p\right).$$

Take as an example the melting of ice at zero under the pressure of the atmosphere. The substance M represents the ice at zero; M', the liquid water at zero. The ice in melting absorbs heat; Q is positive, $u'-u$ negative, the parenthesis positive; therefore the hypothesis $p=p'$ conducts to an inadmissible conclusion.

Thus *ice and liquid water*, both at zero, *have different vapour-tensions*. The difference is very small; the approximate formula (5) shows it readily. The term $ATvp$ is very considerable in comparison with Q, so that p and p' differ very little. This result is entirely conformable to the latter part of M. Regnault's conclusion.

Allotropy.—According to the experiments of MM. Troost and Hautefeuille†, ordinary phosphorus at the temperature of 360° has a vapour-tension $=3{\cdot}2$ atmospheres. This vapour, under the prolonged action of heat, deposits red phosphorus; and the transformation ceases when the tension of the vapour takes the minimum value 0·6 atm., which MM. Troost and Hautefeuille have named *tension of transformation*. This minimum tension may be regarded as the maximum tension of the vapour of red phosphorus at the temperature of the experiment. Supposing, then, white phosphorus to correspond to the first state, M, red phosphorus to the second, M', we have

$$T=273+360,\quad p=3{\cdot}2 \text{ atm.},\quad p'=0{\cdot}6 \text{ atm.};$$

according to MM. Troost and Hautefeuille, at 440°, or

$$T=273+440,\quad p=7{\cdot}5 \text{ atm.},\quad p'=1{\cdot}75 \text{ atm.}$$

The weight of the litre of phosphorus-vapour which remained in the state of vapour at 360° after 240 hours of heating was 1·4 gramme; therefore the specific volume of the vapour of red phosphorus at that temperature is

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* *Comptes Rendus*, vol. lxxvi. p. 76. † Ibid.

$$v' = \frac{1}{1\cdot 4} \text{ cubic metre.}$$

If we apply these data of experiment to formula (5), taking into consideration equation (2), we find, for an approximate expression of the heat of transformation of white into red phosphorus,

$$Q = -17\cdot 5.$$

Thus, as M. Favre announced a little while after M. Schroetter's discovery, white phosphorus disengages heat in passing into the condition of red phosphorus. From an experiment by M. Hittorf, the transformation of liquid white phosphorus at 280° determines a sudden rise of the temperature from 280° to 370°. Designating by c the specific heat of the phosphorus the temperature of which thus rises, we should have $c \times 90 = 17\cdot 5$; from this we deduce $c = 0\cdot 19$, a number which differs little from the specific heat found by M. Regnault.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxvi. pp. 365–368.

ROYAL ASTRONOMICAL SOCIETY.

We were not a little disappointed on attending the Anniversary Meeting on the 14th of February to find that the Medal had not been awarded for the current year. The failure of the Council to find an astronomer whose attainments are such as to entitle him to become the recipient of the highest honour the Society has the power to bestow, suggests some important queries relative to the actual state of astronomy at the present time on the one hand, or to the condition of the Society which is, or ought to be, the representative of the science in England on the other. If we remember rightly, *one* medal only has been awarded during the last *three* years, and that to a foreign astronomer well deserving of it. Is there no English astronomer on whom the Society could gracefully and legitimately bestow it? Are the claims of our American brethren exhausted? or can no continental astronomer be found worthy to swell the list of medallists of the Society by the reception of its mark of highest approbation for services well and faithfully performed? We shall not attempt to reply to these queries. Those distinguished astronomers who take the lead in guiding astronomic thought in this country are well able to answer them. If, however, only one astronomer could be found (and we want but one annually) to whom the presentation of the Medal would alike confer honour on the Society and on the recipient, we should by the award take note of the continued onward progress of the Science; but as it is, we are in doubt as to whether astronomy is declining, or whether it is duly represented by the Society, which hitherto has held astronomical prestige in its hands. Of late years we have looked in vain for the choice spirits who held rule and sway at Somerset House when Baily, Sheepshanks, Herschel, and others sat in council; but few such spirits are now left to reflect the former glory of the Society; and the question remains, Is the Society now so constituted that it represents the true state of astronomy in England, and is the administration of its affairs such as to encourage, stimulate, and reward the patient worker in the humbler ranks of observers, and the veteran who has won his laurels which would gather lustre by the award of the Medal?

ON THE DETERMINATION OF THE BOILING-POINT OF LIQUEFIED SULPHUROUS ACID. BY M. IS. PIERRE.

In his very interesting memoir on sulphurous and chlorhydric acids (*Comptes Rendus*, Jan. 13, 1873), M. Melsens says that from 1860 he has been seeking to determine the exact boiling-point of liquefied sulphurous acid, that he has made very numerous trials with vessels of all sorts, but that all his attempts have been fruitless. Nevertheless, if content with an approximation to 0°·15 or 0°·2, it is very easy to determine the boiling-point of liquid anhydrous sulphurous acid by following the process which I pointed out twenty-six years since (*Annales de Chimie et de Physique*, ser. 3, vol. xxi.), in a memoir on sulphurous acid. This consists in pouring into a tube of *thin* glass, 2·5 to 3 centims. in diameter, having the form of a test-tube for gases, a certain quantity of sulphurous acid previously cooled—fitting to the aperture a cork pierced with two holes, one to give passage to the thermometer, the other larger, intended to give free passage to the vapour of the acid by means of a rather wide tube of thin glass—and, lastly, suspending the apparatus in the air. This is what then takes place:—The surrounding temperature being above that at which sulphurous acid boils, the latter is very soon in ebullition; but the heat rendered latent by its vaporization lowers the temperature of the remaining liquid, and produces an abatement of the ebullition. This is soon followed by a renewal; and thus a series of abatements and renewals of ebullition is observed, during which the differences of temperature indicated by the thermometer rarely reach 0°·2.

The limits are still more contracted when the deposition of moisture on the tube is avoided by covering with flannel the part containing the liquid. With from 25 to 30 grammes of liquid, if the operation takes place under favourable conditions, the experiment may often last more than an hour. I have constantly repeated it in my lectures for the last twenty-five years, on account of its facility.

I have thus found a number which differs very little from 8° below zero (Centigrade). The process, extremely simple, is applicable to all liquefied gases which can be kept in an *open* vessel,—that is to say, which in a certain time emit by ebullition a quantity of vapour that absorbs and renders latent an amount of heat equal to that received by the liquid from the surrounding medium—a condition from which results a temperature of spontaneous ebullition sensibly constant.—*Comptes Rendus de l'Académie des Sciences*, Jan. 27, 1873.

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[FOURTH SERIES.]

APRIL 1873.

XXX. *On Spectral Lines of Low Temperature.*

By The Marquis of Salisbury, *F.R.S.**

If one secondary pole of a powerful inductorium be connected with an insulated metal plate, the other pole being left unconnected, and a thermometer be fixed upright upon the plate, a green light will be visible in the vacuum above the mercury. In order to obtain the effect at its best, the battery should be slightly stronger than is necessary to produce the maximum spark between the secondaries of the coil used; and the plate should be completely insulated. By what kind of electric action this light is produced is not quite clear. Plücker and Mr. Gassiot, and others following them, speak of a similar light produced in a closed tube, without wire electrodes, as being caused by induction. The process appears to me more nearly to resemble conduction, the circuit being completed by leakage. At the point where the bulb rests upon the plate a discharge is visible, oxidizing the plate. At the other end of the thermometer a brush-discharge may (in the dark) be seen escaping. If a metal conductor be supported vertically parallel to the thermometer, with a slight interval between itself and the plate, and insulated at the other end, a similar discharge and similar, though more abundant, escape will be visible. If a piece of wax be inserted between the metal conductor and the plate, the resemblance will be closer still. As long as the conductor is there, the light in the thermometer will not appear, or will appear only by flashes. When the conductor is removed, the light returns to the thermometer. It appears, therefore, that the electric action on the thermometer is of the same kind as the action upon the conductor.

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* Communicated by the Author.

But whatever the process of electric excitement in the thermometer may be—whether by induction or by conduction through leakage—the exhibition of light without any but the minutest development of heat is worthy of notice. The heat caused by a full discharge through Geissler's tubes is well known. It will, as Wüllner notices, disintegrate the glass, producing sodium-, and at last even calcium-lines. If increased by resistance of gas of 500 millims. pressure, it will soften and bend aluminium electrodes. But in the thermometer, where the resistance is that of the thickness of a tube of glass, scarcely any, if any, rise of temperature is produced. With thermometers of the ordinary bore, I have failed, after many trials, to discover any alteration of reading at the end of five minutes' exposure to the discharge of the coil in the manner I have described. Four experiments with a thermometer of very fine bore, graduated to tenths of a degree Fahr., have given an average rise of three quarters of a degree Fahr. in the course of five minutes. I am doubtful whether even this rise is to be attributed to heat, and for this reason. In two other thermometers a small portion of the column of mercury was separated from the rest. The action of the coil after a short time made the interval wider, pushing up the separated fragment without expanding either portion; and in the interval so created a brilliant light appeared. It is evident, therefore, that the coil tends to produce a motion in the mercury of the thermometer analogous to that observed by Poggendorff in larger tubes; and the slight apparent rise of three quarters of a degree in five minutes, produced by the coil in a thermometer of very small bore, may be due to an action of this kind. But even if there be a real rise of temperature to this extent, it is so minute as to be practically unimportant. In effect, therefore, the light was produced at a temperature of less than 60° F.

This light is strong enough to be examined in the spectroscope. One prism, of course, only must be used; and the room must be perfectly dark. It is also necessary to use cross-wires in the telescope, as cross-hairs are too slender to be seen in so dim a light. Of course much the easiest mode of examination is to dispense with a slit altogether; but on that plan the lines of the spectrum, in some thermometers at least, are so rugged that it is difficult to identify them. I have therefore been compelled to use the slit. To my surprise, I found that different thermometers gave different lines; or rather, some gave more lines than others. Instruments made by the best makers, such as Casella or Elliott, give only the three following lines. All the lines observed have been identified by means of comparison with Geissler's tubes showing known spectra. The wave-lengths of the lines in the tubes so used for comparison I afterwards measured with a grating, and verified by Dr. Watts's catalogue. The light of the thermometer itself is too feeble to permit of direct measurement with a grating.

		$\lambda =$	Dr. Watts's No.
a a . .	orange	5768	53·6
c c . .	green	5460	63·3
h h . .	violet	4352	124·2

These are three of the brightest mercury-lines. The tube of comparison was one in which, previously to exhaustion, some mercury had been vaporized. I mention the grounds of my conclusion, because in Wüllner's experiments it was shown that mercury required considerable heat before its lines could be made to appear (Pogg. *Ann.* vol. cxxxv. p. 512). No one, however, who examines these lines will doubt that they are mercury-lines; and they certainly are produced without any appreciable rise of temperature. They appear in every thermometer I have examined. There was a fourth line (*d d*) in one of Casella's thermometers, apparently coincident with mercury-line No. 84·8 of Dr. Watts; but it was so feeble that the cross-wires could not be brought upon it with any certainty.

But in chemical thermometers purchased from six makers of slightly less repute than those above named, four other lines made their appearance in addition. These do not coincide with the mercury-lines, nor with the lines of a hydrogen-tube, nor with the lines of air; but they coincide exactly with some of the strongest lines of tubes containing carbon compounds. I find them in carbonic acid, in paraffin-oil vapour, in hydrocyanic acid, in cyanogen, in olefiant gas, in methyl, in coal-gas, and in carbonic oxide. Their wave-length has been verified with a grating as before.

Thermometer-line.		Dr. Watts's carbon spectra.	λ.
b b	(*j*)	58, yellow	5602
e e	(*k*)	74, green	5195
ff	(*l*)	92, blue	4834
g g	(*m*)	112, violet	4505

Many observers have been plagued by the intrusion of carbon-lines in various tubes where they had no place. Wüllner even assigns to carbon one of the various spectra which oxygen-tubes have been found to present. These carbon-lines are attributed by him to the grease surrounding stopcocks; by others (I think, by Mr. Crookes) to caoutchouc joints. These explanations are clearly not applicable to thermometers; and I think a simpler one, at least in some cases, may be offered. Every photographer must have observed the singular power which glass, exposed to an inhabited atmosphere, possesses of attracting not only moisture, but also thin films of grease. If a photographic glass plate be thoroughly cleaned and put aside for some days, it can very seldom be used at the end of that time without further treatment. Unless it be cleaned afresh with some solvent of grease, such as ether or strong acid, or rubbed with tripoli, it will probably show on the finished picture the streaks which betoken an imperfectly cleaned plate. What is true of a glass plate is probably true of the inner surface of a tube. Unless it is cleaned with something more powerful than water, it is pretty certain to retain a film of grease. I interpret these carbon-lines, therefore, as a simple proof of slovenly preparation. Their presence or absence may serve as an easy test, not of the skill, but of the care with which the thermometer has been made. I find that it depends more upon the reputation of the maker than on the price charged for the instrument. One of Elliott's that showed only mercury-lines is a common two-shilling instrument. In one costly, but apparently very dirty, thermometer—besides the four carbon-lines, one or two other very faint green lines showed themselves in the part of the spectrum near the solar line E; but they were too faint to be identified. In this thermometer, after frequent exposure to the action of the coil, a slight grey deposit was observable in the tube just over the point where the mercury ordinarily stood.

But if these carbon-lines show grease, why does the hydrogen present in grease not show its characteristic lines, especially the line F, which is the first seen of the hydrogen-lines? I believe the reason to be that hydrogen will not become luminous under electric influence at a low temperature, while the vapour of (combined) carbon will. This may be shown in a very simple way. Place a Geissler's tube, exhausted from some hydrocarbon compound (paraffin oil answers well), upright on the insulated plate connected with one secondary pole. Let a wire in connexion with the other pole be so arranged that it can be at will applied to or withdrawn from the upper end of the Geissler's tube while the coil is at work. First apply it, and let the full discharge pass. The line F and the carbon-line I have named *ff* will be seen close together with a deep-black space between them, giving the effect (in a one-prism instrument) of a groove with luminous edges. Withdraw the movable electrode; F will immediately disappear; but the carbon line *ff* will remain, little diminished in brightness. Approach the movable electrode to the top of the tube until a spark occasionally passes. Whenever the spark passes, the line F will shoot across the spectrum; but as long as there is no spark, only the carbon-lines will be visible. It appears to follow that the specific luminosity of carbon under electric excitement is much higher than that of hydrogen. If the light in a Geissler's tube be really produced exclusively by heat, as is commonly assumed, it follows that carbon at low pressure becomes luminous under the influence of very little heat. It is worth noting that with a broken circuit, such as I have described, the carbon-lines are distinct lines equally sharp on both sides. With a full circuit they are (in the prism) sharp only on one side, the least refrangible; on the other they show an expansion, a tendency to melt into the obscure continuous spectrum that lies behind them; on this side they much resemble the hydrogen-lines of the third spectrum, produced by intense heat. In short, the carbon-lines are visible when the hydrogen-lines have not yet appeared; the carbon-lines have begun to expand while the hydrogen-lines still remain sharp. Both substances pass through the same stages in their passage to the continuous spectrum to which all gases, by heat and pressure, can be ultimately reduced; but carbon begins the process first, and probably finishes it first; for it may be inferred that the spectrum of carbon vapour reaches the continuous condition proportionally sooner. I think I have seen it argued that there can be no carbon gases in the sun, because there are no carbon-lines. It has certainly been assumed that the carbon in a candle-flame must be in the solid state, because the spectrum is continuous. The above observations may tend to throw doubt on both these positions. If this carbon-spectrum is visible at a low temperature, it may reach the continuous condition, and consequently not be recognizable, at a temperature lower than the flame of a candle. Of course, if all carbon-spectra implied a similar temperature, the existence of the Bessemer-spectrum would negative such an idea. But Dr. Watts has shown that this is far from being the case.

I do not know whether it would be possible, by slight admixtures with the mercury of a thermometer, to ascertain the lowest temperature of luminosity in the case of other elements; but the investigation would be worth making if it is practicable.

XXXI. *On an advantageous Method of using the Differential Galvanometer for measuring small Resistances.* By OLIVER HEAVISIDE, *Great Northern Telegraph Company, Newcastle-on-Tyne*[*].

IN the usual method of measuring resistances with the differential galvanometer, the current from the battery is divided between the two coils, having opposite effects on the needle within them, so that, if the currents in both the coils are equal, the needle is unaffected. The introduction of resistance in the circuit of one coil will not affect the balance, provided an equal resistance is introduced in the circuit of the other coil. Hence, if on one side we place a rheostat, and on the other an unknown resistance, the latter may be determined by varying the resistance of the rheostat until a balance is obtained. Fig. 1 is a representation of this arrangement. The current from the battery, having a resistance *b* and electromotive force E, divides at the point *a* between the coil *g* and resistance *x*, and the coil *g′* and resistance *r*. When $r=x$, the needle is unaffected.

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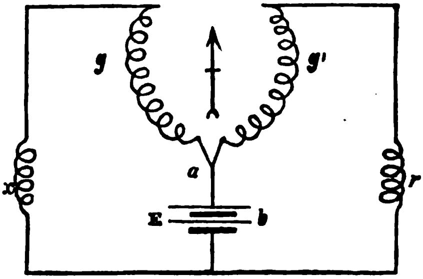
[*] Communicated by the Author.

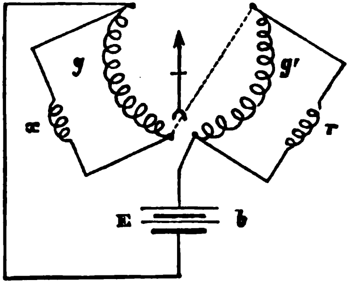
Fig. 1.



By the following method of using the differential galvanometer, a much greater accuracy is obtained when the unknown resistance whose value has to be determined is small.

Instead of dividing the current from the battery E between the two coils, I join up the coils, so that the same current passes through both of them, and by reversing one of the coils, *g′*, prevent the current from influencing the needle (see fig. 2). The rheostat *r* is connected to the two ends of one coil, and the resistance to be measured, *x*, to the two ends of the other. It will easily be seen, without further explanation, that when $r=x$, the currents in *g* and *g′* are equal; but should *r* not equal *x*, there will be a greater current in one coil than in the other, and the needle will move in obedience to the difference of these currents. It then only remains for me to show what, and under what circumstances, advantages are obtained by this method. To do so, we have only to compare the expressions for the difference-currents in the two methods.

Fig. 2.



By the first method the resistance external to the battery is

$$\frac{(x+g)(r+g)}{x+r+2g};$$

therefore the current from the battery is

$$B=\frac{E}{b+\frac{(x+g)(r+g)}{x+r+2g}}=\frac{E(x+r+2g)}{b(x+r+2g)+(x+g)(r+g)}.$$

This current divides between the two paths $x+g$ and $r+g$ in inverse proportion to their resistances; therefore the current in g is

$$G=B\times\frac{r+g}{x+r+2g}=\frac{E(r+g)}{b(x+r+2g)+(x+g)(r+g)},$$

and the current in g' is

$$G'=B\times\frac{x+g}{x+r+2g}=\frac{E(x+g)}{b(x+r+2g)+(x+g)(r+g)}.$$

The effective current (that influencing the needle) will be the difference of G and G′, say

$$D_1=\frac{E(r-x)}{b(x+r+2g)+(x+g)(r+g)}. \quad . \quad . \quad . \quad (1)$$

By the second method, the resistance external to the battery is

$$\frac{xg}{x+g}+\frac{rg}{r+g};$$

therefore the current from the battery is

$$B=\frac{E}{b+\frac{xg}{x+g}+\frac{rg}{r+g}}=\frac{E(x+g)(r+g)}{b(x+g)(r+g)+gx(g+r)+gr(g+x)}.$$

The current in g is

$$G=B\times\frac{x}{x+g}=\frac{Ex(g+r)}{b(x+g)(r+g)+gx(r+g)+gr(x+g)},$$

and the current in g'

$$G'=B\times\frac{r}{r+g}=\frac{Er(g+x)}{b(x+g)(r+g)+gx(r+g)+gr(x+g)}.$$

The effective current will therefore be

$$D_2=G-G'=\frac{Eg(x-r)}{b(x+g)(r+g)+gx(r+g)+gr(x+g)}. \quad . \quad (2)$$

Equations (1) and (2) give the effective current in each case; and we may ascertain the relative sensitiveness of the two methods by comparing D_1 and D_2.

$$\frac{D_2}{D_1}=\frac{b(x+r+2g)+(x+g)(r+g)}{b(x+g)(r+g)+gx(r+g)+gr(x+g)}\times g;$$

and in the limit, when $x=r$,

$$\frac{D_2}{D_1}=g\times\frac{2b+r+g}{b(r+g)+2gr}.$$

When $r=g$, $\frac{D_2}{D_1}=1$, showing that the two methods are equally sensitive for that value of r or x which equals the resistance of one coil of the galvanometer. When r is greater than g, the ordinary method is to be preferred, for $\frac{D_2}{D_1}$ is then less than unity. It can, however, never be less than $\frac{g}{b+2g}$, which happens when r is infinite.

But for values of r less than g, $\frac{D_2}{D_1}$ is greater than unity, and increases rapidly as r is reduced, until in the limit, when $r=0$, $\frac{D_2}{D_1}=2+\frac{g}{b}$.

This proves that when the resistance to be measured is smaller than that of the galvanometer-coil, the second method is much to be preferred. For instance, let the battery have a resistance of 10 ohms, the galvanometer (each coil) 500 ohms, and $r=10$ ohms, then the second method is 17 times as delicate as the first; and if r were 1 ohm, the second method would be 416 times as delicate.

In fact, if, after getting as true a zero as possible by the ordinary method, the connexions be altered to the second arrangement, the slight inequality between r and x, which was inappreciable by the ordinary method, will be at once rendered evident by a large deflection of the needle.

XXXII. *On the Optics of Mirage.* (Second Paper.) *By* Professor EVERETT, *M.A., D.C.L., Queen's College, Belfast*[*].

XI. WE proved in the preceding paper that wherever a plane of maximum index exists, the surfaces-of-equal-index being parallel planes, the law of index-variation in its immediate neighbourhood must in general be

$$\frac{d\log\mu}{dy}=-\frac{y}{a^2},$$

and that this law implies the existence of conjugate foci at the mutual distance πa.

It may be proved in like manner that, when the surfaces-of-equal-index are circular cylinders having for their common axis a line of maximum index, the law of index-variation in the immediate neighbourhood of this axis must in general be

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[*] Communicated by the Author.

$$\frac{d\log\mu}{dr}=-\frac{r}{a^2},$$

r denoting distance from the axis. Hence it is clear, from Sections II. and III. of the preceding paper, that rays diverging, at small inclinations to the axis, from any point in or near it, and lying in one plane which also contains the axis, will converge to a series of conjugate foci, whose common distance measured parallel to the axis is πa. We shall now show that this property is not confined to rays lying in the same plane with the axis, but extends to all rays of small inclination to the axis.

Employing rectangular axes of x, y, z, and making the axis of x coincide with the line of maximum index, it follows*, from the supposed smallness of the inclination of the rays to this line, that $\frac{d^2x}{ds^2}$ is negligible in comparison with the curvature $\frac{1}{\rho}$, and that, to the same degree of approximation,

$$\frac{d^2y}{ds^2}=\frac{d^2y}{dx^2},\quad \frac{d^2z}{ds^2}=\frac{d^2z}{dx^2}.$$

The radius of curvature ρ is therefore given by the equation

$$\frac{1}{\rho^2}=\left(\frac{d^2y}{dx^2}\right)^2+\left(\frac{d^2z}{dx^2}\right)^2;$$

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* Let α denote the inclination of the ray, at point x, y, z, to the axis of x; then we have

$$\frac{d^2x}{ds^2}=\frac{d}{ds}\frac{dx}{ds}=\frac{d}{ds}\cos\alpha=-\sin\alpha\frac{d\alpha}{ds}.$$

But $\frac{d\alpha}{ds}$ is of the same order of magnitude as $\frac{1}{\rho}$; hence $\frac{d^2x}{ds^2}$ is very small in comparison with $\frac{1}{\rho}$—that is, with the square root of

$$\left(\frac{d^2x}{ds^2}\right)^2+\left(\frac{d^2y}{ds^2}\right)^2+\left(\frac{d^2z}{ds^2}\right)^2.$$

We may therefore write

$$\frac{1}{\rho^2}=\left(\frac{d^2y}{ds^2}\right)^2+\left(\frac{d^2z}{ds^2}\right)^2.$$

The general expression for $\frac{d^2y}{ds^2}$ is

$$\frac{d^2y}{dx^2}\left(\frac{dx}{ds}\right)^2+\frac{dy}{dx}\frac{d^2x}{ds^2},$$

of which the first term is, in the present case, equal to $\frac{d^2y}{dx^2}$, and the second vanishes.

and its direction-cosines (regarding it as drawn *from* the centre of curvature) are

$$0, \quad -\rho\frac{d^2y}{dx^2}, \quad -\rho\frac{d^2z}{dx^2}.$$

Now the line r is in the osculating plane of the ray at the point x, y, z, because the osculating plane always contains the direction of most rapid change of index; hence the lines r and ρ are coincident, their common direction being defined by the intersection of the osculating plane with a plane perpendicular to the axis of x. Their direction-cosines are therefore equal; that is,

$$\frac{y}{r}=-\rho\frac{d^2y}{dx^2}, \quad \frac{z}{r}=-\rho\frac{d^2z}{dx^2}.$$

But we have

$$\frac{1}{\rho}=-\frac{d\log\mu}{dr}=\frac{r}{a^2}.$$

Therefore

$$\frac{d^2y}{dx^2}=-\frac{y}{r}\frac{1}{\rho}=-\frac{y}{a^2}; \quad \frac{d^2z}{dx^2}=-\frac{z}{a^2}.$$

But it is clear, from Section II. of the previous paper, that the general differential equation of a ray in the plane xy is $\frac{d^2y}{dx^2}=-\frac{y}{a^2}$. Hence the projection of a ray upon the plane xy is identical with the path of a ray in the plane xy; or, to state the same thing in general terms, the projection of a ray upon any plane containing the axis of x is identical with the path of a ray in this plane.

The equations of the projections of a ray upon the planes xy, xz are

$$y=b\sin\frac{x-c}{a}, \quad z=b'\sin\frac{x-c'}{a},$$

b, c, b', c' being arbitrary constants. They show that the ray is in general a helicoid or flattened helix, capable of being inscribed in a rectangular tube whose section has the length and breadth $2b$ and $2b'$.

As the substitution of $x+\pi a$ for x merely changes the signs of y and z, every object in the neighbourhood of the axis of x will yield a series of real images alternately inverted and erect, their common distance apart measured parallel to the axis of x being πa. The pencil of rays proceeding from any point to its conjugate will in general consist of helicoids, both right-handed and left-handed, of every degree of flatness, from the true helix to the plane curve of sines. If the point is on the axis of x, all the rays will be plane; and even if the surfaces-of-equal-index be not circular cylinders, provided only they be surfaces of revolution about the axis of x, rays emanating from a point on this axis will converge to a geometrical focus also situated upon it, as proved (for rays in one plane) in the previous paper.

XII. The path of a ray under the conditions treated in last section is the same as that of a particle attracted towards the axis of x by a force varying directly as the distance. For, let the intensity of force at distance r be $\frac{r}{a^2}$; then the components parallel to the axes of y and z will be $\frac{y}{a^2}$ and $\frac{z}{a^2}$; so that the differential equations of motion will be

$$\frac{d^2x}{dt^2}=0, \quad \frac{d^2y}{dt^2}=-\frac{y}{a^2}, \quad \frac{d^2z}{dt^2}=-\frac{z}{a^2}.$$

From the first of these equations we have $x=kt+k'$, or, reckoning time from the instant when $x=0$, and making the constant x-component of velocity unity,

$$x=t.$$

Hence, putting x for t in the other two equations, we have

$$\frac{d^2y}{dx^2}=-\frac{y}{a^2}, \quad \frac{d^2z}{dx^2}=-\frac{z}{a^2},$$

which are identical with the equations to the ray.

In like manner, the path of a ray under the conditions treated in Section II. of the previous paper is the same as that of a particle attracted towards the plane of reference by a force varying directly as the distance from this plane.

The differential equations of the motion of the particle are not (like those of the ray) limited to paths of small inclination.

As regards velocity, that of the particle increases and that of the ray diminishes as the line or plane of reference is approached.

Thus far most of our reasoning respecting rays has been merely approximate, and applicable only to rays of small inclination to the surfaces-of-equal-index. We now proceed to some rigorous deductions from the law of ray-curvature.

XIII. Let the surfaces-of-equal-index be concentric spheres. Required the law of index-variation which will cause all circles described about the common centre to be paths of rays.

Putting r for distance from centre, the required condition is

$$\frac{1}{r}=\frac{1}{\rho}=-\frac{d\log\mu}{dr};$$

that is,

$$d\log r+d\log\mu=0,$$

or

$$\mu r=\text{C}, \text{ an arbitrary constant.}$$

As μ cannot be less than unity, the law can only hold as far as $r=C$. Within this distance, a ray, starting at any point, in a direction normal to the radius vector drawn to it from the centre will describe a circle about the centre. A ray starting at any other angle will describe an equiangular spiral having the centre for pole.

In any medium in which the surfaces-of-equal-index are concentric spheres, if we denote by θ the angle at which a ray cuts the surface-of-equal-index at the point considered, and by p the perpendicular from the centre on the tangent to the ray at this point, we have

$$\frac{1}{\rho}=\frac{1}{r}\frac{dp}{dr}, \quad \cos\theta=\frac{p}{r}.$$

Hence, by the law of ray-curvature, we have

$$\frac{1}{r}\frac{dp}{dr}=-\frac{d\log\mu}{dr}\cos\theta=-\frac{1}{\mu}\frac{d\mu}{dr}\frac{p}{r},$$

or

$$\frac{dp}{p}=-\frac{d\mu}{\mu},$$

or

$$\mu p=C,$$

a well-known result, usually obtained by regarding continuous variation of index as the limit of an indefinite number of small changes *per saltum.*

XIV. When the surfaces-of-equal-index are coaxial circular cylinders, the radius of any cylinder being called r, the value of $\frac{d\log\mu}{dr}$ is the product of $\frac{1}{\rho}$ by the secant of the angle at which a ray cuts the surface-of-equal-index at the point considered; and this product will be the same for all rays at the same value of r. If the ray be a helix described on one of the cylinders, the angle in question is zero, and the curvature $\frac{1}{\rho}$ of this helix will be equal to the value of $\frac{d\log\mu}{dr}$ for this cylinder.

Let the equations of the helix be

$$y=r\cos\frac{x}{a}, \quad z=r\sin\frac{x}{a}.$$

Then the value of $\frac{1}{\rho}$ is found to be $\frac{r}{a^2+r^2}$. Hence we have

$$\frac{d\log\mu}{dr}=-\frac{r}{a^2+r^2};$$

that is,

$$d \log \mu = -\tfrac{1}{2} d \log (a^2 + r^2),$$

or

$$\mu \sqrt{a^2 + r^2} = \text{C}.$$

In a medium in which this law prevails, every helix of step $2\pi a$, coaxial with the surfaces-of-equal-index, will be a ray-path; and, conversely, every helical ray in the medium will have the step $2\pi a$.

In the immediate vicinity of the axis, $\frac{r}{a^2+r^2}$ is sensibly equal to $\frac{r}{a^2}$, and the law of index now under consideration becomes equivalent to that discussed in Section XI.

The step will be zero, and the helices will become circles, if a be zero, in which case the law of index becomes $\mu r = \text{C}$, as in Section XIII.

In general, when the surfaces of equal index are circular cylinders having for their common axis a line of maximum index, helical ray-paths will exist at all distances from the axis, and to each distance there will correspond a different step. The step for any distance r will in fact be determined by putting

$$\frac{d \log \mu}{dr} = -\frac{r}{a^2 + r^2},$$

and multiplying the value of a thus found by 2π.

XV. We found in Section II. of the previous paper that, for rays of small inclination to converge to foci in a medium in which μ is a function of the distance y from a plane of reference, the law of index-variation must be

$$\frac{d \log \mu}{dy} = -\frac{y}{a^2},$$

and that the rays will be curves of sines. Let us now examine the consequences of supposing all rays (whatever their inclinations) to be curves of sines, the surfaces-of-equal-index being still supposed to be parallel planes.

The curvature $\frac{1}{\rho}$ is, by the general law of ray-curvature, equal to $-\frac{d \log \mu}{dy} \frac{dx}{ds}$, and is also equal to $-\frac{d^2 y}{dx^2}\left(\frac{dx}{ds}\right)^3$ by geometry. Hence we have

$$\frac{d \log \mu}{dy} = -\frac{1}{\rho} \frac{ds}{dx} = \frac{\frac{d^2 y}{dx^2}}{1 + \left(\frac{dy}{dx}\right)^2}. \quad . \quad . \quad . \quad . \quad (\text{L})$$

But the equation of any ray through the origin of coordinates is to be of the form

$$y=b\sin\frac{x}{a};$$

whence

$$\frac{dy}{dx}=\frac{b}{a}\cos\frac{x}{a},\quad \frac{d^2y}{dx^2}=-\frac{b}{a^2}\sin\frac{x}{a}.$$

Equation (L) therefore gives

$$\frac{d\log\mu}{dy}=\frac{-b\sin\frac{x}{a}}{a^2+b^2\cos^2\frac{x}{a}}=\frac{-y}{a^2+b^2-y^2}.$$

But $\frac{d\log\mu}{dy}$ is to be simply a function of y, and therefore is not to vary from one ray to another. Hence the expression a^2+b^2 in the denominator of the expression last found must be constant, and may be denoted by c^2. The equation may then be written

$$d\log\mu=\tfrac{1}{2}d\log(c^2-y^2),$$

or

$$\mu=\frac{\mu_0}{c}\sqrt{c^2-y^2},$$

μ_0 denoting the value of μ in the plane of reference. Hence it is clear that when μ varies as $\sqrt{c^2-y^2}$, all rays will be curves of sines; and if b denote the amplitude of one of these curves, its half-wave-length will be

$$\pi a=\pi\sqrt{c^2-b^2},$$

which diminishes as amplitude increases.

For small amplitudes we have

$$a=\sqrt{c^2-b^2}=c\sqrt{1-\frac{b^2}{c^2}}=c\left(1-\frac{b^2}{2c^2}\right)\text{ nearly.}$$

Hence the limiting value of half-wave-length, or, in other words, the geometrical focal length, is πc; and the aberration from this focal length is always negative, its value being $-\pi\frac{b^2}{2c}$.

When b (and therefore also y) is small compared with a, the value of $\frac{d\log\mu}{dy}$ above obtained becomes sensibly equal to $-\frac{y}{a^2}$, thus confirming our previous approximate results.

XVI. Next let us seek a law of index-variation (the surfaces-of-equal-index being still parallel planes) which will cause all rays to be arcs of circles.

Let a and b be the coordinates of the centre of one of these circles, the ray being supposed to pass through the origin, and the radius of the circle being called c. Then the equation of the ray will be

$$x^2-2ax+y^2-2by=0,$$

the value of $\frac{ds}{dx}$ will be $\frac{c}{y-b}$; and since ρ is c, we have by equation (L),

$$\frac{d\log\mu}{dy}=-\frac{1}{\rho}\frac{ds}{dx}=-\frac{1}{c}\frac{c}{y-b}=-\frac{1}{y-b};$$

that is,

$$d\log\mu=-d\log(y-b),$$

or

$$\mu(y-b)=\mathrm{C}.$$

$y-b$ clearly denotes distance from a fixed plane, parallel to the plane of reference, and passing through the centre of the circular ray considered. The result which we have obtained accordingly indicates that, if the value of μ on one side of the plane of reference vary inversely as distance from a fixed plane on the other side of and parallel to the plane of reference, all rays on the first-mentioned side will be arcs of circles, having their centres in the said fixed plane.

If the medium is symmetrical about the plane of reference, there will be two such planes of centres, and the complete course of a ray will be a wavy line consisting of equal circular arcs succeeding one another in reversed positions. The half-wave-length may have any value from zero to infinity, the expression for it being

$$2a=2\sqrt{c^2-b^2},$$

where c varies from one ray to another, while b is constant.

When the two planes of centres merge into one (or, in other words, when b is zero), the arcs become semicircles, and a curious question arises as to the course of a ray after cutting the plane of reference at right angles. If a ray once become normal to the planes-of-equal-index, what is to make it swerve to one side more than to the other? The difficulty vanishes, or at least is indefinitely postponed, when we remember that the velocity of the ray, being inversely as μ, diminishes to zero in approaching the plane of reference, and infinite time will be required to reach this plane.

The results which we have established in the present section might have been deduced at once from the undulatory theory without any application of analysis. For if μ is inversely as the distance from a fixed plane, velocity is directly as this distance, and the velocities at all points in a plane wave-front are therefore directly as their distances from the intersection of the wave-front with the fixed plane. Hence the wave-front will swing round the line of intersection like a door upon its hinges, and each point in the wave-front will describe a circular arc, which will be the path of a ray.

XVII. Instead of employing the differential equation (L), we might have employed its integral, which can be reduced to the form

$$\mu \cos \theta = a,$$

a being a quantity which is constant for any one ray, but which varies from one ray to another. This result has been previously established in Section IV., and is in fact merely a statement of the law of sines as applied to a medium in which the surfaces-of-equal-index are parallel planes.

Again, since the curvature of a ray depends only on the variation of $\log \mu$, we shall have precisely the same paths with the law $\mu = f(y)$ as with the law $\mu \propto f(y)$, since $\frac{d \log \mu}{dy}$ will have the same value in both cases. The only limitation to this remark depends upon the circumstance that μ cannot be less than unity. A small constant factor applied to $f(y)$ may therefore diminish the range through which the law $\mu \propto f(y)$ can hold.

To obtain a convenient formula for determining the path of rays when μ is given as a function of y, observe that

$$1 + \left(\frac{dy}{dx}\right)^2 = \sec^2 \theta = \frac{\mu^2}{(\mu \cos \theta)^2} = \frac{\{f(y)\}^2}{a^2}, \quad . \quad . \quad \text{(M)}$$

whence

$$\frac{dx}{dy} = \frac{a}{\sqrt{\{f(y)\}^2 - a^2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(N)}$$

which is the general differential equation to the paths of rays in the medium, a being a parameter which varies from ray to ray. The following are some of the cases in which the equation can be integrated.

(1) $\mu \propto y, \ \frac{dx}{dy} = \frac{a}{\sqrt{y^2 - a^2}}$, the differential equation of a catenary, the axis of x being the line which is commonly employed as axis of x in treating of the properties of the curve, and a being the distance of the vertex of the curve from this axis.

The cases $\mu \propto y \pm b$ and $\mu \propto b - y$ are reducible to this by substitution.

(2) $\mu \propto \sqrt{y}$, $\frac{dx}{dy} = \frac{a}{\sqrt{y-a^2}}$, the differential equation of a parabola, with the axis of x for directrix, and a^2 for distance of vertex from directrix.

The cases $\mu \propto \sqrt{y \pm b}$ and $\mu \propto \sqrt{b-y}$ are reducible to this.

(3) $\mu \propto \frac{1}{y}$, $\frac{dx}{dy} = \frac{a}{\sqrt{\frac{1}{y^2}-a^2}} = \frac{ay}{\sqrt{1-a^2y^2}}$, the differential equation to a circle of radius $\frac{1}{a}$ having its centre on the axis of x. This result agrees with Section XVI. The cases $\mu \propto \frac{1}{y \pm b}$ and $\mu \propto \frac{1}{b-y}$ are reducible to this.

(4) $\mu \propto \frac{1}{\sqrt{y}}$, $\frac{dx}{dy} = \frac{a}{\sqrt{\frac{1}{y}-a^2}} = \sqrt{\frac{y}{\frac{1}{a^2}-y}}$, the differential equation of a cycloid generated by a circle of diameter $\frac{1}{a^2}$ rolling along the axis of x. The cases $\mu \propto \frac{1}{\sqrt{y \pm b}}$ and $\mu \propto \frac{1}{\sqrt{b-y}}$ are reducible to this.

(5) $\mu \propto \sqrt{b^2-y^2}$, $\frac{dx}{dy} = \frac{a}{\sqrt{b^2-y^2-a^2}}$, $x+c = a \sin^{-1} \frac{y}{\sqrt{b^2-a^2}}$, a result which agrees with Section XV. The case $\mu \propto \sqrt{q \pm py - y^2}$ is reducible to this.

(6) $\mu \propto \sqrt{y^2 \pm b^2}$, $\frac{dx}{dy} = \frac{a}{\sqrt{y^2 \pm b^2 - a^2}}$, whence

$$2y = e^{\frac{x+c}{a}} + (a^2 \pm b^2) e^{-\frac{x+c}{a}},$$

or, determining c so as to make $\frac{dy}{dx}$ vanish with x,

$$2y = \sqrt{a^2 \pm b^2}\,(e^{\frac{x}{a}} + e^{-\frac{x}{a}}).$$

The ordinates of the curve have therefore the constant ratio $\frac{\sqrt{a^2 \pm b^2}}{a}$ to those of a catenary. The case $\mu \propto \sqrt{y^2 \pm py \pm q}$ is reducible to this.

(7) $\mu \propto e^{by}$, or $\frac{d \log \mu}{dy} = b$; so that the curvature for a given

inclination of ray is the same in all parts of the medium. We have

$$\frac{dx}{dy}=\frac{a}{\sqrt{(e^{2by}-a^2)}};$$

or, putting $e^{by}=z$,

$$\frac{dx}{dz}=\frac{a}{b}\frac{1}{z\sqrt{z^2-a^2}},\quad x+c=\frac{1}{b}\sec^{-1}\frac{z}{a}.$$

Making $c=0$, this gives $e^{by}=a\sec bx$; and y will be infinite when $bx=\frac{\pi}{2}$, so that the line $x=\frac{\pi}{2b}$ is an asymptote.

Some, at least, of the foregoing seven cases are to be found in existing text-books and examination-papers; but I believe that the method here set forth is an improvement upon that generally adopted.

XVIII. The following additional proof of the general law of ray-curvature is interesting, as being directly deduced from the law of least time.

Let T denote the time of passage of a ray from a fixed point to the point whose coordinates are x, y, z. Let α, β, γ be the angles which the forward direction of the ray at x, y, z makes with the axes, and let μ be the index and v the velocity at x, y, z, so that $\mu=\frac{V}{v}$, where V denotes the velocity *in vacuo*.

Draw a short line from x, y, z in any direction perpendicular to the ray at this point, and let its direction-cosines be l, m, n. By the principle of least time (or, more correctly, of stationary time), the value of T will be the same (to the first order of small quantities) from the fixed point to both ends of this short line; that is,

$$l\frac{dT}{dx}+m\frac{dT}{dy}+n\frac{dT}{dz}=0.$$

But also

$$l\cos\alpha+m\cos\beta+n\cos\gamma=0;$$

therefore

$$\frac{\frac{dT}{dx}}{\cos\alpha}=\frac{\frac{dT}{dy}}{\cos\beta}=\frac{\frac{dT}{dz}}{\cos\gamma}=k \text{ suppose.}$$

Again, if we draw a short line along the ray from x, y, z, the values of T from the fixed point to the two ends of this line will differ by the time of traversing this line. Hence we have

$$\frac{dT}{dx}\cos\alpha+\frac{dT}{dy}\cos\beta+\frac{dT}{dz}\cos\gamma=\frac{1}{v}.$$

Substituting for $\frac{dT}{dx}, \frac{dT}{dy}, \frac{dT}{dz}$ their values $k\cos\alpha, k\cos\beta, k\cos\gamma$, we have

$$k=\frac{1}{v}=\frac{\mu}{V}.$$

Therefore

$$\cos\alpha=\frac{V}{\mu}\frac{dT}{dx};\ \cos\beta=\frac{V}{\mu}\frac{dT}{dy};\ \cos\gamma=\frac{V}{\mu}\frac{dT}{dz}.$$

To apply these last formulæ to the investigation of the curvature of the ray at any point, make the axis of x coincide with the tangent (drawn forwards), and the axis of y with the principal radius of curvature (drawn from the point towards the centre of curvature). Then, in the neighbourhood of the origin, we have, to the first order of small quantities,

$$\frac{V}{\mu}\frac{dT}{dy}=\cos\beta=\sin\alpha=\alpha;$$

and the curvature is

$$\frac{1}{\rho}=\frac{d\alpha}{dx}=\frac{d}{dx}\left(\frac{V}{\mu}\frac{dT}{dy}\right)=-\frac{V}{\mu^2}\frac{d\mu}{dx}\frac{dT}{dy}+\frac{V}{\mu}\frac{d^2T}{dx\,dy}.$$

But

$$\frac{dT}{dy}=0, \text{ and } \frac{dT}{dx}=\frac{1}{v}=\frac{\mu}{V}.$$

Therefore

$$\frac{1}{\rho}=\frac{V}{\mu}\frac{d^2T}{dx\,dy}=\frac{V}{\mu}\frac{d}{dy}\left(\frac{\mu}{V}\right)=\frac{1}{\mu}\frac{d\mu}{dy};$$

and, from our choice of axes, $\frac{d\mu}{dy}$ denotes the rate at which μ increases in travelling towards the centre of curvature.

Two other proofs will be found in Parkinson's 'Optics,' arts. 122, 123; the result there obtained, namely

$$\frac{\mu}{\rho}=\frac{d\mu}{dx}\frac{dy}{ds}-\frac{d\mu}{dy}\frac{dx}{ds},$$

being clearly equivalent to that which we have employed, since $\frac{dy}{ds}$ and $-\frac{dx}{ds}$ are the direction-cosines of ρ.

I may also remark that the formula

$$\frac{d\mu}{dx}=\frac{d}{ds}\left(\mu\frac{dx}{ds}\right),$$

which is usually obtained by a difficult application of the Calculus of Variations, can be immediately derived from the principles employed in this section. For since

$$\frac{dx}{ds}=\frac{\mathrm{V}}{\mu}\frac{d\mathrm{T}}{dx}, \text{ and } \frac{d\mathrm{T}}{ds}=\frac{\mu}{\mathrm{V}},$$

we have

$$\frac{d\mu}{dx}=\frac{d}{dx}\left(\mathrm{V}\frac{d\mathrm{T}}{ds}\right)=\frac{d}{ds}\left(\mathrm{V}\frac{d\mathrm{T}}{dx}\right)=\frac{d}{ds}\left(\mu\frac{dx}{ds}\right).$$

For this proof I am indebted to Professor Clerk Maxwell.

XXXIII. *On a simple Device for projecting on a Screen the Deflections of the Needles of a Galvanometer.* By Alfred M. Mayer, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.**

THE instrumental problem of obtaining on a screen the deflections of a galvanometer-needle in magnified proportions has occupied the thoughts of several physicists. The subject is evidently one of considerable importance. In delicate researches it is often necessary that the body of the observer should be removed from the instrument, while at the same time he must be able to observe the minute deflections of its needles. In lectures before our college-classes many of the most interesting and fundamental phenomena of radiant heat, electricity, and magnetism are often either entirely omitted or imperfectly presented, in default of an instrument which can be constructed by any one at a small outlay of time and expense. The problem, therefore, has not been deemed below the serious attention of eminent investigators; and although there are some who consider such contrivances trivial, yet I imagine they would think otherwise if they had the habit of continued original investigation, or the proper ambition to address their students in the very language of Nature by bringing them face to face with those phenomena which form the sure foundations of our scientific reasoning.

The method invented by Poggendorff, of observing the deflections of the galvanometer by reflecting to a screen a beam of light from a small mirror attached to the needles, has been used for many years. Sir William Thomson and Professor Tyndall have extensively used this method; and it has the advantage of giving to the reflected beam an angular motion the double of that given to the mirror by the needles. More recently Dr. Tyndall has devised an instrument on the principle of the megascope. He throws a vertical beam from an electric lamp on to the dial and needle of the galvanometer, and by means of a lens and inclined mirror placed above them he obtains their images on the screen.

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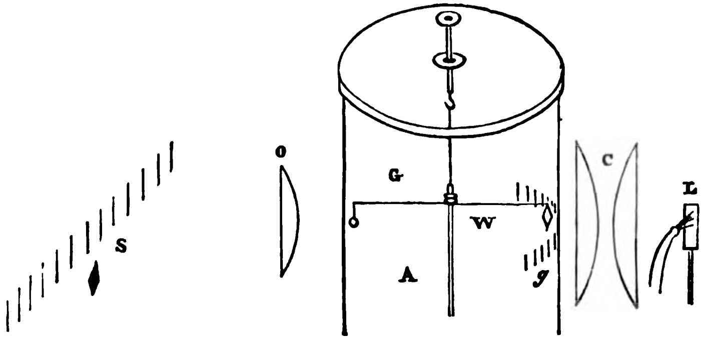
* Communicated by the Author.

In the Number of this Journal for July, 1872, I published the description of a new form of "Lantern-Galvanometer," which article was subsequently republished in Dr. Carl's *Repertorium*. This instrument, although it served admirably in the experiments for which it was specially devised, yet is of difficult construction and of limited application when compared with the very simple apparatus I will now proceed to describe.

G is the glass shade of the galvanometer, on which, at *g*, are drawn in india-ink the vertical graduation-lines of the instrument. A is a piece of aluminium wire, to whose lower end are fixed the needles of the galvanometer, and whose upper end is perforated with a small hole, so that the system can be suspended by a silk fibre. A fine wire of German silver (W) is attached transversely to the aluminium wire, and has its ends bent downwards at right angles to its length. This transverse wire can be placed at any azimuth by rotating it around its centre, which is coiled two or three times round the vertical wire of aluminium. On one of the bent ends of the transverse wire is cemented a diamond-shaped piece of light paper or foil; and the other end carries a small ball of wax whose weight equals that of the piece of paper or foil. The diamond courses around the shade at about 1 millimetre from its interior surface, with its lower point just above the lines of graduation. At C are represented the condensing-lenses of an oxyhydrogen lantern whose jet and lime are at L. O is the objective, which gives on the screen the magnified image of pointer and scale, as seen at S. This scale is not graduated into equal angular divisions, but its units represent units of deflecting-force traversing the galvanometer; and this scale is therefore derived from a careful calibration of the instrument.



The sharpness of the image on the screen is admirable; and with the calcium-light it is distinctly visible in a room considerably illuminated by daylight. With less illumination of the room I have used the instrument when the calcium-light was replaced by a kerosene-flame.

Evidently the precision of the indications of the apparatus just described are vitiated by the parallax of the index; for it does not describe a cylinder which is an extension of the one on which are drawn the graduations. This error is avoided by cementing on the inside of the shade a curved piece of glass whose radius of curvature equals the arm carrying the index, and whose centre coincides with the axis of the aluminium wire. With this modification in the apparatus I have succeeded in reading with precision deflections to 6′ of arc.

By the following arrangement, deflections to 1′ in an arc extending 5° on each side of the 0-point can be determined. A thin slip of microscope-cover glass is coated with a layer of black varnish, and through this varnish are cut, in a dividing-engine, fine equidistant lines. The diamond-shaped pointer is replaced by a light piece of cover-glass, also coated with varnish, and having cut on it one fine vertical line. These lines are illuminated by the lantern; and in front of them is placed an inch or an inch-and-a-half objective. On the screen we have the graduations as a series of bright lines on a dark ground, and along them moves the bright index-line of the pointer.

The zero-points of the scales can be brought accurately to coincide with the normal position of the index by revolving the shade on its base; and by turning the transverse wire so that it points towards the screen when the needles of the galvanometer have come to rest, we can readily project the image of the index and scale in any desired direction.

Although there are some advantages in having the scales attached to the galvanometer and in obtaining on the screen their magnified images, yet we can save much time in the construction of the apparatus by substituting for them scales drawn directly on the screen in very black india-ink.

My experience with this instrument has led me to prefer the use of only one magnetic needle, the one enclosed in the coil of the galvanometer; and this needle I render more or less astatic by means of a damping-magnet placed above the galvanometer and sliding on a vertical rod and rotating on its centre around the same. By means of the magnet, one can with expedition adapt the sensitiveness of the instrument to the requirements of special experiments; and thus the galvanometer is admirably suited for all experiments on radiant heat, electricity, or magneto-electricity. In fact, on holding my hand at a distance of 15 centimetres from the face of a thermopile without its conical reflector, I have often deflected the needle 30°.

One might suppose that the heat from the source of light would cause currents of air in the shade and make the needle fluctuate; but I have not met with this difficulty; and should it arise, it can be removed by placing in front of the condensing-lenses a glass tank containing an aqueous solution of alum.

The evident superiority of this instrument in simplicity of construction over all preceding devices will at once commend it; indeed any one in an hour or two can convert his galvanometer into a more convenient and precise instrument for research, and for the illustration of phenomena which have heretofore been presented to students only after considerable trouble on the part of the Professor. Now, however, the whole of the beautiful phenomena of radiant heat, which require a delicate instrument for their evolution, can be exhibited with ease and pleasure before our college-classes.

December 31, 1872.

XXXIV. *On Differential Galvanometers.*

By Louis Schwendler, *Esq.*[*]

Continued from vol. xliv. p. 170.

THE first part of this investigation concluded with the following question:—

What general condition must be fulfilled in the construction of any differential galvanometer in order to make a simultaneous maximum sensitiveness possible with respect to an alteration of external resistance in either of the differential branches?

To answer this question, it will be necessary to remember that the condition of a simultaneous maximum sensitiveness at or near balance was expressed by three equations, namely,

$$\frac{(w-g)(w'+g')+f(w+w'+g'-g)}{p(g-w)g'}=\frac{2(g+w+f)}{2\sqrt{g}\sqrt{g'}-p(g+w)}, \qquad \text{(II.)}$$

$$\frac{(w'-g')(w+g)+f(w+w'+g-g')}{\frac{(g'-w')}{p}\cdot g}=\frac{2(g'+w'+f)}{2\sqrt{g}\sqrt{g'}-\frac{g'+w'}{p}}, \qquad \text{(II'.)}$$

and

$$g'+w'-p\frac{\sqrt{g'}}{\sqrt{g}}(g+w)=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad \text{(I.)}$$

g and g' being the resistances of the two differential coils, w and w' the two resistances at which balance actually arrives, f

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[*] Communicated by the Author, from the Journal of the Asiatic Society of Bengal, vol. xlii. part 2, 1873; having been read before the Society March 6, 1872.

the total resistance in the battery branch, and p an absolute number expressing what was termed the "*mechanical arrangement*" of the differential galvanometer under consideration.

By these three equations, which are independent of each other, g, g', and p can be expressed in terms of w, w', and f.

By equation (I.) we have, at or very near balance,

$$p=\frac{g'+w'}{g+w}\cdot\frac{\sqrt{g}}{\sqrt{g'}},$$

which value, substituted in equations (II.) and (II'.), gives

$$\frac{(w-g)(w'+g')+f(w+w'+g'-g)}{(g'+w')(g-w)g'}=\frac{2(g+w+f)}{(g'-w')(g+w)}\quad . \quad \text{(II.)}$$

and

$$\frac{(w'-g')(w+g)+f(w+w'+g-g')}{(g+w)(g'-w')g}=\frac{2(g'+w'+f)}{(g-w)(g'+w')};\quad . \quad \text{(II'.)}$$

and from these two equations g and g' may be developed.

This is best done by subtracting equation (II.) from equation (II.'), when, after reduction, we get

$$(w'g-wg')(w'g+wg'+gg'+ww')$$
$$=-f(g+g'+w+w')(w'g-wg'). \quad . \quad . \quad \text{(III.)}$$

Now it must be remembered that, with respect to our physical problem, f, w, w', g, and g' represent nothing else but electrical resistances, and that they have therefore to be taken in any formula as quantities of the same sign (say positive).

Consequently the above equation (III.) would contain a mathematical impossibility (a positive quantity equal to a negative quantity) whenever the common factor $w'g-wg'$ is different from zero.

In other words, equation (III.) can only be fulfilled if we always have

$$w'g-wg'=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(IV.)}$$

This simple relation between the resistances at which balance arrives and the resistances of the two differential coils expresses not only the necessary and sufficient condition under which a simultaneous maximum sensitiveness can exist, but it also affords an easy means of getting at once those special values of g, g', and p, which only solve the physical problem.

Substituting the value of either g or g', as given by equation (IV.), in equations (II.) and (II'.) and developing g and g', we have

$$*g=-\frac{1}{3}\left(w+f\frac{(w+w')}{2w'}\right)+\frac{2}{3}\sqrt{w^2+\frac{w}{w'}(w+w')f+\frac{(w+w')^2}{16w'^2}f^2},(a)$$

$$*g'=-\frac{1}{3}\left(w'+f\frac{(w+w')}{2w}\right)+\frac{2}{3}\sqrt{w'^2+\frac{w'}{w}(w+w')f+\frac{(w+w')^2}{16w^2}f^2},(b)$$

the negative signs of the square roots having been omitted, since they would obviously make g and g' negative, values which cannot solve the physical question.

Further, if we introduce the ratio $\frac{g'}{g}=\frac{w'}{w}$, given by equation (IV.), into equation (I.), and develop p, we get

$$p^2=\frac{w'}{w}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

This latter expression shows the very simple relation which must exist between the *mechanical arrangement* of *any* differential galvanometer and the two resistances at which balance is arrived at in order to make a simultaneous maximum sensitiveness possible.

Thus if the ratio of the two resistances at which balance arrives is fixed, the mechanical arrangement p cannot be chosen arbitrarily, but must be identical with this ratio. This is in fact the answer to the question put at the beginning of this paper.

However, the meaning of this result will be made even still clearer if we revert to equation (I.), by which we have

$$p\frac{\sqrt{g'}}{\sqrt{g}}=\frac{g'+w'}{g+w}=\mathrm{C}, \quad . \quad . \quad . \quad . \quad (\mathrm{I.})$$

expressing the ratio between the total resistances in the two differential branches when balance is established, which ratio is generally known under the name *Constant of the Differential Galvanometer.*

Substituting in the above expression (I.) the value of $\frac{g'}{g}=\frac{w'}{w}$, from equation (IV.) we get at once

$$\frac{w'}{w}=\mathrm{C}; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

and as a second answer to the question put at the beginning of this paper we have therefore:—

A simultaneous maximum sensitiveness with respect to an alteration of external resistance in either branch of any differential galvanometer can be obtained only if the constant of the differential galvanometer is equal to the ratio of the two resistances at which balance arrives; and this clearly necessitates that the resistances of the respective coils to which w and w' belong should stand in the same ratio.

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* See note at end.

The general problem may now be regarded as solved by the following four general expressions:—

$$g=-\frac{1}{3}\left(w+f\frac{(w+w')}{2w'}\right)+\frac{2}{3}\sqrt{w^2+\frac{w}{w'}(w+w')f+\frac{(w+w')^2}{16w'^2}f^2}, \qquad (a)$$

$$g'=\frac{w'}{w}g, \qquad (b)$$

$$p^2=\frac{w'}{w}, \qquad (c)$$

$$C=\frac{w'}{w}. \qquad (d)$$

Additional Remarks.

In the foregoing it has not been shown that the values g and g' expressed by equations (a) and (b) must necessarily correspond to a maximum sensitiveness of the differential galvanometer, because it was clear *à priori* that the function by which the deflection is expressed is of such a nature that no minimum with respect to g and g' is possible. However, to complete the solution mathematically, the following is a very short proof that the values of g and g' really do correspond to a maximum sensitiveness of the differential galvanometer under consideration.

Reverting to one of the expressions for the deflection a° which any differential galvanometer gives before balance is arrived at, we had $a^\circ \propto \mathrm{K}\frac{\sqrt{g}}{\mathrm{N}}\Delta$; and as the increase of deflection at or near balance is identical with the deflection itself, and, further, as the law which binds the resistance of the differential coils to the other resistances in the circuit in order to have a maximum sensitiveness is of practical interest only when the needle is at, or very nearly at, balance, we can solve the question at once by making a° a maximum with respect to g and g', if we only suppose Δ constant and small enough; and as K is known to be independent of g and g', the deflection a° will be a maximum if $\frac{\sqrt{g}}{\mathrm{N}}$ is a maximum for any constant Δ (zero included).

Further, we know that $g'=\mathrm{C}g$, which value for g' in N substituted will make the latter a function of g only, and consequently $\frac{\sqrt{g}}{\mathrm{N}}$ also. We have therefore to deal with a single maximum or minimum; and, according to well-known rules, we have

$$\frac{da}{dg}=\frac{\mathrm{N}-2g\frac{d\mathrm{N}}{dg}}{2\sqrt{g\mathrm{N}^3}}=\frac{\mathrm{U}}{\mathrm{V}}$$

and

$$\frac{d^2a}{dg^2}=\frac{\mathrm{V}\frac{d\mathrm{U}}{dg}-\mathrm{U}\frac{d\mathrm{V}}{dg}}{\mathrm{V}^2};$$

but

$$\frac{da}{dg}=0;$$

it follows that U=0;

$$\therefore \frac{d^2a}{dg^2}=\frac{1}{\mathrm{V}}\frac{d\mathrm{U}}{dg}.$$

Now

$$\frac{d\mathrm{U}}{dg}=-\left(\frac{d\mathrm{N}}{dg}+2g\frac{d^2\mathrm{N}}{dg^2}\right);$$

but $\frac{d\mathrm{N}}{dg}$, as well as $\frac{d^2\mathrm{N}}{dg^2}$, being invariably positive, it follows that $\frac{d\mathrm{U}}{dg}$ is invariably negative; and as, further, V is always positive, it follows finally that $\frac{d^2a}{dg^2}$ is always negative, or the value of g obtained by equation $\frac{da}{dg}=0$ corresponds to a maximum sensitiveness of the differential galvanometer.

In a similar way it can be shown that the value of g' obtained by equation $\frac{da}{dg'}=0$ corresponds also to a maximum sensitiveness of the differential galvanometer.

This is in fact a second and far more simple solution of the problem. However, it is by no means as general, nor does it adhere as closely to the spirit of analysis, as the first more complicated solution.

Effect of Shunts.—It is clear that the introduction of shunts cannot alter the general results as given in equations (*a*), (*b*), (*c*), and (*d*), as long as the shunts are used merely for the purpose of carrying off a fixed quantity of current without in themselves having any direct magnetic action on the needle.

However, to avoid misunderstanding, it is well to remember that, in the case of shunts being used, the values to be given to w and w' in the above equations are *not* those at which balance actually arrives, but those at which balance would arrive if no shunts were used; *i. e.* the resistance at which balance is established when using shunts must be multiplied by the multiplying-power of their respective shunts before they are to be substituted in the equations (*a*), (*b*), (*c*), and (*d*).

Mechanical Arrangement designed by p.—The condition which must be fulfilled in the construction of any differential galvanometer to make a simultaneous maximum sensitiveness possible was expressed by

$$p^2=\frac{w'}{w}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

while $p=\frac{m'n'}{mn}$; and it will be now instructive to inquire what special physical meaning equation (*c*) has.

By m was understood the magnetic effect of an average convolution (*i. e.* one of average size and mean distance from the magnet acted upon when the latter is parallel to the plane of the convolutions) in the differential coil of resistance g when a current of unit strength passes through it. Similarly m' was the magnetic effect of an average convolution in the other differential coil of resistance g'.

Further, n and n' were quantities expressed by

$$\mathrm{U}=n\sqrt{g},$$

and

$$\mathrm{U}'=n'\sqrt{g'},$$

U and U′ being the number of convolutions in the two coils g and g' respectively.

Now we will call A half the cross section of the coil g (cut through the coil normal to the direction of the convolutions), which section, as the wire is to be supposed uniformly coiled, must be uniform throughout.

Thus we have generally

$$\frac{\mathrm{A}}{c(q+\delta)}=\mathrm{U}$$

wherever the normal cut through the coil is taken.

c is a constant indicating the manner of coiling, either by dividing the cross section A into squares, hexagons, or in any other way, but always supposing that, however the coiling of the wire may have been done, it has been done uniformly throughout the coil. (This supposition is quite sufficiently nearly fulfilled in practice, because the coiling should always be executed with the greatest possible care; and, further, the wire can be supposed practically of equal thickness throughout the coil.)

q is the metallic section of the wire, and δ the non-metallic section due to the necessary insulating covering of the wire.

Further, we have $g = \mathrm{U}\frac{b}{q\lambda}$, where b is the length of an average convolution, and λ the absolute conductivity of the wire material, supposed to be a constant for the coil.

Now, for brevity's sake, we will suppose that δ, the cross section of the insulating covering, can be neglected against q the metallic cross section of the wire.

Consequently we have

$$\frac{\mathrm{A}}{cq} = \mathrm{U} \text{ (approximately)}$$

and

$$g = \mathrm{U}\frac{b}{q\lambda};$$

$$\therefore \mathrm{U} = \sqrt{\frac{\mathrm{A}\lambda}{bc}} \, . \sqrt{g},$$

or

$$n = \sqrt{\frac{\mathrm{A}\lambda}{bc}};$$

similarly,

$$n' = \sqrt{\frac{\mathrm{A}'\lambda'}{b'c'}};$$

$$\therefore \frac{n'}{n} = \sqrt{\frac{\mathrm{A}'\lambda' bc}{\mathrm{A}\lambda b'c'}}.$$

But using wire of the same conductivity in both the differential coils, which should be as high as is possible to procure it, and further supposing the manner of coiling to be identical in both coils, we have

$$\lambda = \lambda',$$

$$c = c';$$

$$\therefore \frac{n'}{n} = \sqrt{\frac{\mathrm{A}'}{\mathrm{A}} \cdot \frac{b}{b'}}.$$

Further, we know that if the shape and dimensions of each coil are given, and in addition also their distance from the magnet acted upon, it will always be possible to calculate m and m', though it may often present mathematical difficulties, especially if the forms of the two coils differ from each other and are also not circular. This latter condition is generally necessitated in order to obtain the greatest absolute magnetic action of each coil in as small a space as possible.

However, it is clear that we may assume generally that the two coils have each an average convolution of identical shape and of the same length, placed at an equal distance from the magnet acted upon, and that therefore the magnetic action of each coil is dependent on the number of convolutions only.

In this case we have evidently

$$m=m',$$
$$b=b',$$
$$\frac{n'}{n}=\sqrt{\frac{A'}{A}};$$

and as

$$p=\frac{n'}{n}\cdot\frac{m'}{m},$$

we have, finally,

$$\frac{A'}{A}=\frac{w'}{w}. \quad \ldots\ldots\ldots \quad (e)$$

Equation (*e*) shows at once that under the supposed conditions (*i. e.* when the average convolutions in each coil are of equal size and shape) the wire used in either coil is of the same absolute conductivity, and that the thickness of the insulating material can be neglected against the diameter of the wire:—

The wire used for filling each coil must be invariably of the same diameter; otherwise a maximum sensitiveness is impossible.

How the above simple law expressed by equation (*e*) would be altered when the given suppositions were not fulfilled must be found by further calculation; but as the latter is intricate and a more general result is not required in practice, I shall dispense at present with this labour.

Special Differential Galvanometers.—Here shall be given the special expressions to which the general equations (*a*), (*b*), (*c*), and (*d*) are reduced when certain conditions are presupposed.

1*st case.*—When w and w', the two resistances at which balance is arrived at, are so large that f, the resistance of the testing battery, can be neglected against either of them without perceptible error. Substituting therefore $f=0$ in equations (*a*) and (*b*), we get

$$g=\frac{w}{3}, \quad \ldots\ldots\ldots \quad (a)$$

$$g=\frac{w'}{8}; \quad \ldots\ldots\ldots \quad (b)$$

and the other two remain as they are, namely

$$p^2=\frac{w'}{w}, \quad \ldots\ldots\ldots \quad (c)$$

$$C=\frac{w'}{w}. \quad \ldots\ldots\ldots \quad (d)$$

2nd case.—When the battery resistance f cannot be neglected against either w or w', but when the two resistances at which balance is arrived at are invariably equal.

Thus, substituting in the general equation

$$w=w'=w,$$

we get

$$g=g'=g=-\frac{w+f}{3}+\frac{1}{3}\sqrt{4w^2+8fw+f^2}, \quad . \quad . \quad (a, b)$$

$$p^2=1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C=1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

3rd case.—When the conditions given under 1 and 2 are both fulfilled, or

$$w=w'=w,$$

and

$$f=0;$$

then we have

$$g=g'=g=\frac{w}{3}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a, b)$$

$$p^2=1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

$$C=1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

the very same result which was obtained by direct reasoning at the beginning of this paper.

Applications.—Though the problem in its generality has now been entirely solved, it will not perhaps be considered irrelevant to add here some applications.

For our purpose differential galvanometers may be conveniently divided into two classes, viz. those in which the resistances to be measured vary within narrow limits, and those where these limits are extremely wide.

To the first class belong the differential galvanometers which are used for indicating temperature by the variation of the resistance of a metallic wire exposed to the temperature to be measured—as, for instance, C. W. Siemens's resistance thermometer for measuring comparatively low temperatures, or his electric pyrometer for measuring the high temperature in furnaces.

It is clear that for such instruments the law of maximum sensitiveness should best be fulfilled for the average resistance to be measured, which average resistance under given circumstances is always known.

To the second class belong those differential galvanometers which are used for testing telegraph-lines, at present the most important application of these instruments. In this case each differential coil should consist of separate coils connected with a commutator in such a manner that it is convenient to alter the resistance of each coil according to circumstances, *i. e.* connecting all the separate coils in each differential coil parallel when the resistances to be measured are comparatively low, and all the separate coils consecutively if the resistances to be measured are high &c., fulfilling in each case the law of maximum sensitiveness for certain resistances, which are to be determined under different circumstances differently, but always bearing in mind that it is more desirable to fulfil the law of maximum sensitiveness for high resistances (when the testing current in itself is obviously weak) than for low resistances.

An example will show this clearer. Say, for instance, a differential galvanometer has to be constructed for measuring resistances between 1 and 10,000. A Siemens's comparison box of the usual kind $\left(\frac{1}{10,000}\right)$ being at disposal, it will be convenient and practical to decide that the two differential coils should be of equal magnetic momentum; from which it follows that C as well as p must be unity, or, in other words, that the two coils must be of equal size, shape, and distance from the needle, and must also have equal resistances, *i. e.* must be filled with copper wire of the same diameter. The resistance of each coil is then found by

$$g=-\frac{w+f}{3}+\frac{1}{3}\sqrt{4w^2+8fw+f^2},$$

where f is the resistance of the battery, and w a certain value between 1 and 10,000, the two limits of measurement. The question now remains to determine w.

It is clear that the law of maximum sensitiveness has not to be fulfilled for either limit, because they represent only one of the 10,000 different resistances which have to be measured; but it is also clear that to fulfil the law for the average of the two given limits would be equally wrong, inasmuch as the maximum sensitiveness is far more required towards the highest than the lowest limit. We may assume, therefore, that it is desirable to fulfil the law for the average of the average and the highest limit, which gives

$$w=7500,$$

against which the resistance of the battery may always be neglected.

Consequently we have

$$g=\frac{w}{3}=2500$$

for each coil.

Now, if the coil be small and consequently the wire to be used for filling it is thin, the value $g=2500$ wants a correction to make allowance for the thickness of the insulating material, by which g becomes somewhat smaller*.

Before concluding, I may remark that the question of the best resistance of the coil, when the resistance to be measured varies between two fixed or variable limits, can be solved mathematically by the application of the variation calculus.

XXXV. *On Fractional Distillation.*

By J. C. Glashan, *Strathroy, Ontario*†.

In the current Number of the Philosophical Magazine Mr. J. A. Wanklyn states that up to the present there is no theory of fractional distillation, and thereupon proceeds to deduce one. The following I believe to be the mathematical representation of the law of the rate of separation of the liquids, and indirectly involving the theory of the process. The evolution of this theory is an inverse operation, giving a result involving "an arbitrary function;" but it may be noted in passing that Mr. Wanklyn's theory satisfies the equations—that, in fact, they are very easily deduced by it. If the experiments are correctly made and the observations of p and q, formula (XI.) as calculated directly from (VII.) and indirectly through (X.) affords a test of the truth of the mathematical theory; or, to test the truth of the theory, calculate r from examination of two portions of a continuously formed distillate.

Let $a+b$ represent a homogeneous mixture of a units of a

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* These expressions for g and g' must be corrected if the thickness of the insulating covering of the wire cannot be neglected against its diameter. The formula by which this correction can be made was given by me in the Philosophical Magazine, January 1867—namely,

$$\textit{corrected } g=cg(1-\sqrt[4]{gm^2}),$$

where $g=$ the resistance to be corrected and expressed in Siemens's units, and

$$m=\delta^4\sqrt{\frac{c\pi\lambda}{AB}}.$$

$\delta=$radial thickness of the insulating covering expressed in millimetres.

$c=$a coefficient expressing the arrangement adopted for filling the available space uniformly with wire; namely, if we suppose that the cross section of the coil, by filling it up with wire, is divided into squares, we have $c=4$; if in hexagons, $c=3\,.\,4$. &c.

$\lambda=$absolute conductivity of the wire-material (Hg$=1$ at freezing-point).

A=half the section of the coil in question when cut normal to the direction of the convolutions, and always expressed in square millimetres.

B=length of an average convolution in the coil, and expressed in metres.

† Communicated by the Author.

liquid A of volatility w, and b units of a liquid B of volatility v. Distil till there remains $a_{\prime}+b_{\prime}$ giving a distillate $(a-a_{\prime})+(b-b_{\prime})$; then $b=(a_{\prime}:a)^{v:w}$; or putting p for $a_{\prime}:a$, and r for $v:w$, we have the very simple formulæ:

1st still-liquor, $a+b$, ... (I.)

2nd „ „ $ap+bp^r$, ... (II.)

Distillate, $a(1-p)+b(1-p^r)$, . . . (III.)

(Distillate)^n, $a(1-p)^n+b(1-p^r)^n$. . . (IV.)

(Distillate)^n means that it is the distillate of (distillate)$^{n-1}$.

Solutions of (III.) and (IV.) of 1 degree of approximation are

$(a+br)(1-p)$, ... (V.)

$(a+br^n)(1-p)^n$. . . . . (VI.)

As stated above, these formulæ may be obtained by Mr. Wanklyn's theory. Thus, ratio of liquids in original liquor,

$$a:b;$$

in first infinitesimal distillate,

$$da:br\frac{da}{a};$$

in remaining liquor,

$$a\left(1-\frac{da}{a}\right):b\left(1-r\frac{da}{a}\right).$$

Continue distillation to the separation of n distillates. Ratio of remaining liquor,

$$a\left(1-\frac{da}{a}\right)^n:b\left(1-r\frac{da}{a}\right)^n.$$

This for a finite distillation, or n infinitely large, becomes

$$a\left(1-\frac{da}{a}\right)^n:b\left\{\left(1-\frac{da}{a}\right)^n\right\}^r=ap+bp^r;$$

for limit $\left(1-r\frac{da}{a}\right)^n=$ limit $\left(1-\frac{da}{a}\right)^{rn}$, and, by definition of p,

$$\left(1-\frac{da}{a}\right)^n=p.$$

Mr. Wanklyn uses (V.) and obtains $r=9\cdot6$ for ammonia (water $=1$); but from consideration of the composition of the remaining (or rather last distilling) liquor he concludes $r>13<14$. The correct value as found from (III.) is

$$\frac{2-\text{L}52}{2-\text{L}95}=12\cdot7488.$$

Formula (V.) shows distinctly that for p^r nearly $= 1$ the strength of the distillate is approximately proportional to that of the mother-liquor for the same fraction of A distilled (observed by Mr. Wanklyn). If $r > 1$ or B volatile compared with A, formula (IV.) shows that, with repeated distillations of the early-passing-over distillate, the B strength of the distillate will rapidly increase with the number of distillations, and formula (VI.) shows that, to a rude approximation, the rate of increase is in geometrical progression.

It has been assumed that the distillation is pure, *i. e.* that the vapour does not carry over any liquor held in mechanical suspension; for in this case the coadhesive function of the liquids will influence the result. Further, it is supposed that the whole distillate has been separated from the still-liquor and, if it is used, collected. This requires that no part of the distillate (especially at the beginning) condenses on the sides of the still, in time returning to the still-liquor. The following formulæ afford a test of the accuracy of the experiment (assuming the theory to be true), and also a means of correctly calculating r, even with a common still properly protected, although the second of the above-mentioned difficulties tends to vitiate early-made observations or those continuous from the beginning.

1st still-liquor, $a+b$, ... (VII.)

2nd „ „ $ap+bp^r$, ... (VIII.)

3rd „ „ $(ap)q+(bp^r)q^r$. . . . . (IX.)

1st distillate from (VII.),

$a(1-p)+b(1-p^r)$, ... (X.)

1st distillate from (VIII.),

$ap(1-q)+bp^r(1-q^r)$. . . . . . . (XI.)

Let r be calculated from (X.) and (VII.), (VIII.) is determined by observation for (X.); thus r may also be calculated from (XI.) and (VIII.). The latter value of r will be the more reliable if the elements of (X.) be accurately observed. The distillation is supposed to be continuous. The value of r may also be calculated very accurately from (II.), obtained from (I.) by evaporation under the air-pump.

For simplicity of formulæ I have assumed the mixture to be homogeneous and to remain so throughout the operation; but the instant evaporation commences this condition is violated. Thus in the deduction of the formulæ through Mr. Wanklyn's theory, the process is true only for the first infinitesimal distillation, and all subsequent distillations will involve a function of a quantity which represents the energy of restoration of the homogeneous condition. Again, since the mixture is not homogeneous in respect of its temperature, the restorative function will be affected by the velocity of convection. It is easy to show that formula (II.) is true for a non-homogeneous mixture if such mixture is a perfect liquid. This view shows that (II.) applies to liquids approaching the *perfect* condition, but does not apply to syrupy liquids, to thick oils, and such like. The differential formulæ for these show a tendency to thickening at the top, and also at the point of application of the heat. This tendency increases with the viscosity, and after a certain point may become cumulative, the energy of convection and heat-conducting power of the mixture as it approaches the solid state determining where the cumulative effect will first begin. Attention must also be paid to the direction of the *heat-convection*, i. e. the direction in which the warmer currents flow. Thus, if the convection of water at a temperature above that of its point of maximum density be reckoned positive, its convection below the temperature of the said point must be taken as negative. The deduction of the differential form of (II.) for a non-perfect liquid is not the object of this paper, which was commenced merely to give a simple means of correctly calculating r from Mr. Wanklyn's theory; but I may state that the correct formulæ show that a *crust* may be formed in two ways—(1) by negative convection, (2) by increase of viscosity accompanied by very slight variation of temperature in the parts of the evaporating liquid, or by very slow evaporation, or by a very great difference of temperature between the liquid and the "space" immediately beyond it.

February 22, 1873.

Note.—On examining the theory by which I was led to formula (II.), I find that if the energy of return to homogeneity remains sensibly uniform for all mixtures from $a+b$ to $ar+b$, the proposed formula holds, provided the mixture does not pass during the operation through a point of maximum or minimum density.

February 24, 1873.

XXXVI. *On the Action of Solid Bodies on Gaseous Supersaturated Solutions.* By Charles Tomlinson, *F.R.S.*[*]

IN the March Number of the Philosophical Magazine I gave a translation of Dr. Henrici's paper on the above subject from Poggendorff's *Annalen* for December last. Having also worked in the same field and published the results of my labours in this Magazine*, it may not be thought out of place if I here offer some opinions on Dr. Henrici's results, which stand out, as he admits, in singular contrast to my own.

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[*] Communicated by the Author.

In an inquiry of this kind it is, of course, necessary to define accurately what is meant by (1) a supersaturated gaseous solution, (2) a chemically clean surface in contradistinction to (3) one that is not clean. I do not object to Dr. Henrici's statement as to the first; but he seems to me to have but an imperfect idea as to the second. Having succeeded in making metallic surfaces sufficiently clean for voltaic contact by rubbing them with finely powdered pumice-stone spread on leather, he adopts this same process for making clean the glass rods, metallic wires, and some of the other bodies used in his inquiry; and he objects to one of the methods used by me for the purpose, namely heating them in the flame of a spirit-lamp, on the ground that the heat, instead of getting rid of the impure film that covers them more or less, simply converts that film into a porous body which still invests the surface.

The method of cleaning the wires &c. by means of pumice-powder spread on leather is absolutely of no value with reference to the present inquiry. The surfaces thus treated are still unclean, and naturally become covered with bubbles on immersing them in soda-water &c. So also in the application of heat, Dr. Henrici made use of a small alcohol-flame, and passed the bodies over and above it rather than through it; whereas he should have made his platinum wires red-hot and even white-hot.

In several of my published papers† I have endeavoured to show that the obscure and often contradictory behaviour of solids as nuclei, in separating gas, or vapour, or salt from their supersaturated solutions, becomes clear by considering whether the solids used as nuclei, or the walls of the flasks, test-glasses, and other containing vessels were or were not chemically clean as to surface at the moment of contact with the solution.

A body was defined as chemically clean the surface of which is entirely free from any substance foreign to its own composition. And it will be remarked that I speak of *surface* only. A glass rod is chemically clean, although a particle of carbon, or of oxide of iron, or other matter be enclosed and shut up within it; but not so if that particle reach and form a portion of the surface itself. So also a stick of tallow, stearine, paraffin, resin, sulphur, &c. is chemically clean so long as its surface falls under the definition just given.

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* Phil. Mag. for August and September 1867.

† Some of the definitions given in the text are from the Philosophical Transactions for 1871, p. 51.

Catharization is the act of cleaning the surface of bodies from all alien matter; and the substance is said to be catharised when its surface is so cleared.

The methods of doing this are various. The action of flame or of strong sulphuric or nitric acid, or of alkaline solutions is efficacious according to the nature of the surface. In some cases steeping the solids in water during several days may suffice; in others boiling them in acid or alkaline solutions, or rubbing them between corks or platinum-foil while immersed in the strongest commercial oil of vitriol, or washing them with alcohol or ether may be necessary, but always finishing with a copious rinsing in a stream of water. Should any one of these methods fail in any particular instance, another may be resorted to. For example, a short cylinder of phosphorus cut out of a stick that had been previously scraped was attached loopwise to a platinum wire, and so lowered into a test-tube (cleaned by the action of strong sulphuric acid and copious rinsing) containing soda-water*. Not a single bubble of gas appeared on the walls of the tube; but the phosphorus and the wire were abundantly covered. These were lowered into a tube containing spirits of wine, and after some minutes taken out and rinsed with clean water and again lowered into the soda-water; but they were still active, nearly as much so as before. The wire and the phosphorus were then placed in washed ether, and on being returned to the soda-water the wire was perfectly inactive, but the phosphorus was as briskly active as before,—the fact being that chemically clean platinum wire is not nuclear, but chemically clean phosphorus is.

As every thing exposed to the air of a room or to the touch takes more or less a deposit or film of foreign matter, substances may be conveniently classed as *catharized* or *uncatharized* according as they have been or not so freed from foreign matter.

And it is perhaps not taking too much license with language to extend the term *catharized* (denoting, as it does, the condition of pure surface) to those substances whose surface has not required the process. Thus a flint stone in the rough has an uncatharized surface, and when immersed in soda-water becomes instantly covered with bubbles; but split it, and the inner surface of the pieces will for a time be clean, and if put into soda-water will not have a single bubble of gas upon it.

On the other hand, some forms of matter, although perfectly clean, become covered with bubbles the moment they are immersed in a supersaturated gaseous solution. Thus, newly formed fragments of a lump of resin, or of a roll of sulphur, or of a block of stearine or paraffin, although chemically clean, become covered with bubbles as soon as they are placed in soda-water. But here other conditions are introduced, such as porosity, and the very different adhesion between resin &c. and air or gas, and resin &c. and water.

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* The soda-water used in these experiments was obtained from a convenient portable apparatus known as a "Seltzogene."

Dr. Henrici's idea of clean surfaces is such that, if previously cleaned according to his method with leather and pumice and exposed to the air, wiping them with a cloth or rinsing them is sufficient to restore their purity. A platinum wire or glass rod so cleaned and immersed in soda-water becomes covered with gas-bubbles; and this he urges as a proof of the chemical purity of such surfaces, and he even maintains that this is their proper function; whereas the very reverse is the truth.

Consider the problem in hand*. A supersaturated solution of a gas with its upper surface exposed to the air is always giving off gas either with effervescence or silently. It does so because the excess of gas has but a slight adhesion to the liquid, and the air is virtually a vacuum for it. Now the remaining surface of the liquid, or that confined by the sides of the vessel, may be regarded as being in exactly the same condition, subject, however, to two modifications—(1) the state of chemical purity of their surface, and (2) the pressure exerted by them virtually on the liquid. (1) Suppose the vessel to be of glass, and to be chemically clean according to the definition given above. No gas will be disengaged and no bubbles will form on the sides, because the adhesion between the sides and the gaseous solution is perfect; and therefore the sides may be regarded, *pro ratâ*, as merely a continuation of the liquid itself, and no bubbles will form there any more than in the central parts of the liquid. (2) But suppose the sides to be not chemically clean—to be dirty in fact; adhesion is diminished or destroyed, and therefore the surface of the liquid next to such sides is virtually as free as its upper surface: bubbles will consequently form here just as they do on the upper surface; but in the latter case they do not appear as bubbles (except during effervescence) because there is no pressure; the sides do exert pressure, and therefore bubbles are formed. It does not matter whether there be air or not between the sides and the liquid. It is no function of air to induce the liberation of gas or the formation of gas-bubbles. It is really want of adhesion. Now apply this to the case of a catharized glass rod, platinum wire, or a newly fractured surface of flint. Any one of these placed in the liquid does nothing more than form new sides, as it were, to the vessel; and its effect is merely that of the sides. If chemically clean, the glass rod &c. will form no bubbles round it; and hence it is inactive because its adhesion is perfect. If dirty, the surface of liquid in contact with it will be as free, or almost so, as the upper surface. We hope to give a more definite idea of the word "dirty" presently.

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* See Phil. Mag. Sept. 1867.

If further proof were wanting that Dr. Henrici has fallen into error in consequence of an inadequate conception of the term "chemically clean," it is to be found in his experiments and remarks on the action of mercury in liberating gas from solution. When what he terms "pure" mercury was put into soda-water, it "was immediately covered with rapidly swelling bubbles, which ascended, while others formed in their place. Or when by shaking the glass the bubbles escaped from the mercury, new ones immediately covered it; and this result, notwithstanding the small quantity of gas in solution, could be repeated many times with scarcely any diminution. Even in but slightly impregnated water, in which other surfaces did not act, mercury, by separating numerous bubbles, displayed its surpassing activity. Pure mercury forms indeed the most perfect surface that can be used in these experiments, since it is perfectly wetted by water."

Dr. Henrici does not state by what means he obtains pure mercury; but it is well known to all who have much to do with this metal that it becomes very readily tarnished or dirty, and that it is troublesome to clean. Indeed from the time of Prevost to that of Quincke it was always an anomaly in the phenomena of surface-tension, that water, in which $t=7{\cdot}3$, would not spread out into a film upon mercury, in which $t=47$. But the fact is that in trying this important experiment no one had devoted sufficient attention to the obtaining of chemically clean mercury until Quincke* fulfilled this necessary condition, and thus removed this anomaly from science. So also in testing the liberating action of mercury on the carbonic acid of soda-water, the results are sure to be erroneous unless special means be adopted for obtaining a pure and clean metal. I had some mercury by me that had been cleaned a few years ago; it was bright, convex, and tailless, and when last purified fragments of phosphorus would move over its surface with great freedom. Such was not the case now; the phosphorus was motionless on the surface. A portion of the metal was therefore put into a clean stoppered phial and shaken up with acid nitrate of mercury, and a day or two afterwards with the strongest oil of vitriol, and lastly well rinsed with clean water. The bottle was then filled up with soda-water, and the metal, as was to be expected, so far from displaying the singular activity referred to by Dr.

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* Poggendorff's *Annalen*, January 1870. A translation of this paper appeared in the Philosophical Magazine for April, May, and June 1871. Quincke's method of obtaining pure mercury is given at p. 460.

Henrici, was absolutely passive, even when set in motion so as to expose fresh surfaces to the solution.

When a surface is properly cleaned by one or other of the methods pointed out, the test of the cleansing process is so delicate, that on immersing the solid in soda-water the greater or less appearance of the bubbles or their entire absence marks exactly how much, or how little, or how completely the process has been successful. For example, the middle portion of a thick platinum wire was held in the flame of a spirit-lamp until it glowed. It was then put into soda-water, when there was an abundant deposit of bubbles above and below the part that had been heated, but not a bubble on that part. A glass tube was dipped into sulphuric acid to the depth of about 2 inches; a cork was then driven into the upper end, and the tube sunk to the depth of 4 inches in the acid. It was then taken out and rinsed, the cork being removed, and so placed in soda-water to the depth of 6 inches. 4 inches of the outside and 2 inches of the inside were free from bubbles; but the remainder of the tube, both inside and out, was thickly coated with them. A glass cylinder 7 inches in height, that had been made clean by the action of strong sulphuric acid, was wiped with what would be called a clean duster to the depth of about 2 inches at the open end, and then filled up with soda-water. The part that had been wiped was accurately delineated by being covered with bubbles, while the remaining 5 inches of surface were completely free from them. In a clean glass full of soda-water the finger was introduced below the surface with friction against the side. The finger-mark became immediately apparent in consequence of the formation of bubbles upon it. It was formerly supposed that rough surfaces were particularly favourable to the liberation of gas. Such surfaces have really no action, provided they are chemically clean and not porous, as was shown in my former experiments in the case of a rat's-tail file*. But it may happen that in attempting to clean an iron or steel wire, such as a knitting-needle, in strong nitric or dilute sulphuric acid, the surface becomes graphitic; and in such a case, on placing it in soda-water, it becomes immediately covered with bubbles.

Dr. Henrici found that quartz-sand gave off numerous bubbles of gas—a clear proof that it was unclean. He also found that charcoal made red-hot and plunged into soda-water was inactive, doubtless from the absorption of gas. I obtained the same result some years ago, and also that cocoa-nut-shell charcoal and boxwood-charcoal by long keeping under water that had been boiled, were equally inactive in soda-water, since the pores were already filled, or the gas contained in them had entered into solution. But if the charcoal thus treated be taken out of the soda-water and be placed in distilled water in a flask over the flame of a spirit-lamp, the gas reassumes its elastic form, and copious torrents are poured off from it during a long time. When this is exhausted, steam takes its place; and this action may be continued for any length of time, the effect being to prevent the bumping of the vessel, and to increase the amount of vapour given off*.

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* Phil. Mag. for August 1867. Experiments 3 and 4.

In some of Dr. Henrici's experiments it appears that while in the same gaseous solution platinum had scarcely any action, silver became coated with numerous bubbles. The reason simply was that the platinum happened to be clean and the silver unclean. So also on gently heating a solution of ammoniacal gas (*liquor ammoniæ*), no bubbles were formed either on platinum or silver. The reason was that the alkali made their surfaces clean. But if a platinum wire be drawn between the finger and the thumb previously touched with a fatty body, such as lard, the wire becomes abundantly covered with bubbles on introducing it into the solution of ammonia in a test-tube held over a spirit-lamp so as to warm it gently. The same result may be obtained with an aqueous solution of nitrous oxide under the receiver of an air-pump, a slight diminution of pressure being sufficient for the purpose.

Dr. Henrici obtained no result on placing an oil or a solid fatty body, such as stearine, on the surface of soda-water. Had he dipped a clean glass rod into oil, or rubbed it with stearine, he would have found it become abundantly covered with bubbles on immersing it in soda-water.

The fact is, that in an inquiry of this kind we cannot form clear ideas unless we distinguish and classify the bodies used according to the nature of their surfaces. And in this respect bodies may be arranged into three or four classes, in the first of which we place glass and all vitreous or vitrified surfaces, and the denser metals with a smooth non-porous surface, such as platinum, gold, silver, iron, steel, lead, tin, and mercury. To all these bodies, when chemically clean, the gaseous supersaturated solution adheres in the most perfect manner, so that there is no separation of gas at their surfaces.

In the second class we may place oils, both fixed and volatile, and fatty bodies, whether acid or neutral, various kinds of wax, resin (such as shellac and amber), camphor, phosphorus, and some other bodies, which, when chemically clean and placed in a gaseous supersaturated solution, display a very different kind of adhesion to one of its constituents as compared with the other;

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* "On the Action of Solid Nuclei in liberating Vapour from Boiling Liquids," Proc. Roy. Soc. 1869, p. 240.

for while the gas adheres strongly to such bodies, the water has a much weaker adhesion. Hence, while such bodies are not wetted by the water, they are so by the gas; and consequently in a supersaturated gaseous solution their surfaces become covered with bubbles, which increase in size, loosen their hold, and ascend, while other bubbles are formed in their place. This action goes on until the solution loses its state of supersaturation and becomes saturated only; but in this condition the bodies in this class remained covered with the bubbles last formed upon them, and that for days together. The solution, however, can be restored to its state of supersaturation by diminishing the pressure or by increasing the temperature, in which case the liberation of bubbles from the surfaces is renewed.

It may here be remarked that bodies in class I. are said to be "dirty" or "active," or "dynamic" or "nuclear," or "uncatharised" when they are contaminated, however slightly, with any one of the bodies in class II.

The third, and by far the most numerous class, contains bodies that are distinguished by porosity—such as various kinds of wood and the charcoals made from them, and a large variety of other bodies, including some of the metals. But as the consideration of this, as well as of the fourth class, which includes soluble substances, involves a number of details, we must defer them to another occasion.

Highgate, N.
March 15, 1873.

XXXVII. *On Galvanic Induction.* By A. F. Sundell[*].

ALTHOUGH many natural philosophers have turned their attention to the phenomena of galvanic induction and endeavoured to deduce them from a common principle, yet very few experiments have been made in order to ascertain if the theories agree in all respects with experience. The object of the experiments hitherto made has been, for the most part, to find in what manner the induction depends on the form and magnitude of the circuits and the strength of the primary current. Recently Professor Edlund in Stockholm has made these phenomena the subject of a theoretical research[†], and shown that they, like all known electrical phenomena, have their origin in the luminiferous æther. He has also deduced a formula for the inducing-force. It was for the purpose of verifying this formula that the experiments described in this paper were made, in the physical laboratory of the Royal Academy of Sciences, Stockholm.

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[*] Communicated by the Author.

[†] "Sur la Nature de l'Electricité," *Archives des Sciences de la Bibliothèque Universelle* (Genève), 1872, Mars et Avril; Philosophical Magazine, 1872, vol. xliv. pp. 81 and 174.

1. The induction-coils were arranged in the following manner. A copper wire, 0·5 millim. thick, insulated with silk, was wound in the rectangular incision on the circumference of a circular wooden plate. Four such coils were used. Two of the plates had a diameter of 44·4 centims.; the breadth of the incision was 0·9 centim., and its depth 0·55 centim.; thus the radius of the plate, from its centre to the bottom of the incision, was 21·65 centims. The spirals of these two coils were arranged in two sets. The other two plates had a radius of 6·95 centims. to the bottom of the incision, the breadth of which was 0·6 centim., and the depth 0·3. The incisions of these two plates were wholly filled up by the wire spirals. In the centres of the plates round holes were drilled; thus the plates could be placed on a prismatic wooden bar, on one side of which a paper scale of centimetres was extended. The primary coil was fixed on the end of the bar; the secondary coil was movable along it. In every experiment the plates were placed with their plane perpendicular to the length of the bar; thus its geometrical axis passed through the centre of the coil perpendicular to the planes of its spirals.

The primary current was produced by a galvanic battery of four to six Bunsen's cells. The intensity of this current was measured by a tangent-galvanometer. The disjunctor, or the apparatus for effecting the induction, consisted of two wheels of boxwood on a common axis with a handle. The peripheries of the wheels were divided, by means of sixteen brass plates, into intervals alternately conducting and non-conducting. Two brass springs of equal length pressed on each periphery. The one pair of springs made part of the primary circuit. Thus, by turning the handle, the primary current was alternately established and destroyed; at the moment of its breaking, the other pair of springs was in contact with a brass plate of the second wheel, thus completing the secondary circuit, in which a Weber's magnetometer was inserted. But when the first pair of springs just touched a brass plate, then the second pair pressed on a non-conducting interval, and the secondary circuit was incomplete. By this arrangement we got only secondary currents of the same direction, viz. those produced by breaking the primary current.

In the experiments the handle of the disjunctor was turned once round in half a second, sixteen secondary currents being thus produced. At every complete turn a brass spring pressed gently in an incision on the handle, whereby the experiment could be stopped after a certain number of turns. Usually five turns were made in an experiment; the amplitude of the *first* deflection that the eighty secondary currents produced in the magnetometer was taken as a relative measure of the strength of induction. The amplitude was observed with a telescope and a millimetre-scale by means of the mirror fixed to the magnetic needle. Whenever the handle was turned more or less than five times, the observed deflection was reduced to that for eighty currents. The conducting-power of the secondary circuit was measured by means of a magnet inductor. Before the commencement of an experiment the wheels of the disjunctor were always in such a position as to complete the primary circuit. The wires from the battery, the coils, and the magnetometer were twisted together two by two, or went parallel a short distance. Thus every other inducing effect, except that between the coils, was prevented.

2. We will relate the series of experiments in the order in which they were made. The following designations may be used:—i represents the intensity of the primary current; m the number of windings in the primary coil, and n that in the secondary coil; l the conducting-power of the secondary circuit, and J the first deflection of the needle of the magnetometer for eighty currents. The distance between the coils, denoted by z, is estimated from the plane of the middle winding in the one to the corresponding plane in the other coil.

Series 1. One of the large coils was taken as primary, and one of the small as secondary coil, $i=\tan 15°$, $m=31$, $n=51$. The current induced by the magnet inductor deflected the needle of the magnetometer eighty-seven divisions of the scale; accordingly $l=87$.

	1.	2.	3.	4.	5.	6.
z (centimetres) . .	15	20	25	30	40	40
J (divisions of the scale)	93·4	65·2	46·0	32·8	17·8	18·0
	93·6	65·0	45·6	32·0	17·4	18·4
	93·2	. . .	. . .	32·8		
J (mean)	93·4	65·1	45·8	32·5	17·6	18·2

	7.	8.	9.	10.	11.	12.
z . .	30	25	20	15	10	1·5
J . .	35·0	48·0	67·0	93·4	127·5	176·5
	35·2	47·6	67·2	93·0	127·3	175·5
					127·5	
J (mean)	35·1	47·8	67·1	93·2	127·4	176·0

If the arithmetical mean of the deflections corresponding to the same z is taken, we obtain the following result of this series:—

z . .	1·5	10	15	20	25	30	40
J . .	176·0	127·4	93·3	66·1	46·8	33·8	17·9

Series 2. Here the two greater coils were used; $i=\tan 14°$, $m=31$, $n=32$, and $l=59$.

	1.	2.	3.	4.	5.	6.	7.	8.
z . .	100	90	80	70	60	50	40	35
J . .	5·6	8·0	11·0	15·4	22·4	35·2	57·4	74·8
	5·6	8·2	11·2	15·2	22·6	35·4	57·6	76·0
	5·8	8·0	11·0	15·6	22·6	35·0	57·8	76·0
	5·8	8·0	11·0	15·0	22·8	...	...	75·6
J (mean)	5·7	8·1	11·1	15·3	22·6	35·2	57·6	75·6

	9.	10.	11.	12.	13.	14.	15.
z . . .	30	25	20	20	25	30	35
J . . .	100	134·0	185·0	183·0	134·2	101·0	74·6
	100	134·2	185·2	184·0	134·4	100·6	75·0
	100	134·0	185·2				
J (mean)	100	134·1	185·1	183·5	134·3	100·8	74·8

	16.	17.	18.	19.	20.	21.	22.
z	40	50	60	70	80	90	100
J	57·6	35·4	22·4	15·2	11·0	7·6	5·8
	57·2	34·6	22·4	15·0	10·8	7·8	5·6
J (mean)	57·4	35·0	22·4	15·1	10·9	7·7	5·7

The result is:—

z .	20	25	30	35	40	50	60	70	80	90	100
J .	184·3	134·2	100·4	75·2	57·5	35·1	22·5	15·2	11·0	7·9	5·7.

Series 3. Instead of the wooden bar, a cathetometer was used in this series. The secondary coil was fixed to its movable telescope-holder; the other coil was placed at the top of the vertical pillar. For the rest, the coils had the described relative position; viz. the planes of the wire spirals were parallel to each other and perpendicular to the straight line between their centres. The two smaller coils were used—$i=\tan 20°$, $m=53$, $n=46$, $l=86·1$.

	1.	2.	3.	4.	5.	6.
z . .	10	15	20	25	30	40
J . .	155·0	69	35·2	19·8	11·8	5·4
	154·2	69	35·0	20·0	12·0	5·6
	154·0	..	...	...	12·0	5·6
J (mean)	154·4	69	35·1	19·9	11·9	5·5

	7.	8.	9.	10.	11.	12.
z . .	40	30	25	20	15	10
J . .	5·5	12·0	19·8	34·4	69·0	154·2
	5·5	11·8	20·0	34·2	68·4	154·2
					68·4	155·4
J (mean)	5·5	11·9	19·9	34·3	68·6	154·6

In the mean the deflections in this series were:—

z . .	10	15	20	25	30	40
J . .	154·5	68·8	34·7	19·9	11·9	5·5

3. When a galvanic current of the intensity i begins in a circuit, any one element ds of it induces in an element ds_1 of another circuit an electromotive force, for the magnitude of which Professor Edlund has deduced this expression:—

$$+\frac{i}{r^2}\left(a\cos\theta+\tfrac{3}{4}kh\cos^2\theta\right)\cos\theta_1 ds\, ds_1, \quad . \quad . \quad (1)$$

where r signifies the distance between the elements ds and ds_1, θ the angle between the element ds and the straight line that joins the two elements, and θ_1 the angle between that line and the element ds_1; a and k are constants, and h the velocity of the æther* in the primary circuit. When the induction is to be calculated arising from the breaking of the primary current, the sign + of the expression (1) must be changed into −. The amount of induction for any actual case is found by integrating expression (1) for the whole length of the two circuits. In special cases the result of this integration is previously known. For example, if the circuits are plane curves in such a relative position that the plane of each circuit divides the other into halves situated symmetrically to the dividing plane, the whole integral is equal to zero. If only the plane of one circuit divides the other symmetrically, but not *vice versâ*, the integral of the first term involved in the expression (1) is zero. On the other hand, the second term is of no effect if both the circuits are divided symmetrically by the same plane vertical to their own planes. As the above experiments concern this last case, we shall now examine it a little more closely. We suppose that the circuits are circles, the centres of which are on a perpendicular to their planes. This perpendicular we take for z-axis, and the plane of the primary circuit for xy-plane. Thus the origin corresponds to the centre of this circuit. Every plane through the z-axis is perpendicular to the circuits and divides them symmetrically. Therefore the integral of the second term in the expression (1) disappears, and the whole induction is

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* According to Professor Edlund's theory of electrical phenomena, the galvanic current consists in a translative motion of the luminiferous æther in the direction of the positive current.

$$ai\iint\frac{\cos\theta\cos\theta_1}{r^2}\,ds\,ds_1. \quad \ldots \quad (2)$$

This expression represents the induction in the *first* moment of the phenomenon. In order to obtain the electromotive force for the whole duration of the induction, the function under the signs of integration must be multiplied by a function of r, viz. br, in which b is a constant, whence results

$$abi\iint\frac{\cos\theta\cos\theta_1}{r}\,ds\,ds_1. \quad \ldots \quad (3)$$

The axis of the y-coordinate may be taken in the negative direction through the inducing element ds; and the x-coordinate may be reckoned negative parallel to the direction of the current in ds. Integrating for ds_1, the direction along the secondary circuit is taken opposite to that of the primary current. If R is the radius of the primary circuit, R_1 that of the secondary circuit, and x, y, z are the coordinates of ds_1, we have

$$r=\sqrt{R^2+R_1^2+z^2+2Ry},$$

$$\cos\theta=-\frac{x}{r},$$

$$\cos\theta_1 ds_1=-\frac{R}{r}\,dy,$$

and

$$abi\iint\frac{\cos\theta\cos\theta_1}{r}\,ds\,ds_1=Rabi\iint\frac{x}{r^3}\,dy\,ds. \quad \ldots \quad (4)$$

Observing that for two elements ds_1 of equal length and in a symmetrical position to the yz-plane the function xdy has the same sign, and that each single element of the primary circuit has the same position relative to the secondary circuit, we can write expression (4) thus:—

$$4\pi R^2abi\int_{-R_1}^{+R_1}\frac{xdy}{r^3}=4\pi R^2abi\int_{-R_1}^{+R_1}\frac{\sqrt{R_1^2-y^2dy}}{(R^2+R_1^2+z^2+2Ry)^{\frac{3}{2}}}. \quad . \quad (5)$$

In the above experiments the windings of the coils may be regarded as circles with their centres on the z-axis and their planes parallel to the xy-plane. Expression (5) can, of course, be used for calculating the intensity of the induced electromotive force according to different values of R, R_1, and z. The spirals in a coil have not quite equal radii; nevertheless the results of the calculation will be accurate enough for comparison with the observations if a mean value for the radii be used. As the radius of the greater coils 21·7 centims. is adopted, being the sum of the inner radius of the coil (21·65 centims.) and the thickness (0·05 centim.) of the inner set of spirals. As regards the smaller coils, to their inner radius (6·95 centims.) must be added half the depth of the incision on the peripheries (0·15 centim.); accordingly their radius is estimated at 7·1 centims. As mean values of the distance between a spiral in the one coil and one in the other coil the distances above given under z are adopted. Let the deflection produced by the induced currents in an experiment be J, we have

$$J = 4\pi c R^2 ilmn \int_{-R_1}^{+R_1} \frac{\sqrt{R_1^2 - y^2}}{(R^2 + R_1^2 + z^2 + 2Ry)^{\frac{3}{2}}} dy, \quad . \quad (6)$$

where l, m, n have the same signification as above (§ 2), and c is a constant containing a and b. We substitute a new variable, $u = \frac{y}{R_1}$, and write

$$\frac{R^2 + R_1^2 + z^2}{2RR_1} = \alpha;$$

thus the equation (6) changes into

$$J = \pi c \sqrt{2RR_1}\, ilmn \int_{-1}^{+1} \frac{\sqrt{1-u^2}}{(\alpha+u)^{\frac{3}{2}}} du,$$

or

$$J = \pi c \sqrt{2RR_1}\, ilmn A, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

if we denote by A the integral depending on α. The value of α is in all actual cases of induction greater than unity. In order to compare the different series, we have calculated the deflections J_1 corresponding to $R = R_1 = z = 1$ or $\alpha = \frac{3}{2}$, $i = 1 = \tan 45°$, $l = 1000$, and $mn = 100$. We have

$$J_1 = 100000 \pi c \sqrt{2} . A_1, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where A_1 is the value of A corresponding to $\alpha = \frac{3}{2}$. Eliminating c, we get

$$J_1 = \frac{100000}{ilmn\sqrt{RR_1}} . \frac{A_1}{A} . J. \quad . \quad . \quad . \quad . \quad (9)$$

This equation gives the value of J_1 for every observed J. The theory is confirmed if these values agree within the limits of errors of observation. Then the deflections observed with different distances between the coils may not differ in any remarkable degree from the corresponding values of J calculated by the formula

$$J = \frac{ilmn\sqrt{RR_1}}{100000} . \frac{A}{A_1} . J_1, \quad . \quad . \quad . \quad (10)$$

where the arithmetical mean of the values of J_1 in a series is to be used.

The integral A is found by developing the function in a series. As $\frac{u}{\alpha} < 1$, we have

$$\frac{1}{(\alpha+u)^{\frac{3}{2}}} = \frac{1}{\alpha^{\frac{3}{2}}}\left(1-\frac{3}{2}\cdot\frac{u}{\alpha}+\frac{3\,.\,5}{2\,.\,4}\cdot\frac{u^2}{\alpha^2}-\frac{3\,.\,5\,.\,7}{2\,.\,4\,.\,6}\frac{u^3}{\alpha^3}+\ldots\right)$$

$$=\frac{1}{\alpha^{\frac{3}{2}}}\Sigma_{p=0}^{p=\infty}(-1)^p\mathrm{B}_p\,.\frac{u^p}{\alpha^p},$$

where

$$\mathrm{B}_p=\frac{3\,.\,5\,.\,7\ldots(2p+1)}{2\,.\,4\,.\,6\ldots 2p}.\quad\text{For } p=0,\ \mathrm{B}_0 \text{ is } =1.$$

Hence

$$\mathrm{A}=\frac{1}{\alpha^{\frac{3}{2}}}\Sigma_{p=0}^{p=\infty}(-1)^p\frac{\mathrm{B}_p}{\alpha^p}\int_{-1}^{+1}u^p\sqrt{1-u^2}\,du. \quad . \quad (11)$$

By the theory of binomial integrals we have

$$\int u^p\sqrt{1-u^2}du=-\frac{u^{p-1}(1-u^2)^{\frac{3}{2}}}{p+2}+\frac{p-1}{p+2}\int u^{p-2}\sqrt{1-u^2}du.$$

For the limits $u=+1$ and $u=-1$ the first term in the right-hand member is zero, and

$$\int_{-1}^{+1}u^p\sqrt{1-u^2}du=\frac{p-1}{p+2}\int_{-1}^{+1}u^{p-2}\sqrt{1-u^2}du.$$

In the same manner we find

$$\int_{-1}^{+1}u^{p-2}\sqrt{1-u^2}du=\frac{p-3}{p}\int_{-1}^{+1}u^{p-4}\sqrt{1-u^2}du,$$

$$\int_{-1}^{+1}u^{p-4}\sqrt{1-u^2}du=\frac{p-5}{p-2}\int_{-1}^{+1}u^{p-6}\sqrt{1-u^2}du,\ \&c.$$

If p is an odd number $2q+1$, the last integral of this kind is $\int_{-1}^{+1}u\sqrt{1-u^2}du$; and for p even $=2q$ the last integral is $\int_{-1}^{+1}\sqrt{1-u^2}du$. It is easily found that the former integral is $=0$; thus all coefficients B_p with an odd index disappear. The value of the latter integral is $=\frac{\pi}{2}$. If we ascend successively to the original integral, we find for $p=2q$:—

$$\int_{-1}^{+1}u^{2q}\sqrt{1-u^2}du=\frac{(2q-1)\,(2q-3)\,(2q-5)\ldots 3\,.\,1}{(2q+2)\quad 2q\quad (2q-2)\ldots 6\,.\,4}\cdot\frac{\pi}{2}=\mathrm{C}_{2q}\,.\frac{\pi}{2},$$

if we denote by C_{2q} the fractional coefficient, which is $=1$ for

$q=0$. The result of the calculation is

$$A=\frac{\pi}{2\alpha^{\frac{1}{2}}}\Sigma_{q=0}^{q=\infty}B_{2q}\,.\,C_{2q}\,.\,\frac{1}{\alpha^{2q}},$$

or

$$A=\frac{\pi}{2\alpha\sqrt{\alpha}}\left[1+\frac{1}{4}\cdot\frac{3\,.\,5}{2\,.\,4}\cdot\frac{1}{\alpha^2}+\frac{1\,.\,3}{4\,.\,6}\cdot\frac{3\,.\,5\,.\,7\,.\,9}{2\,.\,4\,.\,6\,.\,8}\cdot\frac{1}{\alpha^4}\right.$$
$$\left.+\frac{1\,.\,3\,.\,5}{4\,.\,6\,.\,8}\cdot\frac{3\,.\,5\,.\,7\,.\,9\,.\,11\,.\,13}{2\,.\,4\,.\,6\,.\,8\,.\,10\,.\,12}\cdot\frac{1}{\alpha^6}+\ldots\right].$$

In calculating the values of A by this formula, the terms have been determined to four decimals. The more the value of α exceeds unity, the more rapidly does this series converge. Generally 4 or 5 terms were sufficient. Only for values of α very near unity was it necessary to calculate 6 or 7 terms besides the first. A_1 is found $=0{\cdot}7079\,.\,\frac{\pi}{2}$; in this value are contained the first nine terms of the series. The results of the whole calculation were the following:—

Series 1. $R=21{\cdot}7$, $R_1=7{\cdot}1$, $mn=1581$, $l=87$, and $i=\tan 15°$.

s.	J_1.	J. Calculated.	J. Observed.	Difference.
1·5	49·74	176·7	176·0	+0·7
10	49·59	128·3	127·4	+0·9
15	49·91	93·4	93·3	+0·1
20	50·05	66·0	66·1	−0·1
25	50·16	46·6	46·8	−0·2
30	50·65	33·3	33·8	−0·5
40	49·53	18·1	17·9	+0·2
Mean	49·95			

Series 2. $R=R_1=21{\cdot}7$, $mn=992$, $l=59$, and $i=\tan 14°$.

s.	J_1.	J. Calculated.	J. Observed.	Difference.
20	51·86	186·4	184·3	+2·1
25	52·55	133·9	134·2	−0·3
30	53·43	98·5	100·4	−1·9
35	53·27	74·0	75·2	−1·2
40	53·24	56·7	57·5	−0·8
50	52·87	34·8	35·1	−0·3
60	52·19	22·6	22·5	+0·1
70	51·84	15·4	15·2	+0·2
80	53·11	10·9	11·0	−0·1
90	52·26	7·9	7·9	0
100	50·29	5·9	5·7	+0·2
Mean	52·45			

Series 3. $R=R_1=7\cdot1$, $mn=2438$, $l=86\cdot1$, and $i=\tan 20°$.

z.	J_1.	J.		Difference.
		Calculated.	Observed.	
10	49·63	153·7	154·5	−0·8
15	49·77	68·2	68·8	−0·6
20	49·17	34·8	34·7	+0·1
25	49·71	19·8	19·9	−0·1
30	48·33	12·2	11·9	+0·3
40	49·66	5·5	5·5	0
Mean	49·38			

The differences are not important; the theory is certainly confirmed in a very remarkable manner. Also the accordance of the mean value of J_1 is very satisfactory; for we have found

in the series	1.	2.	3.
J_1	49·95	52·45	49·38.

The mean value is 50·59, with the probable error ±0·636, a small quantity considering the simple means used for the experiments.

4. We will now consider the case in which the integral of the first term in expression (1) disappears. Let the induced circuit turn, about the diameter parallel to the y-axis, 90° from its position in the preceding experiments. Thus the two circuits come into a relative position in which no induction exists. The integral of the first term disappears, because the plane of the secondary circuit divides the primary circuit symmetrically; and as the xz-plane divides both the circuits symmetrically, the second term also is of no influence. But if we displace the secondary circuit in the yz-plane so that neither the y-axis nor the x-axis passes through its centre or intersects its circumference, the integral of the second term obtains a finite value. In order to show this, we draw through the inducing element ds, the coordinates of which may be $x, y, 0$, a plane (denoted by P) parallel to the x-axis and containing the centre of the secondary circuit. Thus the plane P divides this circuit symmetrically. We combine the element ds with an element ds_1 of the secondary circuit on one side of P. Let the coordinates of ds_1 be $0, \eta, \zeta$; then we find

$$\cos^2\theta=\frac{\eta^2x^2}{r^2R^2},$$

and the electromotive force in the first moment

$$=+\frac{3}{4}ikh\frac{\eta^2x^2}{r^4R^2}\cos\theta_1\,ds\,ds_1. \quad . \quad . \quad . \quad . \qquad (12)$$

To the element $0, \eta, \zeta$ corresponds another element $0, \eta_1, \zeta_1$, at the same distance from the plane of symmetry P, but on the opposite side of it. If we combine that element with ds, we obtain the same numerical value of r and $\cos\theta_1$, but the latter quantity changes its sign. The electromotive force of this combination is

$$-\frac{3}{4}ikh\frac{\eta_1^2x^2}{r^4R^2}\cos\theta_1\,ds\,ds_1. \quad . \quad . \quad . \quad . \qquad (13)$$

By adding (12) and (13) we obtain

$$+\frac{3}{4}ikh\frac{x^2}{r^4R^2}(\eta^2-\eta_1^2)\cos\theta_1 ds\,ds_1. \quad . \qquad (14)$$

If we multiply by br and effect the integration along one of the halves into which the secondary circuit is divided by the plane P, we obtain the inducing force of the element ds. By a second integration along the primary circuit, the whole amount of induction will be known. As none of the finite factors in expression (14) changes sign within the limits of integration, the integral has, mathematically speaking, a finite value. However, it is possible that the effect of the induction may be too feeble to be shown by the experiment, because the coefficient of the second term is very small in comparison with that of the first term*. Moreover an attempt to measure this effect directly by placing the coils so as to annul as much as possible the influence of the first term did not afford a decisive result, though a little modification of the experiment was more successful. It is evident that $\cos\theta$ and the first term in expression (1) change sign when the primary current is reversed; on the contrary, the sign of the second term, which contains $\cos\theta$ in the second degree, is independent of the direction of the current in the element ds. Accordingly, if the induction produced, for example, by breaking the primary circuit is observed for the opposite directions of the inducing current, the coils being in such a position that only the second term has an influence, the deflections of the magnetometer must be equal and towards the same side from the position of equilibrium. A perfectly accurate adjustment of the coils is not possible; therefore even the first term must be taken into account, as also other causes of magnetic deflection. Let a denote the deflection corresponding to the first term, b that for the second term, a_1 and b_1 the same for the induction between the primary coil and the coil of the multiplier and its wires, a_2 and b_2 the induction from the wires of the battery. The primary coil and the wires of the battery were not infinitely distant from the magnetometer; thus a direct influence on the magnetic needle was possible. Let c denote this influence of the coil, and c_1 that of the battery wires. The wires from the disjunctor to the commutator and the coils were twisted together; and therefore their influence may be regarded as inconsiderable. The tangent-galvanometer was only occasionally inserted in the primary circuit to obtain an approximate value of the intensity of the current; in the induction-experiments it was taken out of the circuit. The whole deflection is $a+a_1+a_2+b+b_1+b_2+c+c_1$ for the one direction of the primary current, and $-a-a_1+a_2+b+b_1+b_2-c+c_1$ for the opposite direction. By addition we obtain $2(a_2+b_2+b+b_1+c_1)$. The quantity of $a_2+b_2+c_1$ was measured by excluding the inducing coil from the primary circuit. In relation to the diminished resistance the intensity of the current was very great ($=\tan 52°$); yet the deflection (for 128 secondary currents) did not reach half a division of the scale. When the inducing coil was made part of the circuit, the strength of the current was only $=\tan 28°$; and for so feeble a current the sum $a_2+b_2+c_1$ may be regarded as $=0$. In the following experiments the coils had this position: the windings of the primary coil were parallel to the *xy*-plane; and the middle spiral of the secondary coil was in the *yz*-plane, in such a position that it was not intersected by any one of the axes. For observing the effect of the second term, this relative position of the coils is the most advantageous, as the elements of the integral in expression (14) are all of the same sign. The distance of the centre of the secondary coil from the *xz*-plane, was a little greater than its diameter. The coils were of the described construction; their inner radius was 16 centims., the thickness of the wire 1 millim., and the number of spirals forty-five. The conducting-power of the secondary circuit was $=119$. The deflections (for 128 currents) to rising numbers of the scale are preceded by the sign +, and those to the opposite side by −. Before every new experiment the inducing current was reversed.

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* Edlund, "Sur la Nature de l'Électricité."

	Experiment 1.	2.	3.	4.
Deflections .	+13·2	−12·0	+12·4	−12·6
	+11·2	−12·2	+12·2	−13·0
	+12·8	−12·6	+12·2	−11·4
	+12·2			
Means .	+12·4	−12·3	+12·3	−12·3

	Experiment 5.	6.	7.	8.
Deflections .	+12·4	−12·8	+12·4	−12·4
	+12·0	−13·4	+12·0	−12·2
	+12·2	−12·6	+12·0	−11·6
Means .	+12·2	−12·9	+12·1	−12·1

The mean values of these deflections are +12·25 and −12·40.

As in comparison with the great distance between the inducing coil and the magnetometer, b_1 can be neglected, for b we may assume $2b = -12{\cdot}40 + 12{\cdot}25 = -0{\cdot}15$, or $b = -0{\cdot}08$, a quantity certainly within the limits of errors of observation. Thus these experiments confirm that the second term in the formula of induction is very small compared with the first term.

5. When the æther in a circuit is put in motion, its repulsive force on the surrounding æther-molecules is changed; and these are moved to a new position of equilibrium. Also the æther in a circuit placed in the neighbourhood of the former is momentarily moved; that is to say, a secondary current is excited in it. When the inducing current ceases, the surrounding æther and that in the secondary circuit return to the original position of equilibrium; and thus a secondary current of opposite direction is produced. It is known that secondary currents of the same intensity as those which are excited by establishing or destroying the inducing current, the circuits being in any one relative position, are also produced when the circuits are brought into this position from another (in which no induction is performed by completing or breaking the primary circuit), or *vice versâ*, from the former into the latter position. The new theory explains this in a very simple manner. Suppose the inducing current is established; then the æther in the secondary circuit is displaced from its original position of equilibrium. We can restore the original equilibrium in two ways. The cause of perturbation is removed either by breaking the inducing current or by changing, in a suitable manner, the relative position of the circuits, for example, by making the distance between them very great. Further, by bringing the circuits from this latter position into the former, the æther in the secondary circuit is disturbed in the same manner as by establishing the inducing current in the original position. The velocity of the relative motion has no influence on the total intensity of the induced currents.

In connexion with the experiments related, the following were made in order to ascertain the agreement in results of the two methods of induction. The coils last used were placed horizontally the one upon the other, so that their centres were on the same vertical line. In this position the primary circuit was (once) completed or broken, and the intensity of the excited secondary currents was noted. Then, the primary circuit being closed, the secondary coil was vertically raised to a position in which no inducing effect was observed on establishing or destroying the inducing current. The last experiment consisted in depressing the secondary coil to its original position; the intensity of the secondary currents produced by change of relative position was also observed. The conducting-power of the secondary circuit was =119, and the intensity of the inducing current =tan 28°. The following deflections were observed. The signs denote, as above, the side of the scale towards which the magnetic needle was deflected.

The primary circuit broken.	The secondary coil raised.
−36·0	−36·0
35·8	36·8
36·0	36·4
36·2	36·8
Mean −36·0	Mean −36·5

The circuit closed.	The coil lowered.
+36·2	+36·0
36·4	36·2
36·4	35·4
Mean +36·0	Mean +35·9

Thus, according to the theory, the intensity of the secondary current is the same for all four ways of effecting the induction.

XXXVIII. *The Vibrations which Heated Metals undergo when in contact with cold Material, treated mathematically.* By A. S. Davis, *M.A.**

UNDER certain conditions a heated piece of metal, when laid upon a cold block of metal or some other suitable material, will vibrate by rocking rapidly about upon its points of support; and the vibrations will continue as long as the heated metal continues much hotter than the cold block upon which it rests.

One of the conditions essential for the production of these vibrations is that the heated metal must be so shaped and so placed upon the cold block that it would, if cold, rock rapidly to and fro for a short time upon being slightly tilted and left to itself. This condition may be attained by shaping the piece of metal with two parallel ridges near together, on which it may rest upon the cold block, whilst it finds a third point of support on the table upon which the block is placed. The heated piece of metal may consist of brass, copper, iron, or generally of any of those metals which are good conductors of heat. The best materials known for the cold block are lead and rock-salt.

Sir John Leslie first suggested that the cause of these vibrations is to be found in the expansion of the cold block by the heat which flows into it from the hot metal at the points of contact. Faraday*, Seebeck†, and Tyndall‡ have adopted this explanation; and they have shown that most of the facts which they and others have ascertained respecting these vibrations are easily explained upon this view of their cause, supposing only that the expansion is sufficiently great to produce any sensible effect.

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* Communicated by the Author.

Professor James Forbes§, on the other hand, after an extensive series of experiments, was led to reject Sir John Leslie's explanation, one of his principal reasons for doing so being the impossibility, as it appeared to him, that the expansion occasioned by so slow a process as the conduction of heat could produce any sensible mechanical effect. He says, "Even at first sight it does appear very difficult to conceive how, when the vibrations are increased to 500 or more in a second, a process depending upon so slow an operation as the conduction of heat should cause the metal to expand and contract successively by a finite quantity. The effect has every appearance of being one of active and almost instantaneous repulsion, and bears no resemblance whatever to the slow mechanical elevation of the surface by the process of expansion." After remarking that such inferences are often erroneous, he enters into a closer consideration of the phenomenon, but finds no reason for altering the opinion expressed in the passage just quoted.

It thus appears to be an interesting problem to determine by calculation the amount of expansion produced in the block in any given time, and to find whether it is sufficiently great to cause the vibrations in question. This is what I have attempted to do in the present paper; and the conclusion at which I have arrived confirms the truth of Sir J. Leslie's explanation.

I. To find the expansion produced in any given time.

It will be convenient to consider in the first place the following problem:—A large heated piece of metal is laid upon a large cold piece of the same kind of metal, the surfaces in contact being horizontal planes. To find the height through which the surface of the cold metal has been raised in any given short time.

The time being short, the depth to which the heat will have penetrated will be small; and the pieces of metal may therefore be considered infinite in extent. We have, then, for determining the temperature v at any depth x after the time t, the following equation,

$$v=v_0-\frac{2V}{\sqrt{\pi}}\int_0^{\frac{x}{2\sqrt{kt}}}\epsilon^{-z^2}.dz, \quad . \quad . \quad . \quad . \quad (1)$$

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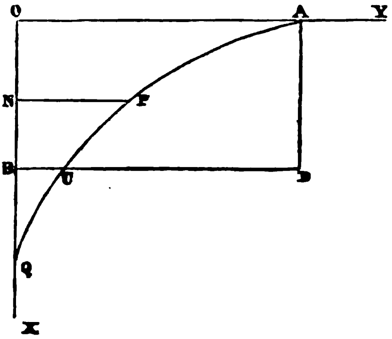
* Proc. of Roy. Inst. vol. ii. p. 119. † Pogg. *Ann.* vol. li.

‡ Proc. of Roy. Inst. 1854. See also 'Heat as a mode of Motion,' p. 127.

§ Phil. Mag. vol. iv.

where V is half the difference between the initial temperatures of the two pieces of metal, v_0 is their arithmetic mean, k is the conductivity of the metal in terms of the thermal capacity of the unit of bulk. (See Thomson and Tait, 'Natural Philosophy,' vol. i. p. 717.)



If O Y be taken in the surface of the metal and O X vertically downwards, and if we take O A to represent V, and N P to represent $v-(v_0-V)$ (*i. e.* the excess of the temperature of the metal at the depth O N $(=x)$ at the time t over the initial temperature of the metal), the curve A P Q will be very near the axis of X at all depths greater than $4\sqrt{kt}$, because the value of the integral $\frac{1}{\sqrt{\pi}}\int_0^w e^{-z^2}dz$ is very nearly equal to $\frac{1}{2}$ for all values of $w > 2$.

Take $OB=2\sqrt{kt}$, and complete the rectangle O D.

The expansion which the metal has undergone will be proportional to the area O A Q; and this expansion bears the same proportion to the expansion which the metal would undergo if heated uniformly to a temperature v_0 down to a depth O B as the area O A Q bears to the area of the rectangle O D. The ratio which the area O A Q bears to O D is constant, and by careful measurement is found to be nearly as 6 : 10. (This curve is carefully drawn in Thomson and Tait's 'Natural Philosophy,' vol. i. p. 719.)

Let d be the increase of unit length of the metal for an increase of one degree in the temperature. Then the height through which the surface has risen in time t is

$$\cdot 6\times V\times 2\sqrt{kt}\times d. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

When the hot and cold metals are of the same kind, the contraction of the hot metal will be equal to the expansion of the cold metal (supposing the dilatability not to vary with the temperature). In this case, then, the mass of hot metal will not be raised by the flow of heat.

Let us next consider the case in which the hot and cold metals are of different kinds. The common temperature of the surfaces in contact will not be, as in the former case, a mean between the initial temperatures of the two metals.

To find what this temperature will be, let T be its excess over the initial temperature of the cold metal, and let $T_{\prime}$ be the excess of the initial temperature of the hot metal over the same temperature. Then if $d, d', \rho, \rho', \sigma, \sigma'$ be respectively the dilatabilities, specific gravities, and specific heats of the cold and hot metals, it will be seen from what has been already proved that the expansion of the cold metal is to the contraction of the hot metal as

$$Td\sqrt{kt} : (T_{\prime}-T)d'\sqrt{k't}.$$

But if h be the heat which has flowed out of the hot metal into the cold metal, the expansion of the cold metal is $\frac{hd}{\rho\sigma}$; for the total expansion would be the same however the heat were distributed; and it is clear that $\frac{hd}{\rho\sigma}$ would be the expansion if the heat h were uniformly distributed over a unit cube of the metal. In the same manner the contraction of the hot metal will be $\frac{hd'}{\rho'\sigma'}$. Thus we have

$$Td\sqrt{kt} : (T_{\prime}-T)d'\sqrt{k't} :: \frac{hd}{\rho\sigma} : \frac{hd'}{\rho'\sigma'},$$

whence

$$T = T_{\prime} \times \frac{\rho'\sigma'\sqrt{k'}}{\rho\sigma\sqrt{k}+\rho'\sigma'\sqrt{k'}}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

When the two metals are of different kinds, the expansion of the one will not in general be equal to the contraction of the other, and consequently the mass of hot metal will be either raised or lowered.

Let us now consider the case in which the hot metal is copper and the cold metal lead. In this case

$\rho = 11{\cdot}35$ $\qquad \rho' = 8{\cdot}88$

$\sigma = {\cdot}0314$ $\qquad \sigma' = {\cdot}095$

$d = 0{\cdot}000028$ $\qquad d' = 0{\cdot}000017.$

Taking a decimetre as unit of length, and a second as unit of time, and 1° C. as unit of temperature,

$$\sqrt{k} = {\cdot}0327, \quad \sqrt{k'} = {\cdot}0475.$$

Substituting, we obtain

$$T = {\cdot}77 \times T_{\prime}.$$

Hence the expansion of the lead is

$$1{\cdot}2 \times 0{\cdot}000028 \times {\cdot}0327 \times {\cdot}77 \times T_{\prime} \times \sqrt{t}$$
$$= {\cdot}000000846 \times T \times \sqrt{t};$$

and the contraction of the copper is

$$1{\cdot}2\times 0{\cdot}000017\times{\cdot}0475\times{\cdot}23\times T_{,}\times\sqrt{t}$$
$$={\cdot}000000223\times T_{,}\times\sqrt{t}.$$

Therefore the copper is raised in time t through a space

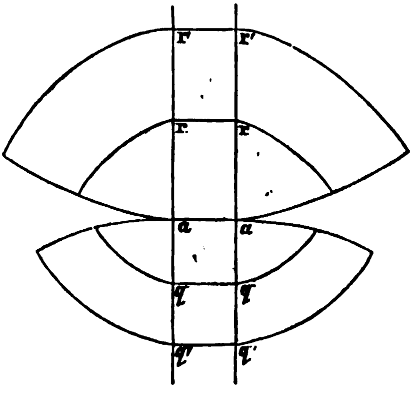
$$\cdot000000623\,T_{,}\sqrt{t}\ \text{decimetre},$$

or

$$\cdot000062\,T_{,}\sqrt{t}\ \text{millimetre}.$$

Let us now consider the case in which the area of the surfaces in contact is very small, which will be the case applicable to the rocker and block.

If the heat which flows out of the hot metal and into the cold metal were wholly confined to those parts which are vertically above and below the surfaces in contact, then the height to which the surfaces would rise would be given by the expression already found. But in general a certain portion of the heat will flow laterally; and consequently the height to which the surfaces rise will in general be less than the expression already found. The proportion of heat which escapes laterally will be greater the greater the depth to which the heat has flowed.



Thus let *a a* be the part of the surfaces in contact. When the heat has flowed to the depth *q' q'*, the whole amount of heat which has escaped laterally bears a larger proportion to the heat which has flowed directly downwards than it does when the heat has only flowed to the depth *q q*. It will be seen also that the proportion of heat which escapes laterally depends upon the ratio of the diameter of the surface in contact to the depth to which the heat has flowed.

Assuming, as a probable value, ·2 millim. for the diameter of the surface in contact, and supposing that the vibrations are taking place at the rate of 225 per second, let us inquire what, under these circumstances, will be the proportion of heat which flows laterally. It will be seen from the figure that only a small part of the heat which has flowed into the lead has penetrated to a depth greater than $2\sqrt{kt}$, and that two thirds of the whole amount is confined to a depth less than $\sqrt{kt}$. Hence, in the case under consideration, only a small part of the heat has penetrated ·44 millim. into the lead, and two thirds of the whole amount has not penetrated ·22 millim. In the same way it will be found that but little heat has flowed out of the copper from a depth greater than ·63 millim., and that two thirds of the whole amount has come from a depth less than ·32 millim. If, then, we draw the figure so that $a\,a$, $a\,q$, $a\,q'$, $a\,r$, $a\,r'$ are proportional respectively to the numbers 20, 22, 44, 32, 63, we may, by considering the proportions which the masses of heated and cooled metal vertically above and below the surfaces in contact bear to the whole masses of heated and cooled metals respectively, form a rough estimate of the proportion of heat which is available in raising the rocker.

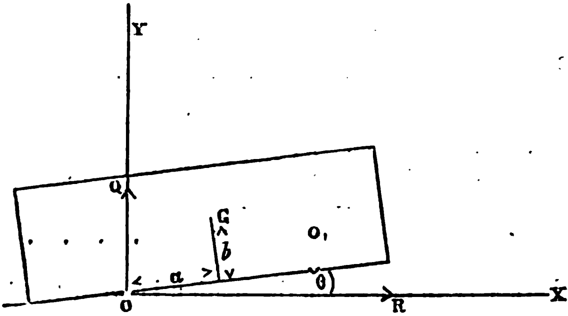
Taking one fifth of the heat which flows into the lead as available in raising the rocker, and one tenth of that which flows out of the copper as available in diminishing the height to which the rocker is raised, the height through which the rocker is raised will be

$$\cdot 0000147 \times T_1 \times \sqrt{t} \text{ millim.*}, \quad . \quad . \quad . \quad . \quad (4)$$

when t has a value about $\frac{1}{225}$.

II. Taking this as the correct expression for the height through which the rocker is raised, I now proceed to determine the difference of temperature between the copper and lead necessary to produce continuous vibrations at a given rate in a rocker of given shape.

Let the rocker be a rectangular parallelopiped with two parallel ridges on its underside. Let the point of support which rests upon the table be so far removed from the body of the



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[*] I here assume that the total flow of heat into the lead is the same as if there were no lateral flow of heat. The fact that a lateral flow of heat occurs will probably increase the total flow, and will also probably have the effect of diminishing the depth to which the heat flows in a given time. For each of these reasons the value found above would be too small.

rocker by means of a long thin rod, and so arranged, that every vertical slice of the body of the rocker has as nearly as possible the same motion. Let M be the mass of the rocker, Mk^2 the moment of inertia about an axis through the centre of gravity parallel to the length of the rocker, $2a$ the distance between the ridges, $2b$ the depth, and $2c$ the breadth of the rocker. Let ω, ω' be the angular velocities just before and just after the impact of the rocker upon the point O. Let R, Q be the horizontal and vertical components of the impulse, u, u' the horizontal, v, v' the vertical component velocities of the centre G, ω, ω' the angular velocities of the rocker before and after impact, θ the inclination of the rocker. The equations of impulsive motion are

$$M(u'-u)=R,$$
$$M(v'-v)=Q,$$
$$Mk^2(\omega'-\omega)=R\,.\,y-Q\,.\,x.$$

Eliminating R and Q, we have

$$k^2(\omega'-\omega)=y(u'-u)-x(v'-v). \quad . \quad . \quad . \quad . \quad (5)$$

If x, y be the coordinates of G, and H the height of O' above O X, the geometrical relations are, before impact,

$$2a-x=a\cos\theta+b\sin\theta,$$
$$y-H=b\cos\theta-a\sin\theta;$$

whence, by differentiation,

$$-u=(-a\sin\theta+b\cos\theta)\omega,$$
$$v-\frac{dH}{dt}=(-b\sin\theta-a\cos\theta)\omega.$$

After impact the geometrical relations are

$$x=a\cos\theta-b\sin\theta,$$
$$y=a\sin\theta+b\cos\theta;$$

whence

$$u'=(-a\sin\theta-b\cos\theta)\omega',$$
$$v'=(\quad a\cos\theta-b\sin\theta)\omega'.$$

Substituting these values in (5) and neglecting very small quantities, we obtain

$$\omega=\frac{k^2+b^2+a^2}{k^2+b^2-a^2}\times\omega'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

We have now to determine the motion between two impacts. If the vibrations are continuous, the angular velocity acquired just before the next impact upon the other point of support O' must be equal to $-\omega$. Immediately after the impact upon O, the point O begins to rise. Let h be the height to which it has risen in the time t. Let R', Q' be the horizontal and vertical components of the pressure upon the rocker at the point O at the time t. Then the equations of motion are

$$M\frac{d^2x}{dt^2}=R',$$

$$M\frac{d^2y}{dt^2}=Q'-Mg,$$

$$Mk^2\frac{d^2\theta}{dt^2}=R'y-Q'x.$$

The geometrical relations are

$$x=a\cos\theta-b\sin\theta,$$

$$y=b\cos\theta+a\sin\theta+h.$$

From these equations we obtain

$$k^2\frac{d^2\theta}{dt^2}=-x\left(g+\frac{d^2h}{dt^2}\right)$$

$$=-a\left(g+\frac{d^2h}{dt^2}\right) \text{ very nearly.}$$

Integrating and noticing that when $t=0$, $\frac{d\theta}{dt}=\omega'$, we have

$$k^2\frac{d\theta}{dt}=-agt-a.\frac{dh}{dt}+k^2\omega'. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

Integrating again and noticing that when $t=0$, $h=0$, and $\theta=\frac{H}{2a}$, we have

$$k^2.\theta=-\frac{agt^2}{2}-ah+k^2\omega't+k^2.\frac{H}{2a}.$$

Now, just before the next impact O will have risen to a height H, and O′ will have sunk down to its original position. Therefore the values of θ and $\frac{d\theta}{dt}$ will be respectively $-\frac{H}{2a}$ and $-\omega$. Hence, if π be the period of the vibration, we have

$$\frac{k^2.H}{a}=\frac{ag\pi^2}{2}+a.H-k^2\omega'\pi;$$

$$\therefore\ \omega'=\frac{a^2g.\pi^2+2H(a^2-k^2)}{2ak^2\pi}. \quad . \quad . \quad . \quad . \quad (8)$$

From (7) we have

$$k^2(\omega+\omega')=ag\pi+a\frac{dh}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

From (8) and (9) we obtain

$$\frac{\omega-\omega'}{\omega+\omega'}=\frac{a^2\pi\frac{dh}{dt}+2\mathrm{H}(k^2-a^2)}{a^2g\pi^2+a^2\frac{dh}{dt}\pi}.$$

But from (6),

$$\frac{\omega-\omega'}{\omega+\omega'}=\frac{a^2}{k^2+b^2}.$$

Also

$$\mathrm{H}=\cdot0000147\times\mathrm{T}_{\prime}\sqrt{\pi},$$

and

$$\frac{dh}{dt}=\frac{\cdot0000074\times\mathrm{T}_{\prime}}{\sqrt{\pi}};$$

$$\therefore\ \frac{a^2}{k^2+b^2}=\frac{\cdot0000147\times\mathrm{T}_{\prime}\left(\frac{k^2}{a^2}-3\right)}{2g\pi^{\frac{3}{2}}+\cdot0000147\times\mathrm{T}_{\prime}}.$$

In the rocker I have employed, $a=2\cdot5$, $b=22$, $c=10$ millims.

Putting also $g=9800$ and $\pi=\frac{1}{225}$ and reducing, we obtain

$$\mathrm{T}_{\prime}=10^\circ\ \mathrm{C.}\ \text{very nearly.}$$

Thus we conclude that if, after each impact, the heated lead and the cooled copper in the neighbourhood of the point of contact were to regain their initial temperatures before the next impact upon the same point took place, then a difference of only 10° C. between the two metals would be sufficient to keep up a vibration of the rocker at the rate of 225 impacts per second. This, of course, will not be the case; but it is not difficult to see that, owing to the fact that the little masses of heated lead and cooled copper are each half surrounded by metal, the reduction of the temperature of the metals to the temperature of the surrounding metal by the process of conduction will take place very rapidly. Thus if, just after the contact is broken, the heat has spread through a portion of the lead (approximately hemispherical) to a depth d, say, then before the contact is again made the heat derived from the previous contact will have spread to a depth $\sqrt{2}\,.\,d$, and will occupy a space approximately $2\sqrt{2}$ times that which it occupied when the last contact was broken. Consequently the increment of temperature produced by the previous contact will be diminished to $\frac{1}{2\sqrt{2}}$ of its former amount.

In the same manner the increment of temperature gained by the last contact but one will be diminished to about one eighth of its former amount. Only a very few of the previous contacts will therefore have any appreciable effect in causing the temperatures of the two metals at their points of contact to differ by a smaller amount than the temperatures of the general masses of the two metals. A difference then of 50° or 100° in the temperatures of the two metals will probably be quite sufficient to produce the vibrations in question.

It has already been seen that the larger the areas in contact, the greater the proportion of heat available for raising the block. For this reason large areas in contact are advantageous for the production of vibrations. But, on the other hand, the larger the areas in contact, the more slowly will the two metals tend to regain their original difference of temperature during the intervals when they are not in contact; and for this reason large areas in contact are disadvantageous for the production of vibrations. There will therefore be a certain magnitude for the area in contact which will give stronger vibrations than either larger or smaller areas. This conclusion is borne out by experiment.

I have pointed out that when the hot and cold metals are of the same material there will in general be no vibration, because the contraction of the hot metal will exactly compensate for expansion of the cold metal. This, as a result of his experiments, Forbes has stated to be a general law. There is, however, an exception to this law, first noticed and explained by Professor Tyndall, to which I should refer. He found that when the hot rocker rests upon the edges of thin sheets or upon points of the same metal as itself, vibrations will often occur. The explanation is to be found in the fact that, while a lateral flow of heat out of the hot metal takes place, the lateral flow of heat into the cold metal is partially prevented.

Another general result which Forbes deduced from his experiments is, that " the vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting-powers of the metals for heat, the metal having the least conducting-power being necessarily the coldest." Now, if the various metals differed from one another only as regards their conductivities this law would be strictly true; for in this case the possibility of vibration could only arise from the lateral flow of heat out of the hot metal being greater than the lateral flow into the cold metal, and this would only be the case when the hot metal was the better conductor of heat. But in any case the lateral flow of heat has an important influence upon the vibrations; and consequently the above law will be approximately true.

If, however, the lateral flow of heat into the cold metal be prevented by making the rocker rest upon thin sheets or points of cold metal, this law will altogether fail. The experiments of Tyndall prove that this is the case.

Leeds Grammar School,
March 6, 1873.

XXXIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 231.]

January 16, 1873.—T. Archer Hirst, Ph.D., Vice-President, in the Chair.

THE following communication was read:—

"Additional Note to the Paper 'On a supposed Alteration in the Amount of Astronomical Aberration of Light produced by the Passage of the Light through a considerable thickness of Refracting Medium.'" By the President.

Some months since I communicated to the Royal Society* the result of observations on γ Draconis made with the water-telescope of the Royal Observatory (constructed expressly for testing the equality of the coefficient of sidereal aberration, whether the tube of a telescope be filled with air, as usual, or with water) in the spring and autumn of 1871. Similar observations have been made in the spring and autumn of 1872, and I now place before the Society the collected results. It will be remembered, from the explanation in the former paper, that the uniformity of results for the latitude of station necessarily proves the correctness of the coefficient of aberration employed in the Nautical Almanac.

Apparent Latitude of Station.

		° ′ ″
1871.	Spring	51 28 34·4
	Autumn	51 28 33·6
1872.	Spring	51 28 33·6
	Autumn	51 28 33·8

I now propose, when the risk of frost shall have passed away, to reverify the scale of the micrometer, and then to dismount the instrument.

Jan. 23.—T. Archer Hirst, Ph.D., Vice-President, in the Chair.

The following communication was read:—

"Note on the Wide-slit Method of viewing the Solar Prominences." By William Huggins, D.C.L., L.L.D., F.R.S.

When editing the English translation of Schellen's 'Spectrum Analysis,' I discovered that the short account of the method of viewing the forms of the solar prominences by means of a wide slit, which I had the honour of presenting to the Royal Society on February 18, 1869†, does not agree exactly in one respect with the account of the observation of February 13 as it was entered at the time in my observatory book. The short note was written at the suggestion of a friend during a Committee held in the Royal Society's Apartments, and, as the concluding words show, was intended to be followed by a more detailed account of the method of observation. The point in question relates to the position of a second slit which was used to screen the eye from every part of the spectrum except that under observation. The words in my book written at the time are, "narrow slit found to be best at focus of little telescope with positive eyepiece." In the note the second slit was stated to have been placed before the object-glass of the little telescope. Such an arrangement was tried in connexion with some other experiments in progress at the time. The plan of limiting the field of view to the part of the spectrum corresponding to the refrangibility of the light of the prominence, as well as the employment of a ruby glass, is of value when the air is not favourable, or when a spectroscope of small dispersive power is used.

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* Phil. Mag. vol. xliii. p. 310.

† Phil. Mag. S. 4, vol. xxxviii p. 68.

Jan. 30.—George Busk, Esq., Vice-President, in the Chair.

The following communication was read:—

"On Just Intonation in Music; with a description of a new Instrument for the easy control of all Systems of Tuning other than the ordinary equal Temperament of twelve divisions in the Octave." By R. H. M. Bosanquet, Fellow of St. John's College, Oxford.

The object of this communication is to place the improved systems of tuning within the reach of ordinary musicians; for this purpose the theory and practice are reduced to their simplest forms.

A notation is described, adapted to use with ordinary written music, by which the notes to be performed are clearly distinguished.

The design of a key-board is described, by which any system of tuning, except the ordinary equal temperament, can be controlled, if only the fifths of the system be all equal. The design is on a symmetrical principle; so that all passages and combinations of notes are performed with the same handling, in whatever key they occur.

The theory of the construction of scales is then developed; and a diagram is given, from which the characteristics of any required system can be ascertained by inspection.

An account is then given of the application of such systems to the new key-board, and particularly of an harmonium which has been constructed and contains at present the division of the octave into fifty-three equal intervals in a complete form. Rules for tuning are given.

Finally, the application of the system of fifty-three to the violin is discussed.

Throughout, the work of former labourers in the same field is reviewed: the obligations of the writer are due to Helmholtz, the late General T. Perronet Thompson, F.R.S., and others.

Feb. 13.—Rear-Admiral Richards, C.B., Vice-President, in the Chair.

The following communication was read:—

"On a new Relation between Heat and Electricity." By Frederick Guthrie.

It is found that the reaction between an electrified body and a neighbouring neutral one, whereby the electricity in the neutral body is inductively decomposed and attraction produced, undergoes a modification when the neutral body is considerably heated.

Under many circumstances it is found that the electrified body is rapidly and completely discharged. The action of discharge is shown to depend mainly upon the following conditions:—(1) the temperature of the discharging body, and its distance from the electrified one; (2) the nature (+ or —) of the latter's electricity.

With regard to (1), it is shown that the discharging power of a hot body diminishes as its distance increases, and increases with its temperature; but, concerning the temperature, it is proved that the discharging power of a hot body does not depend upon the quantity of heat radiated from it to the electrified body, but chiefly upon its quality. Thus a white-hot platinum wire connected with the earth may exercise an indefinitely greater discharging power, at the same distance, than a large mass of iron at 100° C., though the latter may impart more heat to the electrified body.

Neither the mere reception of heat, however intense, by the electrified body, unless the latter have such small capacity as to be itself intensely heated, discharges the electricity if the source of heat be distant; nor is discharge effected when the electrified body and a neighbouring cold one are surrounded by air through which intense heat is passing. But, for the discharge, it is necessary that heat of intensity pass to the electrified body from a neutral body, within inductive range.

White- and red-hot metallic neutral bodies exercise this discharging power even when isolated from the earth, but always with less facility than when earth-connected.

The hotter the discharging body, whether isolated or earth-connected, the more nearly alike do + or — electricities behave in being discharged; but at certain temperatures distinct differences are noticed. The — electricity, in all cases of difference, is discharged with greater facility than the +.

Attempts are made to measure the critical temperatures at which earth-connected hot iron (1) discharges + and — electricity with nearly the same facility, (2) begins, as it cools, to show a preferential power of discharging —, and (3) ceases to discharge —. The temperatures so obtained are measured by the number of heat-units, measured from 0° C., in 1 gram of iron of the respective temperature, represented by the value of the expression $Fe\,\Sigma_0^t u$.

It is shown that various flames, both earth-connected and isolated, have an exceedingly great power of discharging both kinds of electricity.

The effects in regard to discharge are shown to be similar when platinum wire, rendered hot by a galvanic current, is used, and also when the condensed electricity of a Leyden jar is experimented on.

As hot iron shows a preferential power of discharging — over + electricity, so it is found that white-hot but isolated iron refuses to be charged either with + or — electricity. As the iron cools, it acquires first the power of receiving —; and afterwards of receiving +. Further, while white-hot iron in contact with an electrified body prevents that body from retaining a charge of either kind of electricity, as it cools it permits a + charge to be received, and subsequently a — one.

A suggestion is made as to the existence of an electrical coercitive force, the presence of which together with its diminution by heat would explain much of what has been described.

Feb. 20.—Rear-Admiral Richards, C.B., Vice-President, in the Chair.

The following communication was read:—

"On a new Locality of Amblygonite, and on Montebrasite, a new Hydrated Aluminium and Lithium Phosphate." By A. O. Des Cloizeaux.

A mineral found in 1862 at Hebron, Maine, U. S. A., after a mere tentative examination by Professor Brush, who announced the presence in it of lithia in considerable quantity, resembled the amblygonite of Penig so closely as to lead to its being looked on as amblygonite. The crystalline system and birefringent optical characters of this mineral were determined by the author in 1863. In 1870 a mineral found in the tin vein of Montebras (Creuse), though resembling the amblygonite of Hebron, appeared to the author to differ from it so far as to justify his designation of it under the name of Montebrasite. Towards the close of 1871 he received another specimen from Montebras, which presented all the characters of the American amblygonite, and which consequently was easily distinguished from the montebrasite. Subsequently, analyses by Pisani, v. Kobell, and Rammelsberg, and optical observations by the author, proved the identity of the montebrasite of Montebras with the amblygonite from Penig. But this is not the case with the amblygonite from Hebron, nor with that from Montebras, which had been analyzed by Pisani. These differ from the amblygonites of Saxony and Montebras (which last he had previously named montebrasite) by the absence of soda, by the preponderance of lithia, and the presence of a notable amount of water, while at the same time they contain almost equal proportions of phosphoric acid and alumina.

The differences which these two minerals present in their physical and chemical characters are sufficiently decided to compel our treating them as distinct species. The name amblygonite should be retained for the sodolithic species first discovered at Penig by Breithaupt; and the white or violet-tinted lamellar masses abundant at Montebras will be included under it; the hydrated and entirely lithic species, comprising the laminar specimens and the crystals from Maine, as well as some greenish masses from Montebras, should be embraced under the name montebrasite.

The amblygonite of Montebras has only been met with in laminar masses with a faint tinge of violet. These masses exhibit two cleavages presenting nearly the same degree of facility, making with one another an angle of 105° 44′. Close observation shows that the sharpness of the reflected images is generally a little greater on one of the cleavages than on the other; and this induces one to suppose that they do not both belong to equivalent crystallographic planes. The study of some of their optical properties, though presenting certain special difficulties, arising from the small extent of the transparent portions and the presence of numerous twin plates, even in the specimens that to all appearance are the most homogeneous, has proved that the laminar masses of montebrasite must be referred to the triclinic system. The optic axes are situated in a plane which divides into two very unequal parts the acute angle of 74° 16′ of the two cleavages. This direction is entirely different from that found for montebrasite of Hebron and of Montebras, in which the plane of the axes lies in the obtuse angle of 105° formed by the two principal cleavages.

The appearance of the bars traversing the central ring of each system indicates very distinctly a twisted dispersion, as well as a small amount of inclined dispersion, which is characteristic of a crystal belonging to the triclinic system.

In November 1871 the author received a specimen from the middle of a mass of amblygonite from Montebras resembling the mineral from Hebron. It has three principal cleavages, *p*, *m*, *t*, which the author recognized in the mass from Hebron, the angles between which are $p\ m=105°$, $m\,t=135°$ to $136°$, $p\,t=89°$ to $89°\ 15'$.

By means of artificial twins formed of two plates, each of which had been worked perpendicular to the two cleavages *p* and *m*, and which were united by their faces *p*, it appeared that the plane of the optic axes is situated in the obtuse angle *p m*, and traverses the edge $\frac{p}{m}$, but that it is not quite normal to *m*, since it gives angles of about 82° with *m* and 23° with *p*. The character of the coloured rings shows that in montebrasite of Montebras, as in that from Hebron, there coexists with the horizontal a well-marked inclined dispersion; and these are peculiar to crystals of the triclinic system.

Analyses by M. Pisani.

	Hebron.	Montebras.
Fluorine	5·22	3·80
Phosphoric acid	46·65	47·15
Alumina	36·00	36·90
Lithia	9·75	9·84
Water	4·20	4·75
	101·82	102·44
Specific gravity	3·03, Pisani. 2·99, Damour.	3·01, Pisani. 2·977, Damour.

Wavellite, in the form of thin coatings, forms a layer over almost all the fissures that occur in the amblygonite of Montebras. In cavities in these coatings are found long thin needles, which have enabled the author to correct the older measurements of this mineral.

M. Pisani has ascertained that the variety from Montebras yields:—

Fluorine	2·27
Phosphoric acid	34·30
Alumina	38·25
Water	26·60
	101·42
Specific gravity	2·33

GEOLOGICAL SOCIETY.

[Continued from p. 235.]

November 6, 1872.—Prof. Ramsay, F.R.S., V.P., in the Chair.

The following communications were read:—

1. "A Report by F. T. Gregory, Esq., Mining-land Commissioner in Queensland, on the recent discoveries of Tin-ore in that Colony."

According to this report, the district in Queensland in which tin-ore has been discovered is situated about the head-waters of the Severn river and its tributaries, comprising an area of about 550 square miles. The district is described as an elevated granitic tableland intersected by ranges of abrupt hills, some attaining an elevation of about 3000 feet above the sea. The richest deposits are found in the beds of the streams and in alluvial flats on their banks, the payable ground varying from a few yards to five chains in extent. The aggregate length of these alluvial bands is estimated at about 170 miles; the average yield per linear chain of the stream-beds at about ten tons of ore (cassiterite).

Numerous small stanniferous lodes have been discovered, but only two of much importance—namely, one near Ballandean Head Station on the Severn, and another in a reef of red granite rising in the midst of metamorphic slates and sandstones at a distance of about six miles. The lodes run in parallel lines bearing about N. 50° E.; and one of them can be traced for a distance of nine or ten miles. The ore, according to Mr. Gregory and Mr. D'Oyly Aplin, is always associated with red granite—*i. e.* "the felspar a pink or red orthoclase, and the mica generally black; but when crystals of tin-ore are found *in situ*, the mica is white." The crystals of tin-ore are generally found in and along the margins of quartz threads or veins in bands of loosely aggregated granitoid rock, but are sometimes imbedded in the micaceous portions. The report concludes with some statements as to the present condition and prospects of the district as regards its population.

2. "Observations on some of the recent Tin-ore Discoveries in New England, New South Wales." By G. H. F. Ulrich, Esq. F.G.S.

The district referred to by the author is in the most northern part of the colony of New South Wales, almost immediately adjoining the tin-region of Queensland described in the preceding report. It forms a hilly elevated plateau, having Ben Lomond for its highest point, nearly 4000 feet above the sea-level. The predominant rocks are granite and basalt, enclosing subordinate areas composed of metamorphic slates and sandstones; the basalt has generally broken through the highest crests and points of the ranges, and spread in extensive streams over the country at the foot.

The workings of the Elsmore Company, situated on the north-west side of the Macintyre river, about twelve miles E. of the township of Inverell, include a granite range about 250 feet in height and nearly two miles in length. The granite of the range is micaceous, with crystals of white orthoclase, and is traversed by quartz-veins which contain cassiterite in fine druses, seams, and scattered crystals, and by dykes of a softer granite, consisting chiefly of mica, and with scarcely any quartz, in which cassiterite is distributed in crystals, nests, and bunches, and also in irregular veins several inches in thickness. This granite yields lumps of pure ore up to at least 50 lbs. in weight. The quartz-veins contain micaceous portions which resemble the "Greisen" of the Saxon tin-mines. The deepest shaft sunk in one of the quartz-veins was about 60 feet in depth. The author noticed certain minerals found in association with the tin-ore, and the peculiarities of the crystalline forms presented by the latter.

The drift is very rich, and consists of a generally distributed recent granitic detritus, from 6 in. to 2 ft. thick, and of an older drift (probably Pliocene) capping the top of the range, and probably dipping beneath the adjoining basalt. The washing of the granitic detritus gives from 3 ozs. to more than 2 lbs. of ore per dish (of about 20 lbs.). The older drift is rather poor in tin to within about a foot of the bottom; but the bottom layer is in part very rich, some having yielded as much as 6 lbs. of ore per dish.

The author also described the Glen Creek, about 40 miles north of the Elsmore mine, from the surface-deposits of which tin-ore has been obtained by washing. The course of the creek is mostly through a black, hard slate destitute of fossils; but at one part, for about 10 chains, its bed consists of a fine-grained hard granite, with numerous veins of arsenical and copper pyrites, and one solid vein of tin-ore, about ¾ in. in thickness, all of which pass from the granite into the slate without any interruption or change, the passage from one rock into the other being also gradual.

The chief underlying rock of the district is a black slate; but dispersed through it are small outcrops of a rather coarse-grained micaceous granite, close to one of which several veins of solid tin-ore, from 1 to 4 inches thick, have been found traversing the slate rock. The tin-ore disseminated through the surface-deposits has been derived from these veins and from a very hard and tough greenstone (diabase), which occurs in large dykes and patches in various places, and is probably younger than the granite.

In conclusion the author referred to the probability that a deficiency of water may prove a great obstacle to the full development of the tin-mining industry in this district, but stated that "it seems not unlikely that the production of tin-ore from this part of Australia will reach, if not surpass, that of all the old tin-mining countries combined."

3. "On the included Rock-fragments of the Cambridge Upper Greensand." By W. Johnson Sollas and A. J. Jukes-Browne.

The occurrence of numerous subangular fragments in the Upper Greensand formation was so far remarkable that it had already attracted the notice of two previous observers (Mr. Bonney and Mr. Seeley), who had both briefly hinted at the agency of ice. While ignorant of the suggestions of these gentlemen, the authors of this paper had been forced to the same conclusion. A descriptive list had been prepared of the most remarkable of the included fragments. The infallible signs of the Upper Greensand origin consisted in incrustations of *Plicatula sigillum*, *Ostrea vesiculosa*, and "Coprolite," without which, it was stated, the boulders would be undistinguishable from those of the overlying drift. The following generalizations were then put forword:—

1. The stones are mostly subangular; some consist of friable sandstones and shales, which could not have borne even a brief journey over the ocean-bed.

2. Many are of large size, especially when compared with the fine silt in which they were imbedded; the stones and silt could not have been borne along by the same marine current.

3. The stones are of various lithological characters, and might be referred to granitic, schistose, volcanic and sedimentary rocks, probably of Silurian, Old Red Sandstone, and Carboniferous age.

Such strata are not found *in situ* in the neighbourhood; and the blocks must have come from Scotland or Wales. Numerous arguments were adduced in favour of their Scottish derivation.

The above considerations, that numerous rock-fragments, some of which are very friable, have been brought from various localities and yet retain their angularity, were thought sufficient evidence for their transportation by ice. The majority showed no ice-scratches; but the small proportion of scratched stones in the moraine matter borne away on an iceberg, and the small percentage of ice-scratched boulders in many deposits of glacial drift, show that the absence of these striæ is not inconsistent with the glacial origin of the included fragments. Besides this, the stores of the Greensand consisted of rock from which ice-marks would readily have been removed by the action of water. The authors stated, however, that they had found more positive evidence in a stone which was unmistakably ice-scratched, consisting of a siliceous limestone, and preserved in the Woodwardian Museum. The fauna, so far as it proved any thing, suggested a cold climate; though abundant, the species were dwarfed, in striking contrast to those of the Greensand of Southern England and the fauna of the succeeding Chalk. The authors concluded that a tongue of land separated the Upper-Greensand sea into two basins, the northern of which received icebergs from the Scottish-Scandinavian chain; the climate of this was cold, that of the southern basin much warmer.

XL. *Intelligence and Miscellaneous Articles.*

ON THE ELECTRICAL RESISTANCE OF METALS. BY M. BENOIST.

IT has long been known that the electrical resistance of metals increases as their temperature rises. This increase has been measured up to 100° by M. Becquerel and by Matthiessen, and to 200° in some metals by M. Lenz and, more recently, by M. Arndtsen. I proposed to myself to trace the variation beyond these limits, and to determine the increment of specific resistance at very high temperatures.

Calling x the specific resistance of a metal (that is to say, its resistance in unit length and unit section), the resistance of a wire of the same metal of length l and section s is, according to Davy's laws,

$$R = x\frac{l}{s},$$

or, substituting for s its value as a function of the volume V, weight P, and density D, of the wire,

$$R = \frac{xl^2}{V} = \frac{xDl^2}{P}.$$

If D, P, and l are known, and if R at t° be determined, the value of the specific resistance at that temperature can be deduced from the last relation.

To measure R, I have chiefly employed the differential-galvanometer method of M. Becquerel. The current from two Daniell's elements was divided into two equal parts, which passed, in opposite directions, through the two wires of a very delicate differential-galvanometer. The wire to be studied was intercalated in one of the circuits, in the other a length of the wire of a rheostat, of which the resistance was equivalent when the needle was at zero. The resistances R, R′, ... of different wires submitted to experiment were proportional to the lengths l, l', ... of rheostat-wire which had served to measure them; and in order to express them as functions of a given unit, it was sufficient if the ratio of the rheostat-wire itself to this unit had been determined once for all. The rheostat consisted simply of two identical, very regular platinum wires, stretched parallel on a horizontal rule of two metres length. These wires traversed a cork cup containing mercury, carried by a cursor movable along the rule. The current, arriving through the first wire, traversed the mercury and issued through the second wire. On the rule was a scale of millimetres; and shifting the cursor n divisions increased or diminished the length of the circuit the value of $2n$. I shall not dwell upon the details, which permitted great precision to be attained, nor on the verifications which I made of the method and of the apparatus.

The wire under examination was soldered at each end to a copper rod, then wrapped round a cylinder of pipe-clay, and, finally, heated in a narrow and deep muffle which occupied the axis of a large wrought-iron jar. This was placed in a gas-stove with two concentric envelopes; by introducing a suitable volatilizable substance, and heating to ebullition, the whole apparatus, and consequently the wire, was brought to a fixed and known temperature.

By determining thus the resistances of one and the same metal at various known temperatures, a number of points are obtained, from which the *curve of the resistances* can be constructed and its elements calculated.

The following are the temperatures which served for my determinations:—

Ebullition of water .	100°	Ebullition of sulphur .	440°
„ mercury	360	„ cadmium	860

I made, besides, a great number of measurements below 360°, the apparatus being filled with mercury and heated by a regular current of gas; the temperature was indicated by thermometers placed in the muffle at different depths.

The results obtained by the preceding method were controlled and confirmed by determinations with Wheatstone's bridge, with the aid of a set of resistances similar to those employed in telegraphy.

The results are summed up in the Tables which follow.

In the first the conductivities at zero are expressed as functions of the two units which are now-a-days usually employed:—the theoretic absolute unit, or *ohm*, proposed by the British Association; and the mercury unit, adopted by M. Werner Siemens. The third column gives the ratios of the conductivities to that of silver, in order that the results may be compared with the well-known coefficients of MM. Becquerel, Lenz, Matthiessen, &c.

The second Table gives the formulæ of the increment of the resistance with the temperature. This takes place regularly up to the melting-point, following the ordinates of a curve the abscissæ of which represent the corresponding temperatures, and which generally differs very little from a right line. Comparing the resistances to the resistance at zero, they can be expressed by a formula of the form

$$R_t = R_0(1 + at + bt^2).$$

The constants a and b were calculated by the method of least squares, which makes all the observations cooperate for the determination of the most probable values of the unknown quantities.

The increment varies from one metal to another. In steel and in iron the initial resistance is doubled at about 170°; in silver, copper, and gold, at about 255°; in platinum, at about 455°. In alloys the increment is in general less: in German silver, for example, at 860° the resistance has increased by only 0·3 of its value at zero. The numbers in this Table express the variation of the *specific resistance—i. e.* of the resistance reduced in each case to the unit of length and section. If we wish to use them to calculate the resistance at t° of *a given wire* the resistance of which at zero is known, account must be taken of the changes of dimension of the wire; in other words, the resistance obtained must be multiplied by $\frac{1}{1+\delta t}$, δ being the coefficient of dilatation. This correction cannot be neglected when the temperature exceeds certain limits.

Specific Resistances of Metals at Zero.

	Resistance of 1 metre length and 1 square millimetre section.		Conductivities compared with silver.
	ohm.	Siemens unit.	
Pure silver, annealed	0·0154	0·0161	100
Copper, annealed	0·0171	0·0179	90
Silver ($\frac{750}{1000}$), annealed	0·0193	0·0201	80
Pure gold, annealed	0·0217	0·0227	71
Aluminium, annealed	0·0309	0·0324	49·7
Magnesium, cold-beaten	0·0423	0·0443	36·4
Pure zinc, annealed at 350°	0·0565	0·0591	27·5
Pure zinc, cold-beaten	0·0594	0·0621	25·9
Pure cadmium, cold-beaten	0·0685	0·0716	22·5
Brass, annealed	0·0691	0·0723	22·3
Steel, tempered	0·1099	0·1149	14·0
Pure tin	0·1161	0·1214	13·3
Aluminium bronze	0·1189	0·1243	13·0
Iron, tempered	0·1216	0·1272	12·7
Palladium, tempered	0·1384	0·1447	11·1
Platinum, tempered	0·1575	0·1647	9·77
Thallium	0·1831	0·1914	8·41
Pure lead	0·1985.	0·2075	7·76
German silver	0·2654	0·2755	5·80
Pure mercury	0·9564	1·0000	1·61

Variation of Resistance with Temperature.

Steel	$R_t=R_0(1+0{\cdot}004978t+0{\cdot}000007351t^2)$
Iron	„ $(1+0{\cdot}004516t+0{\cdot}000005828t^2)$
Tin	„ $(1+0{\cdot}004028t+0{\cdot}000005826t^2)$
Thallium	„ $(1+0{\cdot}004125t+0{\cdot}000003488t^2)$
Cadmium	„ $(1+0{\cdot}004264t+0{\cdot}000001765t^2)$
Zinc	„ $(1+0{\cdot}004192t+0{\cdot}000001481t^2)$
Lead	„ $(1+0{\cdot}003954t+0{\cdot}000001430t^2)$
Aluminium	„ $(1+0{\cdot}003876t+0{\cdot}000001320t^2)$
Silver	„ $(1+0{\cdot}003972t+0{\cdot}000000687t^2)$
Magnesium	„ $(1+0{\cdot}003870t+0{\cdot}000000863t^2)$
Copper	„ $(1+0{\cdot}003637t+0{\cdot}000000587t^2)$
Gold	„ $(1+0{\cdot}003678t+0{\cdot}000000426t^2)$
Silver ($\frac{750}{1000}$)	„ $(1-0{\cdot}003522t+0{\cdot}000000667t^2)$
Palladium	„ $(1+0{\cdot}002787t+0{\cdot}000000611t^2)$
Platinum	„ $(1+0{\cdot}002454t+0{\cdot}000000594t^2)$
Brass	„ $(1+0{\cdot}001599t)$
Aluminium bronze	„ $(1+0{\cdot}001020t)$
German silver	„ $(1+0{\cdot}000356t)$
Mercury	„ $(1+0{\cdot}000882t+0{\cdot}000001140t^2)$.

—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 342–346.

ON THE CONDITIONS REQUISITE FOR THE MAXIMUM OF RESISTANCE OF GALVANOMETERS. BY M. TH. DU MONCEL.

Mr. Schwendler, and several other physicists previously, have found that, for a galvanometer to be in the best possible conditions of sensitiveness in relation to a circuit of given resistance, the resistance of its magnetizing helix must be equal to that of the exterior circuit in communication with it. Several experiments having demonstrated to me that the sensibility increases with the length of the helix-wire, under other conditions than those thus indicated, I submitted to calculation the galvanometric effects with regard to a circuit of given resistance; and I have ascertained that those conditions of sensitiveness demand a considerably greater length of wire in the multiplier than that which corresponds to the resistance of the exterior circuit.

To demonstrate the law which he had laid down, Schwendler endeavours to calculate the number t of the turns of the helix of the multiplier as a function of the space C occupied by the helix-wire, and also as a function of the resistance H of the latter. Designating by s the section of this wire, t becomes equal to $\frac{C}{s}$, and the length H of the helix equal to $\frac{Ct}{s}$; which supposes wrongly that the resistance is proportional to the number of the spiral turns, and inversely as the section of the wire.

According to these data, it would result from the combination of the values of t and H that t would be expressed by $\sqrt{H}$; and as, besides, the value of the intensity of the current is $\frac{E}{R+H}$ (R being the resistance of the exterior circuit, E the electromotive force of the source of electricity), the magnetic moment F of the needle would be

$$F=\frac{E\sqrt{H}}{R+H},$$

an expression susceptible of a maximum for $R=H$.

But in reality the value of t is far from being expressed by $\sqrt{H}$; and if we amend the preceding formula by inserting the true quantities, we arrive at altogether different conditions.

In fact, let a be the thickness of the layers of spires, b the width of the galvanometric framework, c the diameter of the circular part which terminates it on each side, d the length of the framework between these two circular portions, and g the diameter of the wire (including its insulating covering); the number of the turns t will be expressed by $\frac{ab}{g^2}$, and the length H of the helix by

$$H=\frac{ab}{g^2}[(a+c)\pi+2d];$$

consequently the magnetic moment F of the needle will be

$$F=\frac{\alpha Eab}{g^2R+ab[(a+c)\pi+2d]},$$

α denoting the constant of the instrument. Now the conditions of a maximum for this formula, taking a for variable, which is the sole quantity proportional to t, lead to the relation

$$R=\frac{\pi ba^2}{g^2},$$

which shows that the resistance of the galvanometer-wire must be greater than that of the exterior circuit by a quantity represented by

$$\frac{ab}{g^2}(\pi c+2d).$$

We can, moreover, easily assure ourselves of the truth of this deduction by supposing the framework of the galvanometer represented by a simple coil, as in the Thomson galvanometer, and taking the magnetic moments of the needle with the two lengths of the wire of the helix corresponding to the two conditions for a maximum given by Mr. Schwendler and me. For greater simplicity, we will represent the diameter c by $2r$. In these conditions the length H of the helix, instead of being greater than the resistance R by a quantity represented by $\frac{ab}{g^2}(\pi c+2d)$, will be greater only by a quantity $\frac{ab}{g^2}\pi c$, or will be, in proportion to R, as $a+2r$ is to a. We shall therefore have:—

1. With $H=R$,

$$F=\frac{Eab}{2\pi ba(a+2r)};$$

2. With $H=R\frac{a+2r}{a}$,

$$F'=\frac{Eab}{2\pi ba'(a'+r)},$$

which leads to

$$\frac{F'}{F}=\frac{a+2r}{a'+r}.$$

And as then we have $\frac{\pi ba(a+2r)}{g^2}=\frac{\pi ba'}{g^2}$, we deduce $a'=\sqrt{a(a+2r)}$, and consequently

$$\frac{F'}{F}=\frac{a+2r}{\sqrt{a^2+2ar}+r}.$$

As $a+2r$ can be put in the form $\sqrt{a^2+2ar+r^2}+r$, it is at once seen that F' is greater than F.

The experimental verification of the above-exhibited deduction not being easy to realize, on account of the too great sensitiveness of galvanometers with resistant helices and with continual variations of the resistance of the exterior circuit with galvanometers of short helix, I made the experiment with electromagnets, the attractive force of which, reckoned according to the laws of MM. Dub and Jacobi, admits of the same conditions of a maximum relative to the resistance of the magnetizing helices, as I have shown in my researches on the best construction of electromagnets. Now the following are the results I obtained with one and the same electromagnet excited by a Daniell pile of 20 elements, to which I applied successively magnetizing coils of two different resistances, viz. a resistance of 75 kilometres of telegraphic wire of 4 millims. diameter, and a resistance of 200 kilometres. In order to avoid static reactions, the attractive forces were measured with 1 millimetre distance of separation from the armature.

(1) Forces of the electromagnet with coils of 75 kilometres resistance.

	metres.	metres.	Attractive force. grammes.
The exterior circuit having	18620+	0	80
„ „ „	18620+	100000	15
„ „ „	18620+	200000	5·5
„ „ „	18620+	370000	0

(2) Forces of the electromagnet with coils of 200 kilometres resistance.

The exterior circuit having	18620+	0	58
„ „ „	18620+	100000	25
„ „ „	18620+	200000	14
„ „ „	18620+	370000	0

The wire of these circuits was perfectly insulated; and the time during which the current was closed was long enough to develop the maximum of magnetization.

Now we see by these numbers that it is when the resistance of the coils is much superior to the exterior resistance that the maximum is attained, and that, *for the exterior circuit of* 100 *kilometres, the maximum is obtained with the helices of* 200 *kilometres*—that is to say, helices presenting double the resistance. Calculation leads to the same deduction; for in the electromagnet the thickness a of the helix was equal to the diameter c of the electromagnet, and consequently the value of H, which must be equal to $R\frac{a+c}{a}$ to satisfy the conditions of a maximum, was found, under the circumstances of my experiments, to be equal to 2R.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxvi. pp. 368–371.

STRATIFICATION IN A LIQUID IN OSCILLATORY MOTION.

BY J. STEFAN.

Fine powder diffused in a horizontal tube containing a column of water set in motion to and fro distributes itself in slices or strata perpendicular to the direction of the motion. To study this, M. Stefan employed a very simple arrangement, which consists in introducing water holding in suspension oxide of iron into a horizontal tube of glass with two vertical prolongations. To one of these vertical branches is fitted a piece of caoutchouc tubing closed at its free extremity with a cork. By pressing this between the finger and thumb an impulse is communicated to the column of water; and by repeating the pressure at equal intervals a regular reciprocating motion is impressed upon it. The powder which has sunk to the lower part of the glass tube is then distributed in streaks which are so much finer and closer as the motion of the water which carries them with it has less amplitude.

M. Stefan explains this grouping of the particles by the fact that their displacement is effected more easily in one direction than in the other—which depends either upon the water moving less quickly in one of the two phases of its oscillatory motion than in the other, or else on the shape of the particles. We have here a fact analogous to that observed in the production of the lycopodium figures obtained by M. Kundt in acoustic tubes, and having also some affinity to the stratification of the electric light. At least M. Stefan thinks that this latter phenomenon may be attributed to a cause altogether similar to the one just indicated. "In the Geissler tubes," he says, "a part of the gaseous molecules, playing the same part as the powder-particles in the above experiment, receive from the alternate discharges impulses which are more powerful in one direction than in the other. As to the influence which may be exerted by the particular nature of the molecules, it is not necessary to attribute it to their form departing more or less from the spherical; it is sufficient if we invoke the variety of the motions produced by heat in the different molecules at a given moment."—*Archives des Sciences Physiques et Naturelles*, vol. xlvi. p. 270.

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XLI. *On the Ultramundane Corpuscules of* Le Sage, *also on the Motion of Rigid Solids in a Liquid circulating irrotationally through perforations in them or in a Fixed Solid. By* Sir William Thomson, *F.R.S.**

LE SAGE, born at Geneva in 1724, devoted the last sixty-three years of a life of eighty to the investigation of a mechanical theory of gravitation. The probable existence of a gravific mechanism is admitted, and the importance of the object to which Le Sage devoted his life pointed out, by Newton and Rumford† in the following statements:—

"It is inconceivable that inanimate brute matter should, without the mediation of something else, which is not material, operate upon, and affect other matter without mutual contact; as it must do, if gravitation, in the sense of *Epicurus*, be essential and inherent in it. And this is the reason why I desired you would not ascribe innate gravity to me. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers."—NEWTON'S *Third Letter to Bentley*, February 25th, 1692–3.

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* Communicated by the Author, from the Proceedings of the Royal Society of Edinburgh, 1871–72.

† On the other hand, by the middle of last century the mathematical naturalists of the Continent, after half a century of resistance to the Newtonian principles (which, both by them and by the English followers of Newton, were commonly supposed to mean the recognition of gravity as a force acting simply at a distance without mediation of intervening matter), had begun to become more "Newtonian" than Newton himself. On the 4th February, 1744, Daniel Bernoulli wrote as follows to Euler: "Uebrigens glaube ich, dass der Aether sowohl *gravis versus solem*, als die Luft versus terram sey, und kann Ihnen nicht bergen, dass ich über diese Puncte ein völliger Newtonianer bin, und verwundere ich mich, dass Sie den Principiis Cartesianis so lang adhäriren; es möchte wohl einige Passion vielleicht mit unterlaufen. Hat Gott können eine *animam*, deren Natur uns unbegreiflich ist, erschaffen, so hat er auch können eine attractionem universalem materiæ imprimiren, wenn gleich solche attractio *supra captum* ist, da hingegen die Principia Cartesiana allzeit *contra captum* etwas involviren."

"Nobody surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal Newton enabled to make, merely by the investigation of the laws of its action"[*].

Le Sage expounds his theory of gravitation, so far as he had advanced it up to the year 1782, in a paper published in the Transactions of the Royal Berlin Academy for that year, under the title "Lucrèce Newtonien." His opening paragraph, entitled "But de ce mémoire," is as follows:—

"Je me propose de faire voir: que si les premiers Epicuriens avoient eu; sur la Cosmographie des idées aussi saines seulement, que plusieurs de leurs contemporains, qu'ils négligeoient d'écouter[†]; et sur la Géométrie, une partie des connoissances qui étoient déjà communes alors: ils auroient, tres probablement, découvert sans effort; les Loix de la Gravité universelle, et sa Cause mécanique. *Loix*; dont l'invention et la démonstration, font la plus grande gloire du plus puissant génie qui ait jamais existé: et *Cause*, qui après avoir fait pendant longtems, l'ambition des plus grands Physiciens; fait à présent, le désespoir de leurs sucesseurs. De sorte que, par exemple, les fameuses Règles de *Kepler*; trouvées il y a moins de deux siècles, en partie sur des conjectures gratuites, et en partie après d'immenses tâtonnemens; n'auroient été que des corollaires particuliers et inévitables, des lumières générales que ces anciens Philosophes pouvoient puiser (comme en se jouant) dans le mécanisme proprement dit de la Nature. Conclusion; qu'on peut appliquer exactement aussi, aux Loix de *Galilée* sur la chûte des Graves sublunaires; dont la découverte a été plus tardive encore, et plus contestée: joint à ce que, les expériences sur lesquelles cette découverte étoit établie; laissoient dans leurs résultats (nécessairement grossiers), une latitude, que les rendoit également compatibles avec plusieurs autres hypothèses; qu'aussi, l'on ne manqua pas de lui opposer: au lieu que, les conséquences du choc des Atoms; auroient été absolument univoques en faveur du seul principe véritable (des Accélérations égales en Tempuscules égaux)."

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[*] "An Inquiry concerning the Source of the Heat which is excited by Friction. By Count Rumford," Philosophical Transactions, 1798.

[†] "Vobis (Epicureis) minùs notum est, quemadmodum quidque dicatur. Vestra enim solùm legitis, vestra amatis; cæteros, causâ incognitâ, condemnatis.—CICÉRON, *De natura Deorum*, ii. 29."

If Le Sage had but excepted Kepler's third law, it must be admitted that his case, as stated above, would have been thoroughly established by the arguments of his "mémoire;" for the Epicurean assumption of parallelism adopted to suit the false idea of the earth being flat, prevented the discovery of the law of the inverse square of the distance, which the mathematicians of that day were quite competent to make, if the hypothesis of atoms moving in all directions through space, and rarely coming into collision with one another, had been set before them, with the problem of determining the force with which the impacts would press together two spherical bodies, such as the earth and moon were held to be by some of the contemporary philosophers to whom the Epicureans "would not listen." But nothing less than direct observation, proving Kepler's third law—Galileo's experiment on bodies falling from the tower of Pisa, Boyle's guinea-and-feather experiment, and Newton's experiment of the vibrations of pendulums composed of different kinds of substance —could either give the idea that gravity is proportional to mass, or prove that it is so to a high degree of accuracy for large bodies and small bodies, and for bodies of different kinds of substance. Le Sage sums up his theory in an appendix to the "Lucrèce Newtonien," part of which, translated (literally, except a few sentences which I have paraphrased), is as follows:—

Constitution of Heavy Bodies.

1st. Their indivisible particles are cages—for example, empty cubes or octahedrons vacant of matter except along the twelve edges.

2nd. The diameters of the bars of these cages, supposed increased each by an amount equal to the diameter of one of the gravific corpuscles, are so small relatively to the mutual distance of the parallel bars of each cage, that the terrestrial globe does not intercept even so much as a ten-thousandth part of the corpuscles which offer to traverse it.

3rd. These diameters are all equal; or if they are unequal, their inequalities sensibly compensate one another [in averages].

Constitution of Gravific Corpuscules.

1st. Conformably to the second of the preceding suppositions, their diameters added to that of the bars is so small relatively to the mutual distance of parallel bars of one of the cages, that the weights of the celestial bodies do not differ sensibly from being in proportion to their masses.

2nd. They are isolated; so that their progressive movements are necessarily linear.

3rd. They are so sparsely distributed (that is to say, their diameters are so small relatively to their mean mutual distances) that not more than one out of every hundred of them meets another corpuscule during several thousands of years; so that the uniformity of their movements is scarcely ever troubled sensibly.

4th. They move along several hundred thousand millions of different directions, in counting for one same direction all those which are [within a definite very small angle of being] parallel to one straight line. The distribution of these straight lines is to be conceived by imagining as many points as one wishes to consider of different directions, scattered over a globe as uniformly as possible, and therefore separated from one another by at least a second of angle, and then imagining a radius of the globe drawn to each of those points.

5th. Parallel, then, to each of those directions, let a current or torrent of corpuscules move; but, not to give the stream a greater breadth than is necessary, consider the transverse section of this current to have the same boundary as the orthogonal projection of the visible world on the plane of the section.

6th. The different parts of one such current are sensibly equidense, whether we compare, among one another, collateral portions of sensible transverse dimensions, or successive portions of such lengths that their times of passage across a given surface are sensible. And the same is to be said of the different currents compared with one another.

7th. The mean velocities, defined in the same manner as I have just defined the densities, are also sensibly equal.

8th. The ratios of these velocities to those of the planets are several million times greater than the ratios of the gravities of the planets towards the sun to the greatest resistance which secular observations allow us to suppose they experience. For example, [these velocities must be] some hundredfold a greater number of times the velocity of the earth, than the ratio of 190,000 * times the gravity of the earth towards the sun to the greatest resistance which secular observations of the length of the year permit us to suppose that the earth experiences from the celestial masses.

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* To render the sentence more easily read, I have substituded this number in place of the following words:—"le nombre de fois que le firmament contient le disque apparent du soleil."

CONCEPTION, *which facilitates the application of Mathematics to determine the mutual Influence of these Heavy Bodies, and these Corpuscules.*

1st. Decompose all heavy bodies into molecules of equal mass, so small that they may be treated as attractive points with respect to theories in which gravity is considered without reference to its cause; that is to say, each must be so small that inequalities of distance and differences of direction between its particles and those of another molecule, conceived as attracting it and being attracted by it, may be neglected. For example, suppose the diameter of the molecule considered to be a hundred thousand times smaller than the distance between two bodies of which the mutual gravitation is examined, which would make its apparent semidiameter, as seen from the other body, about one second of angle.

2nd. For the surfaces of such a molecule, accessible but impermeable to the gravific fluid, substitute one single spherical surface equal to their sum.

3rd. Divide those surfaces into facets small enough to allow them to be treated as planes, without sensible error [&c. &c.].

Remarks.

It is not necessary to be very skilful to deduce from these suppositions all the laws of gravity, both sublunary and universal (and consequently also those of Kepler, &c.), with all the accuracy with which observed phenomena have proved those laws. Those laws, therefore, are inevitable consequences of the supposed constitutions.

2nd. Although I here present these constitutions crudely and without proof, as if they were gratuitous hypotheses and hazarded fictions, equitable readers will understand that on my own part I have at least some presumptions in their favour (independent of their perfect agreement with so many phenomena), but that the development of my reasons would be too long to find a place in the present statement, which may be regarded as a publication of theorems without their demonstrations.

3rd. . . . . . There are details upon which I have wished to enter on account of the novelty of the doctrine, and which will readily be supplied by those who study it in a favourable and attentive spirit. If the authors who write on hydrodynamics, aërostatics, or optics had to deal with captious readers, doubting the very existence of water, or air, or light, and therefore not adapting themselves to any tacit supposition regarding equivalencies or compensations not expressly mentioned in their treatises, they would be obliged to load their definitions with a vast number of specifications which instructed or indulgent readers do no not require of them. One understands "*à demi-mot*" and "*sano sensu*" only familiar propositions towards which one is already favourably inclined.

Some of the details referred to in this concluding sentence of the appendix to his *Lucrèce Newtonien,* Le Sage discusses fully in his *Traité de Physique Mécanique,* edited by Pierre Prévost, and published in 1818 (Geneva and Paris).

This treatise is divided into four books.

I. "Exposition sommaire du système des corpuscules ultramondains."

II. "Discussion des objections qui peuvent s'élever contre le système des corpuscules ultramondains."

III. "Des fluids élastiques ou expansifs."

IV. "Application des théories précédentes à certaines affinités."

It is in the first two books that gravity is explained by the impulse of ultramundane corpuscules, and I have no remarks at present to make on the third and fourth books.

From Le Sage's fundamental assumptions, given above as nearly as may be in his own words, it is, as he says himself, easy to deduce the law of the inverse square of the distance, and the law of proportionality of gravity to mass. The object of the present note is not to give an exposition of Le Sage's theory, which is sufficiently set forth in the preceding extracts, and discussed in detail in the first two books of his posthumous treatise. I may merely say that inasmuch as the law of the inverse square of the distance, for every distance, however great, would be a perfectly obvious consequence of the assumptions, were the gravific corpuscules infinitely small, and therefore incapable of coming into collision with one another, it may be extended to as great distances as we please, by giving small enough dimensions to the corpuscules relatively to the mean distance of each from its nearest neighbour. The law of masses may be extended to as great masses as those for which observation proves it (for example, the mass of Jupiter), by making the diameters of the bars of the supposed cage-atoms constituting heavy bodies, small enough. Thus, for example, there is nothing to prevent us from supposing that not more than one straight line of a million drawn at random towards Jupiter and continued through it, should touch one of the bars. Lastly, as Le Sage proves, the resistance of his gravific fluid to the motion of one of the planets through it, is proportional to the product of the velocity of the planet into the average velocity of the gravific corpuscules; and hence, by making the velocities of the corpuscules great enough, and giving them suitably small masses, they may produce the actual forces of gravitation, and not more than the amount of resistance which observation allows us to suppose that the planets experience. It will be a very interesting subject to examine minutely Le Sage's details on these points, and to judge whether or not the additional knowledge gained by observation since his time requires any modification to be made in the estimate which he has given of the possible degrees of permeability of the sun and planets, of the possible proportions of diameters of corpuscules to interstices between them in the "gravific fluid," and of the possible velocities of its component corpuscules. This much is certain, that if hard indivisible atoms are granted at all, his principles are unassailable, and nothing can be said against the probability of his assumptions. The only imperfection of his theory is that which is inherent to every supposition of hard, indivisible atoms. They must be perfectly elastic or imperfectly elastic, or perfectly inelastic. Even Newton seems to have admitted as a probable reality hard, indivisible, unalterable atoms, each perfectly inelastic.

Nicolas Fatio is quoted by Le Sage and Prévost as a friend of Newton, who in 1689 or 1690 had invented a theory of gravity perfectly similar to that of Le Sage, except certain essential points, had described it in a Latin poem not yet printed, and had written, on the 30th March, 1694, a letter regarding it, which is to be found in the third volume of the works of Leibnitz, having been communicated for publication to the editor of those works by Le Sage. Redeker, a German physician, is quoted by Le Sage as having expounded a theory of gravity of the same general character, in a Latin dissertation published in 1736, referring to which Prévost says, "Où l'on trouve l'exposé d'un système fort semblable à celui de Le Sage dans ses traits principaux, mais dépourvu de cette analyse exacte des phénomènes qui fait le principal mérite de toute espèce de théorie." Fatio supposed the corpuscules to be elastic, and seems to have shown no reason why their return velocities after collision with mundane matter should be less than their previous velocities, and therefore not to have explained gravity at all. Redeker, we are told by Prévost, had very limited ideas of the permeabilities of great bodies, and therefore failed to explain the law of the proportionality of gravity to mass: "he enunciated this law very correctly in section 15 of his dissertation; but the manner in which he explains it shows that he had but little reflected upon it. Notwithstanding these imperfections, one cannot but recognize in this work an ingenious conception which ought to have provoked examination on the part of naturalists, of whom many at that time occupied themselves with the same investigation.

Indeed there exists a dissertation by Segner on this subject*. But science took another course, and works of this nature gradually lost appreciation. Le Sage has never failed on any occasion to call attention to the system of Redeker, as also to that of Fatio" †.

Le Sage shows that, to produce gravitation, those of the ultramundane corpuscules which strike the cage-bars of heavy bodies must either stick there or go away with diminished velocities. He supposed the corpuscules to be inelastic (*durs*), and points out that we ought not to suppose them to be permanently lodged in the heavy body (*entassés*), that we must rather suppose them to slip off, but that, being inelastic, their average velocities after collision must be less than that which they had before collision‡.

That these suppositions imply a gradual diminution of gravity from age to age was carefully pointed out by Le Sage, and referred to as an objection to his theory. Thus he says, ". . . Donc, la durée de la gravité seroit *finie* aussi, et par conséquent la durée du monde.

Réponse. Concedo; mais pourvu que cet obstacle ne contribue pas à faire finir le monde plus promptement qu'il n'auroit fini sans lui, il doit être considéré comme nul" §.

Two suppositions may be made on the general basis of Le Sage's doctrine:—

1st (which seems to have been Le Sage's belief). Suppose the whole of mundane matter to be contained within a finite space, and the infinite space round it to be traversed by ultramundane corpuscules, and a small proportion of the corpuscules coming from ultramundane space to suffer collisions with mundane mattter, and get away with diminished gravific energy to ultramundane space again. They would never return to the world were it not for collision among themselves and other corpuscules. Le Sage held:—that such collisions are extremely rare that; each collision, even between the ultramundane corpuscules themselves, destroys some energy ‖; that at a not infinitely remote past time they were set in motion for the purpose of keeping gravitation throughout the world in action for a limited period of time; and that both by their mutual collisions, and by collisions with mundane atoms, the whole stock of gravific energy is being gradually reduced, and therefore the intensity of gravity gradually diminishing from age to age.

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* De Causâ gravitatis Redekerianâ.

† Le Sage was remarkably scrupulous in giving full information regarding all who preceded him in the development of any part of his theory.

‡ Le Sage estimated the velocity after collision to be two thirds of the velocity before collision.

§ Posthumous *Traité de Physique Mécanique*, edited by Pierre Prévost. Geneva and Paris, 1818.

‖ Newton (Optics, Query 30, ed. 1721, p. 373) held that two equal and similar atoms, moving with equal velocities in contrary directions, come to rest when they strike one another. Le Sage held the same; and it seems that writers of last century understood this without qualification when they called atoms hard.

Or, 2nd, suppose mundane matter to be spread through all space, but to be much denser within each of an infinitely great number of finite volumes (such as the volume of the earth) than elsewhere. On this supposition, even were there no collisions between the corpuscules themselves, there would be a gradual diminution in their gravific energy through the repeated collisions with mundane matter which each one must in the course of time suffer. The secular diminution of gravity would be more rapid according to this supposition than according to the former, but still might be made as slow as we please by pushing far enough the fundamental assumptions of very small diameters for the cage-bars of the mundane atoms, very great density for their substance, and very small volume and mass, and very great velocity for the ultramundane corpuscules.

The object of the present note is to remark that (even although we were to admit a gradual fading away of gravity, if slow enough), we are forbidden by the modern physical theory of the conservation of energy to assume inelasticity, or any thing short of perfect elasticity, in the ultimate molecules, whether of ultramundane or of mundane matter—and, at the same time, to point out that the assumption of diminished exit-velocity of ultramundane corpuscules, essential to Le Sage's theory, may be explained for perfectly elastic atoms, consistently both with modern thermodynamics, and with perennial gravity.

If the gravific corpuscules leave the earth or Jupiter with less energy than they had before collision, their effect must be to continually elevate the temperature throughout the whole mass. The energy which must be attributed to the gravific corpuscules is so enormously great, that this elevation of temperature would be sufficient to melt and evaporate any solid, great or small, in a fraction of a second of time. Hence, though outward-bound corpuscules must travel with less velocity, they must carry away the same energy with them as they brought. Suppose, now, the whole energy of the corpuscules approaching a planet to consist of translatory motion: a portion of the energy of each corpuscule which has suffered collision must be supposed to be converted by the collision into vibrations, or vibrations and rotations. To simplify ideas, suppose for a moment the particles to be perfectly smooth elastic globules. Then collision could not generate any rotatory motion; but if the cage-atoms constituting mundane matter be each of them, as we must suppose it to be, of enormously great mass in comparison with one of the ultramundane globules, and if the substance of the latter, though perfectly elastic, be much less rigid than that of the former, each globule that strikes one of the cage-bars must (Thomson and Tait's 'Natural Philosophy,' § 301) come away with diminished velocity of translation, but with the corresponding deficiency of energy altogether converted into vibration of its own mass. Thus the condition required by Le Sage's theory is fulfilled without violating modern thermo-dynamics; and, according to Le Sage, we might be satisfied not to inquire what becomes of those ultramundane corpuscules which have been in collision either with the cage-bars of mundane matter or with one another; for at present, and during ages to come, these would be merely an inconsiderable minority, the great majority being still fresh with original gravific energy unimpaired by collision. Without entering on the purely metaphysical question, Is any such supposition satisfactory? I wish to point out how gravific energy may be naturally restored to corpuscules in which it has been impaired by collision.

Clausius has introduced into the kinetic theory of gases the very important consideration of vibrational and rotational energy. He has shown that a multitude of elastic corpuscules moving through void, and occasionally striking one another, must, on the average, have a constant proportion of their whole energy in the form of vibrations and rotations, the other part being purely translational. Even for the simplest case—that, namely, of smooth elastic globes—no one has yet calculated by abstract dynamics the ultimate average ratio of the vibrational and rotational to the translational energy. But Clausius has shown how to deduce it for the corpuscules of any particular gas from the experimental determination of the ratio of its specific heat, pressure constant, to its specific heat, volume constant*. He found that

$$\beta=\frac{2}{3}\frac{1}{\gamma-1},$$

if γ be the ratio of the specific heats, and β the ratio of the whole energy to the translational part of it. For air, the value of γ found by experiment is 1·408, which makes $\beta=1·634$. For steam, Maxwell says, on the authority of Rankine, β "may be as much as 2·19; but this is very uncertain." If the molecules of gases are admitted to be elastic corpuscules, the validity of Clausius's principle is undeniable; and it is obvious that the value of the ratio β must depend upon the shape of each molecule, and on the distribution of elastic rigidity through it, if its substance is not homogeneous. Further, it is clear that the value of β, for a set of equal and similar corpuscules, will not be the same after collision with molecules different from them in form or in elastic rigidity as after collision with molecules only of their own kind. All that is necessary to complete Le Sage's theory of gravity in accordance with modern science, is to assume that the ratio of the whole energy of the corpuscules to the translational part of their energy is greater, on the average, after collisions with mundane matter than after intercollisions of only ultramundane corpuscules. This supposition is neither more nor less questionable than that of Clausius for gases, which is now admitted as one of the generally recognized truths of science. The corpuscular theory of gravity is no more difficult in allowance of its fundamental assumptions than the kinetic theory of gases as at present received; and it is more complete, inasmuch as, from fundamental assumptions of an extremely simple character, it explains all the known phenomena of its subject, which cannot be said of the kinetic theory of gases so far as it has hitherto advanced.

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* Maxwell's 'Elementary Treatise on Heat,' chap. xxii. Longmans, 1871.

Postscript, April 1872.

In the preceding statement I inadvertently omitted to remark that if the constituent atoms are æolotropic in respect of permeability, crystals would generally have different permeabilities in different directions, and would therefore have different weights according to the direction of their axes relatively to the direction of gravity. No such difference has been discovered; and it is certain that, if there is any, it is extremely small. Hence the constituent atoms, if æolotropic as to permeability, must be so but to an exceedingly small degree. Le Sage's second fundamental assumption, given above under the title "*Constitution of Heavy Bodies,*" implies sensibly equal permeability in all directions, even in an æolotropic structure, unless much greater than Jupiter, provided that the atoms are isotropic as to permeability.

A body having different permeabilities in different directions would, if of manageable dimensions, give us a means for drawing energy from the inexhaustible stores laid up in the ultramundane corpuscules, thus:—First, turn the body into a position of minimum weight; secondly, lift it through any height; thirdly, turn it into a position of maximum weight; fourthly, let it down to its primitive level. It is easily seen that the first and third of those operations are performed without the expenditure of work; and, on the whole, work is done by gravity in operations 2 and 4. In the corresponding set of operations performed upon a movable body in the neighbourhood of a fixed magnet, as much work is required for operations 1 and 3 as is gained in operations 2 and 4—the magnetization of the movable body being either intrinsic or inductive, or partly intrinsic and partly inductive, and the part of its æolotropy (if any) which depends on inductive magnetization being due either to magnecrystallic quality of its substance or to its shape*.

On the Motion of Rigid Solids in a Liquid circulating irrotationally through perforations in them or in a Fixed Solid†.

1. Let $\psi, \phi, \ldots$ be the values at time t, of generalized coordinates fully specifying the positions of any number of solids movable through space occupied by a perfect liquid destitute of rotational motion, and not acted on by any force which could produce it. Some or all of these solids being perforated, let χ, χ', χ'', &c. be the quantities of liquid which from any era of reckoning, up to the time t, have traversed the several apertures. According to an extension of Lagrange's general equations of motion, used in vol. i. of Thomson and Tait's 'Natural Philosophy,' §§ 331–336, proved in §§ 329, 331 of the German translation of that volume, and to be further developed in the second English edition now in the press, we may use these quantities $\chi, \chi', \ldots$ as if they were coordinates so far as concerns the equations of motion. Thus, although the position of any part of the fluid is not only not explicitly specified, but is actually indeterminate, when $\psi, \phi, \ldots \chi, \chi', \ldots$ are all given, we may regard $\chi, \chi' \ldots$ as specifying all that it is necessary for us to take into account regarding the motion of the liquid, in forming the equations of motion of the solids; so that if $\xi, \eta, \ldots$, and $\Psi, \Phi \ldots$ denote the generalized components of momentum and of force [Thomson and Tait, § 313 (*a*) (*b*)] relatively to $\psi, \phi \ldots$, and if $\kappa, \kappa', \ldots$ K, K$' \ldots$ denote corresponding elements relatively to $\chi, \chi', \ldots$, we have (Hamiltonian form of Lagrange's general equations)

$$\left.\begin{array}{ll} \dfrac{d\xi}{dt}+\dfrac{\mathfrak{d}T}{d\psi}=\Psi, & \dfrac{d\eta}{dt}+\dfrac{\mathfrak{d}T}{d\phi}=\Phi,\ldots \\ \dfrac{d\kappa}{dt}+\dfrac{\mathfrak{d}T}{d\chi}=K, & \dfrac{d\kappa'}{dt}+\dfrac{\mathfrak{d}T}{d\chi'}=K',\ldots \end{array}\right\} \quad . \quad . \quad . \quad (1)$$

where T denotes the whole kinetic energy of the system, and $\mathfrak{d}$ differentiation on the hypothesis of $\xi, \eta, \ldots \kappa, \kappa' \ldots$ constant.

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* "Theory of Magnetic Induction in crystalline and non-crystalline substances," Phil. Mag., March 1851; "Forces experienced by inductively magnetized ferro-magnetic and diamagnetic non-crystalline substances," Phil. Mag., Oct. 1850; "Reciprocal action of diamagnetic particles," Phil. Mag., Dec. 1855; all to be found in a collection of reprinted and newly written papers on electrostatics and magnetism, nearly ready for publication (Macmillan, 1872).

† The title and first part (§§ 1–13) are new; the remainder (§§ 14, 15) was communicated to the Royal Society at the end of last December.—W. T., September 26, 1872.

2. To illustrate the meaning of χ, K, κ, $\dot{\chi}$, . . ., let B be one of the perforated solids, to be regarded generally as movable, draw an immaterial barrier surface Ω across the aperture to which they are related, and consider this barrier fixed relatively to B. Let N denote the normal component velocity, relatively to B and Ω of the fluid at any point of Ω; and let $\iint d\sigma$ denote integration over the whole area of Ω; then

$$\iint \mathrm{N}d\sigma=\dot{\chi}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and

$$\chi-\int dt\iint \mathrm{N}d\sigma, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which is a symbolical expression of the definition of χ. To the surface of liquid coinciding with Ω at any instant, let pressure be applied of constant value K per unit of area, over the whole area; and at the same time let force (or force and couple) be applied to B equal and opposite to the resultant of this pressure supposed for a moment to act on a rigid material surface Ω rigidly connected with B. The "motive" (that is to say, system of forces) consisting of the pressure K on the fluid surface, and force and couple B as just defined, constitutes the generalized component force corresponding to χ [Thomson and Tait, § 313 (*b*)]; for it does no work upon any motion of B or other bodies of the system if χ is kept constant; and if χ varies, work is done at the rate

$$\mathrm{K}\dot{\chi} \text{ per unit of time,}$$

whatever other motions or forces there may be in the system. Lastly, calling the density of the fluid unity, let κ denote "circulation"* [V. M. § 60 (*a*)]† of the fluid in any circuit crossing β once, and only once; it is this which constitutes the generalized component momentum relatively to χ [Thomson and Tait, § 313 (*e*)]; for (V. M. § 72) we have

$$\kappa=\int_0^t \mathrm{K}dt, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

if the system given at rest (or in any state of motion for which $\kappa=0$) be acted on by the motive K during time t‡.

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* Or $\int \mathrm{F}ds$ if F denote the tangential component of the absolute velocity of the fluid at any point of the circuit, and $\int ds$ line integration once round the circuit.

† References distinguished by the initials V. M. are to the part already published of the author's paper on Vortex Motion (Transactions of the Royal Society of Edinburgh, 1867–8 and 1868–9).

‡ The general limitation, for impulsive action, that the displacements effected during it are infinitely small, is not necessary in this case. Compare § 5 (11) below.

3. The kinetic energy T is, of course, necessarily a quadratic function of the generalized momentum-components $\xi, \eta, \ldots \kappa, \kappa' \ldots$; with coefficients generally functions of ψ, ϕ, ..., but necessarily independent of χ, χ', ... In consequence of this peculiarity it is convenient to put

$$T = Q(\xi - \alpha\kappa - \alpha'\kappa' - \&c., \eta - \beta\kappa - \beta'\kappa' - \&c., \ldots) + \mathfrak{Q}(\kappa, \kappa', \ldots), \quad (5)$$

where Q, $\mathfrak{Q}$ denote two quadratic functions. This we may clearly do, because, if i be the number of the variables ξ, η, ..., and j the number of κ, κ', ..., the whole number of coefficients in the single quadratic function expressing τ is $\frac{(i+j)(i+j+1)}{2}$, which is equal to the whole number of the coefficients $\frac{i(i+1)}{2} + \frac{j(j+1)}{2}$ of the two quadratic functions, together with the i, j available quantities $\alpha, \beta, \ldots \alpha', \beta', \ldots, \ldots$

4. The meaning of the quantities $\alpha, \beta, \ldots \alpha', \ldots$ thus introduced is evident when we remember that

$$\frac{dT}{d\xi} = \dot{\psi}, \quad \frac{dT}{d\eta} = \dot{\phi}, \ldots \quad \frac{dT}{d\kappa} = \dot{\chi}, \quad \frac{dT}{d\kappa'} = \dot{\chi}', \ldots \quad (6)$$

For, differentiating (5) and using these, we find

$$\dot{\psi} = \frac{dQ}{d\xi}, \quad \dot{\phi} = \frac{dQ}{d\eta}; \quad \ldots \ldots \quad (7)$$

and using these latter,

$$\dot{\chi} = \frac{d\mathfrak{Q}}{d\kappa} - \alpha\dot{\psi} - \beta\dot{\phi} - \&c., \quad \dot{\chi}' = \frac{d\mathfrak{Q}}{d\kappa'} - \alpha'\dot{\psi} - \beta'\dot{\phi} - \&c., \ldots \quad (8)$$

Equations (8) show that $-\alpha\dot{\psi}$, $-\beta\dot{\phi}$, $-\alpha'\dot{\psi}$, &c. are the contributions to the flux across Ω, Ω', &c. given by the separate velocity-components of the solids. And (7) show that to prevent the solids from being set in motion when impulses $\kappa, \kappa', \ldots$ are applied to the liquid at the barrier surfaces, we must apply to them impulses expressed by the equations

$$\xi = \alpha\kappa + \alpha'\kappa' + \&c., \quad \eta = \beta\kappa + \beta'\kappa' + \&c. \ldots \ldots \quad (9)$$

5. To form the equations of motion, we have in the first place

$$\frac{\mathfrak{d}T}{d\chi} = 0, \quad \frac{\mathfrak{d}T}{d\chi'} = 0, \ldots, \quad \ldots \quad (10)$$

and therefore, by (1),

$$\frac{d\kappa}{dt} = K, \quad \frac{d\kappa'}{dt} = K', \ldots, \quad \ldots \quad (11)$$

which show that the acceleration of κ, under the influence of K, follows simply the law of acceleration of a mass under the influence of a force. Again (for the motions of the solids), let

$$\xi_0=\xi-\alpha\kappa-\alpha'\kappa'-\&c.,\quad \eta_0=\eta-\beta\kappa-\beta'\kappa'-\&c.,\ldots; \qquad (12)$$

and let $\frac{\mathfrak{D}Q}{d\psi}$, &c. denote variations of Q on the hypothesis of ξ_0, η_0, ... each constant.

We have from (5), remembering that $\frac{\mathfrak{d}T}{d\psi}$ &c. denote variations of T, on the hypothesis of $\xi, \eta, \ldots \kappa, \kappa', \ldots$ constant,

$$\left(\frac{\mathfrak{d}T}{d\psi}=\frac{\mathfrak{D}Q}{d\psi}-\frac{dQ}{d\xi}-d\kappa\frac{d\alpha}{d\psi}+\kappa'\frac{d\alpha'}{d\psi}+\ldots\right)$$
$$-\frac{dQ}{\eta}\left(\kappa\frac{d\beta}{d\psi}+\kappa'\frac{d\beta'}{d\psi}+\&c.\right)-\&c.+\frac{\mathfrak{d}\mathfrak{Q}}{d\psi},$$

or, by (7),

$$\frac{\mathfrak{d}T}{d\psi}=\frac{\mathfrak{D}Q}{d\psi}-\dot{\psi}\left(\kappa\frac{d\alpha}{d\psi}+\kappa'\frac{d\alpha}{d\psi}+\&c.\right)$$
$$-\dot{\phi}\left(\kappa\frac{d\beta}{d\psi}+\kappa'\frac{d\beta'}{d\psi}+\&c.\right)-\&c.+\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}\ldots \qquad (13)$$

Hence, by (1),

$$\frac{d\xi}{dt}+\frac{\mathfrak{D}Q}{d\psi}-\dot{\psi}\left(\kappa\frac{d\alpha}{d\psi}+\kappa'\frac{d\alpha'}{d\psi}+\&c.\right)$$
$$-\dot{\phi}\left(\kappa\frac{d\beta}{d\psi}+\kappa'\frac{d\beta'}{d\psi'}+\&c.\right)-\&c.+\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}=\Psi\ldots \qquad (14)$$

Now remark that, according to the notation of (12), ξ_0, η_0, ... are the momentum-components of the solids due to their own motion alone without cyclic motion of the liquid; and therefore eliminate $\xi, \eta, \ldots$ by (12) from (14). Thus we find

$$\frac{d\xi_0}{dt}+\frac{\mathfrak{D}Q}{d\psi}+\alpha\frac{d\kappa}{dt}+\alpha'\frac{d\kappa'}{dt}+\&c.$$
$$+\dot{\phi}\left\{\kappa\left(\frac{d\alpha}{d\phi}-\frac{d\beta}{d\psi}\right)+\kappa'\left(\frac{d\alpha'}{d\phi}-\frac{d\beta'}{d\psi}\right)+\&c.\right\}$$
$$+\dot{\theta}\left\{\kappa\left(\frac{d\alpha}{d\theta}-\frac{d\gamma}{d\psi}\right)+\kappa'\left(\frac{d\alpha'}{d\phi}-\frac{d\gamma'}{d\psi}\right)+\&c.\right\}$$
$$+\quad\&c. \qquad =\Psi-\frac{\mathfrak{d}\mathfrak{Q}}{d\psi},\ldots \qquad (15)$$

which, with the corresponding equation for ξ_0 &c., and with (11) for κ, κ', &c., are the desired equations of motion.

6. The hypothetical mode of application of K, K′, ... (§ 1) is impossible; and every other (such as the influence of gravity on a real liquid at different temperatures in different parts) is impossible for our ideal "liquid"—that is to say, a homogeneous incompressible perfect fluid. Hence we have $K=0$, $K'=0$, and from (11) conclude that $\kappa, \kappa', \dots$ are constants. [They are sometimes called the "cyclic constants" (V. M. §§ 62–64).] The equation of motion (15) thus becomes simply

$$\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\phi} + \dot\phi\left\{\kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\phi} - \frac{d\beta'}{d\psi}\right) + \dots\right\}$$
$$+\dot\theta\left\{\kappa\left(\frac{d\alpha}{d\theta} - \frac{d\gamma}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\theta} - \frac{d\beta'}{d\psi}\right) + \dots\right\}$$
$$+ \quad \&c. \qquad = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \dots \quad (16)$$

with corresponding equations for η_0, ζ_0, and with the following relations from (7) between $\xi_0, \eta_0, \dots$ and $\dot\psi, \dot\phi, \dots$

$$\frac{dQ}{d\xi_0} = \dot\psi, \quad \frac{dQ}{d\eta_0} = \dot\phi, \quad \frac{dQ}{d\zeta_0} = \dot\theta, \ \&c. \quad . \quad . \quad . \quad (17)$$

7. Let

$$\kappa\left(\frac{d\alpha}{d\phi} - \frac{d\beta}{d\psi}\right) + \kappa'\left(\frac{d\alpha'}{d\psi} - \frac{d\beta'}{d\psi}\right) + \&c. \text{ be denoted by } \{\phi, \psi\}, \quad (18)$$

so that we have

$$\{\phi, \psi\} = -\{\psi, \phi\}. \quad . \quad . \quad . \quad . \quad (19)$$

These quantities, $\{\phi, \psi\}$, $\{\theta, \psi\}$, &c., linear functions of the cyclic constants, with coefficients depending on the configuration of the system, are to be generally regarded simply as given functions of the coordinates $\psi, \phi, \theta, \dots$; and the equations of motion are

$$\left.\begin{aligned} &\frac{d\xi_0}{dt} + \frac{\mathfrak{D}Q}{d\psi} + \{\phi, \psi\}\dot\phi + \{\theta, \psi\}\dot\theta + \&c. = \Psi - \frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \\ &\frac{d\eta_0}{dt} + \frac{Q}{d\psi} - \{\phi, \psi\}\dot\psi + \{\theta, \phi\}\dot\theta + \&c. = \Theta - \frac{\mathfrak{d}\mathfrak{Q}}{d\theta}. \\ &\quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \end{aligned}\right\} . \quad (20)$$

In these (being of the Hamiltonian form) Q is regarded as a quadratic function of $\xi_0, \eta_0, \zeta_0 \dots$ with its coefficients functions of ψ, ϕ, θ, &c.; and $\mathfrak{D}$ applied to it indicates variations of these coefficients. If now we eliminate $\xi_0, \eta_0, \zeta_0, \dots$ from Q by the linear equations, of which (17) is an abbreviated expression, and so have Q expressed as a quadratic function of $\dot{\psi}$, $\dot{\phi}$, $\dot{\theta}, \ldots$, with its coefficients functions of ψ, ϕ, θ, &c.; and if we denote by $\frac{dQ}{d\phi}$, $\frac{dQ}{d\psi}$, &c. variations of Q depending on variations of these coefficients, and by $\frac{dQ}{d\dot{\psi}}$, $\frac{dQ}{d\dot{\phi}}$, &c. variations of Q depending on variations of $\dot{\psi}$, $\dot{\phi}$, &c., we have [compare Thomson and Tait, § 329 (13) and (15)]

$$\left.\begin{aligned} &\xi_0=\frac{dQ}{d\dot{\psi}}, \quad \eta_0=\frac{dQ}{d\dot{\psi}}, \ldots \\ \text{and} \quad &\frac{\mathfrak{D}Q}{d\psi}=-\frac{dQ}{d\psi}, \quad \frac{\mathfrak{D}Q}{d\phi}=-\frac{dQ}{d\phi}, \ldots \end{aligned}\right\} \quad . \quad . \quad (21)$$

and the equations of motion become

$$\left.\begin{aligned} &\frac{d}{dt}\frac{dQ}{d\dot{\psi}}-\frac{dQ}{d\psi}+\{\phi,\psi\}\dot{\phi}+\{\theta,\psi\}\dot{\theta}+\ldots=\Psi-\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \\ &\frac{d}{dt}\frac{dQ}{d\dot{\phi}}-\frac{dQ}{d\phi}-\{\phi,\psi\}\dot{\psi}+\{\theta,\phi\}\dot{\theta}+\ldots=\Phi-\frac{\mathfrak{d}\mathfrak{Q}}{d\phi}, \\ &\frac{d}{dt}\frac{dQ}{d\dot{\theta}}-\frac{dQ}{d\theta}-\{\theta,\psi\}\dot{\psi}-\{\theta,\phi\}\dot{\phi}+\ldots=\Theta-\frac{\mathfrak{d}\mathfrak{Q}}{d\theta}. \\ &\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \end{aligned}\right\} . \, (22)$$

The first members here are of Lagrange's form, with the remarkable addition of the terms involving the velocities simply (in multiplication with the cyclic constants) depending on the cyclic fluid motion. The last terms of the second members contain traces of their Hamiltonian origin in the symbols $\frac{\mathfrak{d}}{d\psi}$, $\frac{\mathfrak{d}}{d\phi}, \ldots$

8. As a first application of these equations, let $\dot{\psi}=0$, $\dot{\phi}=0$, $\dot{\theta}=0, \ldots$. This makes $\xi_0=0$, $\eta_0=0, \ldots$, and therefore also $Q=0$; and the equations of motion (16) (now equations of equilibrium of the solids under the influence of applied forces Ψ, Φ, &c. balancing the fluid pressure due to the polycyclic motion $\kappa, \kappa', \ldots$) become

$$\Psi=\frac{\mathfrak{d}\mathfrak{Q}}{d\psi}, \quad \Phi=\frac{\mathfrak{d}\mathfrak{Q}}{d\phi}, \text{ \&c.}, \quad . \quad . \quad . \quad . \quad (23)$$

a result which a direct application of the principle of energy renders obvious (the augmentation of the whole energy produced by an infinitesimal displacement, $\delta\psi$, is $\frac{d\mathfrak{Q}}{d\psi}\delta\psi$; and $\Psi\delta\psi$ is the work done by the applied forces). It is proved in §§ 724–730 of a volume of collected papers on electricity and magnetism soon to be published, that $\frac{d\mathfrak{Q}}{d\psi}$, $\frac{d\mathfrak{Q}}{d\phi}$, &c. are the components of the forces experienced by bodies of perfect diamagnetic inductive capacity placed in the magnetic field analogous* to the supposed cyclic irrotational motion. Hence the motive influence of the cyclic motion of the liquid upon the solids in equilibrium is equal and opposite to that of magnetism in the magnetic analogue.

This is proposition II. of the paper "On the Forces experienced by Solids immersed in a Moving Liquid," which relates to the forces required to keep the movable solids at rest. The present investigation shows Prop. II. of that article to be false. Compare 'Reprint,' § 740.

9. Equations (16) for the case of a single perforated movable solid undisturbed by others, agree substantially with equations (6) and (14) of my communication† to the Royal Society of Edinburgh of February 1871. The ξ_0, η_0, ... of the present article correspond to the $\frac{dT}{du}$, $\frac{dT}{dv}$, &c. of the former; the ξ, η, ... mean the same in both. The equations now demonstrated constitute an extension of the theory not readily discovered or proved by that simple consideration of the principle of momentum, and moment of momentum, on which alone was founded the investigation of my former article.

10. Going back to the analytical definition of $\mathfrak{Q}$ in § 3 (5), we see that, when none of the movable solids is perforated, this configurational function is equal to the whole kinetic energy (E) which the polycyclic motion would have were there no movable solid, diminished by the energy (W) which would be given up were the liquid, which on this supposition flows through the space of the movable solid or solids, suddenly rigidified and brought to rest. Putting then

$$\mathfrak{Q} = E - W, \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and remarking that E is independent of the coordinates of the movable solids, we may put $-W$ in place of $\mathfrak{Q}$ in the equations of motion, which for this slight modification need not be written out again. W might be directly defined as the whole quantity of work required to remove the movable solids, each to an infinite distance from any other solid having a perforation with circulation through it; and, with this definition, $-W$ may be put for $\mathfrak{Q}$ in the equations of motion without exclusion of cases in which there is circulation through apertures in movable solids.

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* Proposition I. of article "On the Forces experienced by Solids immersed in a Moving Liquid," Proc. Roy. Soc. Edinb. February 1870, reprinted in Volume of Electric and Magnetic Papers, §§ 733–740.

† See Proc. Roy. Soc. Edinb. Session 1870–71, or reprint in Philosophical Magazine, November 1871.

11. I conclude with a very simple case, the subject of my communication to the Royal Society of last December, in which the result was given without proof. Let there be only one moving body, and it spherical; let the perforated solid or solids be reduced to an infinitely fine immovable rigid curve or group of curves (endless, of course—that is, either finite and closed or infinite), and let there be no other fixed solid. The rigid curve or curves will be called the "core" or "cores," as their part is simply that of core for the cyclic or polycyclic motion. In this case it is convenient to take for ψ, ϕ, θ the rectangular coordinates (x, y, z) of the centre of the movable globe. Then, because the cores, being infinitely fine, offer no obstruction to the motion of the liquid, making way for the globe moving through it, we have

$$Q=\tfrac{1}{2}m(\dot{x}^2+\dot{y}^2+\dot{z}^2, \quad . \quad . \quad . \quad . \quad . \quad (25)$$

where m denotes the mass of the globe, together with half that of its bulk of the fluid. Hence

$$\left.\begin{array}{l} \dfrac{dQ}{dx}=0, \quad \dfrac{dQ}{dy}=0, \quad \dfrac{dQ}{dz}=0, \\ \text{and} \quad \xi_0\left(=\dfrac{dQ}{d\dot{x}}\right)=m\dot{x}, \quad \eta_0=m\dot{y}, \quad \zeta_0=m\dot{z}. \end{array}\right\} \quad . \quad (26)$$

A further great simplification occurs, because in the present case $\alpha d\psi+\beta d\phi+\ldots$, or, as we now have it, $\alpha dx+\beta dy+\gamma dz$ is a complete differential*. To prove this, let V be the velocity-potential at any point (a, b, c) due to the motion of the globe, irrespectively of any cyclic motion of the liquid. We have

$$V=\frac{1}{2}r^3\left(\dot{x}\frac{d}{dx}+\dot{y}\frac{d}{dy}+\dot{z}\frac{d}{dz}\right)\frac{1}{D},$$

where r denotes the radius of the globe, and

$$D=\{(x-a)^2+(y-b)^2+(z-c)^2\}^{\frac{1}{2}}.$$

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[*] Which means that if the globe, after any motion whatever, great or small, comes again to a position in which it has been before, the integral quantity of liquid which this motion has caused to cross any fixed area is zero.

Hence, if N denote the component velocity of the liquid at (a, b, c) in any direction λ, μ, ν, we have

$$N=\left(\dot{x}\frac{d}{dx}+\dot{y}\frac{d}{dy}+z\frac{d}{dz}\right)F(x, y, z, a, b, c), \quad . \quad . \quad (27)$$

where

$$F(x, y, z, a, b, c)=\frac{1}{2}r^3\left(\lambda\frac{d}{da}+\mu\frac{d}{db}+\nu\frac{d}{dc}\right)\frac{1}{D}.$$

Let now (a, b, c) be any point of the barrier surface Ω (§ 2), and λ, μ, ν the direction-cosines of the normal. By (2) of § 2 we see that the part of $\dot{\chi}$ due to the motion of the globe is $\iint Nd\sigma$, or, by (26),

$$\left(\dot{x}\frac{d}{dx}+\dot{y}\frac{d}{dy}+\dot{z}\frac{d}{dz}\right)\iint F(x, y, z, a, b, c)d\sigma. \quad . \quad (28)$$

Hence, putting

$$\iint F(x, y, z, a, b, c)d\sigma=U,$$

we see by (8) of § 4 that

$$\alpha=-\frac{dU}{dx}, \quad \beta=-\frac{dU}{dy}, \quad \gamma=-\frac{dU}{dz}. \quad . \quad . \quad (29)$$

Hence with the notation of § 7 (18) for $x, y, \ldots$ instead of $\psi, \phi, \ldots$

$$\{y, z\}=0, \quad \{z, x\}=0, \quad \{x, y\}=0.$$

By this and (25) the equations of motion (22) with (24) become simply

$$m\frac{d^2x}{dt^2}=X+\frac{\mathfrak{d}W}{dx}, \quad m\frac{d^2y}{dt^2}=Y+\frac{\mathfrak{d}W}{dy}, \quad m\frac{d^2z}{dt^2}=Z+\frac{\mathfrak{d}W}{dz}. \quad (30)$$

These equations express that the globe moves as a material particle of mass m, with the forces (X, Y, Z) expressly applied to it, would move in a "field of force," having W for potential.

12. The value of W is, of course, easily found by aid of spherical harmonics from the velocity-potential, P, of the polycyclic motion which would exist were the globe removed, and which we must suppose known; and in working it out (see next paragraph) it is readily seen that if, for the hypothetical undisturbed motion q denote the fluid velocity at the point really occupied by the centre of the rigid globe, we have

$$W=\tfrac{1}{2}\mu q^2+w, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (31)$$

where μ denotes once and a half the volume of the globe, and w denotes the kinetic energy of what we may call the internal motion of the liquid occupying for an instant in the undisturbed motion the space of the rigid globe in the real system. To define w, remark that the harmonic analysis proves the velocity of the centre of inertia of an irrotationally moving liquid globe to be equal to q, the velocity of the liquid at its centre*; and consider the velocity of any part of the liquid sphere, relatively to a rigid body moving with the velocity q. The kinetic energy of this relative motion is what is denoted by w. Remark also that if, by mutual forces between its parts, the liquid globe were suddenly rigidified, the velocity of the whole would be equal to q; and that $\frac{1}{2}mq^2$ is the work given up by the rigidified globe and surrounding liquid when the globe is suddenly brought to rest, being the same as the work required to start the globe with velocity q from rest in a motionless liquid.

Let $P+\psi$ be the velocity-potential at (x, y, z) in the actual motion of the liquid when the rigid globe is fixed. Let a be the radius of the globe, r distance of (x, y, z) from its centre, and $\iint d\sigma$ integration over its surface. At any point of the surface of the instantaneous liquid globe the component velocity perpendicular to the spherical surface in the undisturbed motion is $\left(\frac{dP}{dr}\right)_{r=a}$; and hence the impulsive pressure on the spherical surface required to change the velocity-potential of the external liquid from P to $P+\psi$, being $-\psi$, undergoes an amount of work equal to

$$\iint d\sigma\psi \cdot \frac{1}{2}\frac{dP}{dr}$$

in reducing the normal component from that value to zero. On the other hand, the internal velocity-potential is reduced from P to zero; and the work undone in this process is

$$\iint d\sigma P \cdot \frac{1}{2}\frac{dP}{dr}.$$

Hence

$$W=\frac{1}{2}\iint d\sigma(P+\psi)\frac{dP}{dr}. \quad . \quad . \quad . \quad . \quad (32)$$

The condition that with velocity-potential $P+\psi$ there is no flow perpendicular to the spherical surface gives

$$\left(\frac{dP}{dr}+\frac{d\psi}{dr}\right)_{r=a}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

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* This follows immediately from the proposition (Thomson and Tait's 'Natural Philosophy,' § 496) that any function V, satisfying Laplace's equation $\frac{d^2V}{dx^2}+\frac{d^2V}{dy^2}+\frac{d^2V}{dz^2}$ throughout a spherical space, has for its mean value through this space its value at the centre; for $\frac{dP}{dx}$ satisfies Laplace's equation.

Now let

$$\left.\begin{aligned} P&=P_0+P_1\frac{r}{a}+\ldots+P\left(\frac{r}{a}\right)^i+\&c.\\ \psi&=\Psi_1\left(\frac{a}{r}\right)^2+\ldots+\Psi_i\left(\frac{a}{r}\right)^{i+1}+\&c.\end{aligned}\right\}\quad .\ .\quad (34)$$

be the spherical harmonic developments of P and ψ relatively to the centre of the rigid globe as origin—the former necessarily convergent throughout the largest spherical space which can be described from this point as centre without enclosing any part of the core, the latter necessarily convergent throughout space external to the sphere. By (33) we have

$$\Psi_i=\frac{i}{i+1}P_i.\quad .\ .\ .\ .\ .\ .\ .\ .\quad (35)$$

Hence (32) gives

$$W=\frac{1}{2}\iint d\sigma\left(\Sigma\frac{2i+1}{i+1}P_i\right)(\Sigma iP_i),$$

which, by

$$\iint d\sigma P_iP_{i'}=0,$$

becomes

$$W=\frac{1}{2a}\,\Sigma\,\frac{i(2i+1)}{i+1}\iint d\sigma P_i^2.\quad .\ .\ .\quad (36)$$

Now, remarking that a solid spherical harmonic of the first degree may be any linear function of x, y, z, put

$$P_1\frac{r}{a}=Ax+By+Cz,\quad .\ .\ .\ .\ .\ .\quad (37)$$

which gives

$$q^2=A^2+B^2+C^2,$$

and

$$\frac{1}{a}\iint d\sigma P_1^2=(A^2+B^2+C^2)\,.\,\frac{a}{3}\,.\iint d\sigma=q^2\times\text{vol. of globe}=\frac{2}{3}\mu q^2.$$

Hence, by (36),

$$W=\frac{1}{2}\mu q^2+\frac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}P_2^2+\frac{3\,.\,7}{4}P_3^2+\ldots\right);\quad .\quad (38)$$

and therefore, by comparison with (31),

$$w=\frac{1}{2}\iint d\sigma\left(\frac{2\,.\,5}{3}P_2^2+\frac{3\,.\,7}{4}P_3^2+\ldots\right).\quad .\ .\ .\quad (39)$$

13. When the radius of the globe is infinitely small,

$$W=\tfrac{1}{2}\mu q^2,\quad .\ .\ .\ .\ .\ .\ .\ .\quad (40)$$

where μ denotes once and a half the volume of the globule, and q the undisturbed velocity of the fluid in its neighbourhood, This corresponds to the formula which I gave twenty-five years ago for the force experienced by a small sphere (whether of ferromagnetic or diamagnetic non-crystalline substance) in virtue of the inductive influence which it experiences in a magnetic field*.

14. By taking an infinite straight line for the core a simple but very important example is afforded. In this case the undisturbed motion of the fluid is in circles having their centres in the core (or axis as we may now call it) and their planes perpendicular to it. As is well known, the velocity of irrotational revolution round a straight axis is inversely proportional to distance from the axis. Hence the potential function W for the force experienced by an infinitesimal solid sphere in the fluid is inversely as the square of the distance of its centre from the axis; and therefore the force is inversely as the cube of the distance, and is towards the nearest point of the axis. Hence, when the globule moves in a plane perpendicular to the axis, it describes one or other of the forms of Cotesian spirals†. If it be projected obliquely to the axis, the component velocity parallel to the axis will remain constant, and the other component will be unaffected by that one; so that the projection of the globule on the plane perpendicular to the axis will always describe the same Cotesian spiral as would be described were there no motion parallel to the axis. If the globule be left to itself in any position, it will commence moving towards the axis as if attracted by a force varying inversely as the cube of the distance. It is remarkable that it traverses at right angles an increasing liquid current without any applied force to prevent it from being (as we might erroneously at first sight expect it to be) carried sideways with the augmented stream. A properly trained dynamical intelligence would at once perceive that the constancy of moment of momentum round the axis requires the globule to move directly towards it.

15. Suppose now the globule to be of the same density as the liquid. If (being infinitely small) it is projected in the direction and with the velocity of the liquid's motion, it will move round the axis in the same circle with the liquid; but this motion would be unstable [and the neglected term w (39) adds to the

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* "On the Forces experienced by small Spheres under Magnetic Influence, and some of the Phenomena presented by Diamagnetic Substances," Cambridge and Dublin Mathematical Journal, May 1847; and "Remarks on the Forces experienced by Inductively Magnetized Ferromagnetic or Diamagnetic Non-crystalline Substances," Phil. Mag. October 1850. Reprint of papers on Electrostatics and Magnetism, §§ 634–668. Macmillan, 1872.

† Tait and Steele's 'Dynamics of a Particle,' § 149 (15).

instability]. Compare Tait and Steele's 'Dynamics of a Particle,' § 149 (15), Species IV., case A=0 and AB finite; also limiting variety between Species I. and Species V. The globule will describe the same circle in the opposite direction if projected with the same velocity opposite to that of the fluid. If the globule be projected either in the direction of the liquid's motion or opposite to it with a velocity less than that of the liquid, it will move along the Cotesian spiral (Species I. of Tait and Steele) from apse to centre in a finite time with an infinite number of turns. If it be projected in either of those directions with a velocity greater by v than that of the liquid, it will move along the Cotesian spiral (Species V. of Tait and Steele) from apse to asymptote. Its velocity along the asymptote at an infinite distance from the axis will be

$$\sqrt{v\left(v+\frac{\kappa}{\pi a}\right)},$$

and the distance of the asymptote from the axis will be

$$a\frac{v+\dfrac{\kappa}{2\pi a}}{\sqrt{v\left(v+\dfrac{\kappa}{\pi a}\right)}},$$

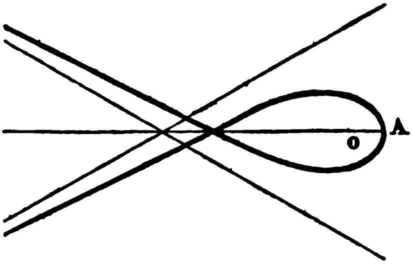
where a denotes the distance of the apse from the axis, and $\frac{\kappa}{2\pi a}$ the velocity of the liquid at that distance from the axis. If the globule be projected from any point in the direction of any straight line whose shortest distance from the axis is p, it will be drawn into the vortex or escape from it, according as the component velocity in the plane perpendicular to the axis is less or greater than $\frac{\kappa}{2\pi p}$. It is to be remarked that, in every case in which the globule is drawn in to the axis (except the extreme one in which its velocity is infinitely little less than that of the fluid, and its spiral path infinitely nearly perpendicular to the radius vector), the spiral by which it approaches, although it has always an infinite number of convolutions, is of finite length, and therefore, of course, the time taken to reach the axis is finite. Considering, for simplicity, motion in a plane perpendicular to the axis, at any point infinitely distant from the axis let the globule be projected with a velocity v along a line passing at distance p on either side of the axis. Then if τ denote the velocity of the fluid at distance unity from the axis [which is equal to $\frac{\kappa}{2\pi}$], and if we put

$$n^2=1-\frac{\tau^2}{v^2p^2}, \quad . \quad . \quad . \quad . \quad . \quad (41)$$

the polar equation of the path is

$$r=\frac{np}{\cos n\theta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (42)$$

Hence the nearest approach to the axis attained by the globule is np, and the whole change of direction which it experiences is $\pi\left(\frac{1}{n}-1\right)$. The case of $\frac{1}{n}=2{\cdot}3$ is represented in the annexed diagram, copied from Tait and Steele's book [§ 149 (15), Species V.].



XLII. *On the Absorption of the Chemically Active Rays in the Sun's Atmosphere.* By H. C. VOGEL*.

It has been observed by Bouguer that the light emitted by the sun is less intense near the edges of the solar disk than in the middle, in consequence of the absorption in the atmosphere surrounding the sun. Photometric measurements showed that the intensity of the light emitted by a point situated in the centre of the solar disk is to that emitted by a point situated at a distance of three fourths of the solar radius from the centre as 48 is to 35. Observations to this effect have been made lately by Secchi† and Liais‡. Secchi has extended these observations to the heat-rays, the absorption of which he tried to determine.

It might be of interest to extend the observations to the chemically active and more refrangible rays. Although every photographic image of the sun shows a very strong decrease of light towards the border of the sun's disk, the absorbing-power of the sun's atmosphere for chemically active rays has never been determined. I have endeavoured to solve this question, and take the liberty to communicate now my observations.

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* Translated by Arthur Schuster, Ph.D., from Poggendorff's *Annalen*, 1873, No. 1.

† *Le Soleil*, p. 121.

‡ "Sur l'intensité relative de la Lumière dans les divers points du disque du Soleil," *Mém. Cherbourg*, vol. xii. (1866) pp. 277–342.

Herschel*, Claudet†, and others have already tried to determine the intensity of light by photographic colouring; but the experiments did not succeed in consequence of the difficulty of preparing a photographic layer of the same sensitiveness throughout, and of discovering a law connecting the time of exposure, intensity of light, and the shade of tint of the photographic paper. The photochemical researches of Bunsen and Roscoe‡ have even shown that no proportionality exists between the intensity of the light and the tint of the photographic paper after a certain time of insolation. Their observations, which were made with great care, showed *"that equal products of the intensity of the light into the times of insolation correspond within very wide limits to equal shades of tints produced on chloride-of-silver paper of uniform sensitiveness."* Calling the intensity of light I, and the time of insolation t, the equation $It=I_1t_1$ exists for equal shades. By the discovery of this important law we are enabled to express the intensity of light in comparable measures.

The observations of Bunsen and Roscoe only relate to the chloride-of-silver paper, but do not remain valuable for photographs on collodion, since the development of the image on collodion changes the relative intensity of the shades, as is known to every photographer. I have therefore prepared photographs of the sun on chloride-of-silver paper in order to measure the intensity of the light emitted by different points of the sun's disk.

The paper was carefully prepared, stretched over a plate of glass, and exposed in the camera fixed to the telescope. Two images of the sun were prepared on the 8th of March, 1872; one was exposed during 30 seconds, the other during 40 seconds. The diameter of the images was 108 millims.; the weakening of the light towards the edge of the disk was clearly seen in both. I then determined four points of equal intensity by a scale prepared photographically§. We assume that the intensity i of the light used for the preparation of the scale was constant, and that the time t of exposure was the same for all points of the sun's image. If I_0 is the intensity of the light in the middle of the sun's disk, I_1 that at another point, we have

$$I_0t=it_1,$$
$$I_1t=it_2,$$

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* Phil. Trans. 1840, p. 46. † Phil. Mag. vol. xxxiii. p. 339.

‡ Pogg. *Ann.* vol. cxvii. p. 329.

§ I think it superfluous to give a detailed description of the manner in which the scale was prepared and the comparison with the sun's image made, as the method was in general the same as that used by Bunsen and Roscoe, and described by them accurately in their 'Photochemical Researches.'

and therefore

$$I_0 : I_1 = t_1 : t_2.$$

I have collected the observations, which seem to me pretty consistent, in the following Table. The numbers given by the readings of the scale have been reduced so as to make the intensity in the middle of the disk equal to 100.

Distance from centre.	Intensities.	
Radius = 54.	First image.	Second image.
0	100·0*	100·0
4	100·0	99·0
8	......	97·9
9	99·0	
11	98·9	
12	......	100·0
13	97·9	
15	97·9	
16	......	95·8
17	95·6	
19	95·6	
20	......	95·0
21	93·4	
23	93·4	
24	......	90·1
25	93·3	
27	90·4	
28	.....	85·4
29	85·9	
31	83·7	
32	......	84·5
33	78·5	
35	77·2	
36	......	78·1
37	75·0	
39	68·8	
40	.....	67·7
41	64·6	
43	60·3	
44	......	56·4
45	53·1	
46	......	48·4
47	45·8	......
48	......	41·5
49	37·3	
50	......	25·3
51	29·8	
52	......	18·0
53	15·9	10·1
53·3	10·7	

The observations were repeated on the 13th of March. Two sheets of paper were again exposed during 30 and 40 seconds. The numbers given in the following Table were calculated by taking the mean of four observations, which were taken by determining the intensity on four points at equal distances from the sun, and situated on two diameters at right angles to each other.

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* In the observations the relative intensities were only expressed by whole numbers; the decimals in the Table arise from the reduction.

Distance from centre. Radius =54.	Intensities.
0	100·0
4	100·0
14	98·9
24	94·3
34	78·5
40	70·0
45	53·9
47	40·0
49	32·6
51	28·6
53	18·7

In the first days of the month of May numerous observations were made on photographs taken in the focus of the large equatorial instrument, which have only a diameter of 45·4 millims. In order to be able to compare these observations with those just given, I have assumed their radius to be 54, and reduced the observations by multiplying them with $\frac{54}{22·7}$. The numbers given are the means of eight observations on six photographs.

Distance from centre. Radius =54.	Intensities.
0·0	100·0
11·9	98·3
23·8	93·0
35·7	78·0
47·6	49·9
51·6	22·0
52·8	14·5

On the 5th of May a magnified photograph was taken, and the relative intensity was measured on points lying symmetrically from the centre on a diameter. The diameter of the photograph was 102·9. The tints were compared with two scales prepared at different times.

Distance from centre.	Intensities.	
Radius = 54.	Scale 1.	Scale 2.
0·0	100·0	100·0
10·5	98·4	99·0
21·0	90·0	94·1
31·5	84·3	86·3
41·9	65·9	68·3
47·2	49·0	48·0
52·5	28·1	30·7
53·5	18·8	19·5

I have tried to determine the relative intensities of the larger sun-spots, their penumbræ, and the adjoining surface of the sun. The mean of several observations showed the ratio of the intensity of the spot to the adjoining parts of the sun's surface to be 0·067; that of the penumbra was 0·630*.

If we take the distance from the centre as abscissa, the intensity as ordinate, we obtain a curve showing the decrease of the intensity from the centre of the sun's disk. The ordinates I of the curve which agrees best with the observations have been determined as follows for the distances E from the centre:—

E = sin θ.	θ.	I.
Radius = 12.		
0	0° 0′·0	100·0
1	4 46·8	100·0
2	9 35·7	99·4
3	14 28·6	98·2
4	19 28·3	96·4
5	24 37·5	93·7
6	30 0·0	89·8
7	35 41·2	84·5
8	41 48·6	77·0
9	48 35·4	66·0
10	56 26·6	51·0
11	66 26·6	33·0
12	90 0·0	13·5

If we compare the above observations with those made by Liais, Secchi, and others for the less-refrangible rays (*i.e.* the optically active heating rays), we see that the absorption for chemically active rays is considerably larger.

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* The observations must only be considered preliminary, and will be repeated as soon as large sun-spots appear. Liais (*l. c.* p. 327) determines the ratio of the intensity of light of a spot to the adjoining parts of the sun's disk to be 0·091 for rays of mean refrangibility, and that of the penumbra 0·5.

If these researches were executed for homogeneous rays, we might obtain interesting information as to the constitution of the sun's atmosphere.

The author of the above paper seems to have been unaware of the observations taken by Professor Roscoe in 1863 and communicated to the Royal Society (see Phil. Mag. vol. xxvii. p.384). Professor Roscoe has proved that the intensity of the chemically active rays at the centre of the sun is greater than at the edge of the disk. The absorption of the sun's atmosphere, according to Professor Roscoe's experiments, is not so large as that resulting from Vogel's observations. Calling the intensity at the centre of the solar disk 100, the mean of Roscoe's observations gives about 28 for the intensity at the edge, while Vogel gives 13. The discrepancy may be partly accounted for by the rapid decrease in the intensity near the edge; but it is likely that this is not the full explanation of it. Vogel does not state whether he used a refracting or a reflecting telescope. Suppose he used a reflecting one, the mean wave-length of the rays the intensity of which he measured would be smaller than that of the rays measured by Roscoe, who used a refracting telescope; and if this should be so, it will add another proof of the fact that the smaller the wave-length the greater is its absorption.—ARTHUR SCHUSTER, Ph.D.

XLIII. *On the Effects of Magnetization in changing the Dimensions of Iron, Steel, and Bismuth bars, and in increasing the Interior Capacity of Hollow Iron Cylinders.*—Part I. *By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.*[*]

I PURPOSE giving, in a series of papers, the results of a prolonged and careful research on the above subject.

Introduction.—In 1842 Joule discovered that when a current of electricity was passed through a helix which enclosed a bar of iron, the latter, on its magnetization, suddenly elongated a minute fraction of its length.

To present clearly Dr. Joule's experiments, we will give these abstracts from the excellent paper which he published in the Philosophical Magazine in 1847:—

"In order to ascertain how far my opinion as to the invariability of the *bulk* of a bar of iron under magnetic influence was well founded, I devised the following apparatus. Ten copper wires, each 110 yards long and $\frac{1}{20}$ of an inch in diameter, were bound together by tape so as to form a good and at the same time very flexible conductor. The bundle of wires thus formed was coiled upon a glass tube 40 inches long and 1½ inch in diameter. One end of the tube was hermetically sealed; and the other end was furnished with a glass stopper, which was itself perforated so as to admit of the insertion of a capillary tube. In making the experiments, a bar of annealed iron, 1 yard long and ½ an inch square, was placed in the tube, which was then filled up with water. The stopper was then adjusted, and the capillary tube inserted so as to force the water to a convenient height within it.

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[*] Communicated by the Author, having been read before the National Academy of Sciences in Cambridge, Massachusetts, November 22, 1872.

"The bulk of the iron was about 4,500,000 times the capacity of each division of the graduated tube; consequently a very minute expansion of the former would have produced a very perceptible motion of the water in the capillary tube; but, on connecting the coil with a Daniell's battery of five or six cells (a voltaic apparatus quite adequate to saturate the iron), no perceptible effect whatever was produced either in making or breaking contact with the battery, whether the water was stationary in the stem, or gradually rising or falling from a change of temperature. Now, had the usual increase of length been unaccompanied by a corresponding diminution of the diameter of the bar, the water would have been forced through twenty divisions of the capillary tube every time that contact was made with the battery.

"Having thus ascertained that the bulk of the bar was invariable, I proceeded to repeat my first experiments with a more delicate apparatus, in order, by a more careful investigation of the laws of the increment of length, to ascend to the probable cause of the phenomenon.

"A coiled glass tube, similar to that already described, was fixed vertically in a wooden frame. Its length was such that when a bar 1 yard long was introduced so as to rest on the sealed end, each extremity of the bar was a full inch within the corresponding extremity of the coil. The apparatus for observing the increment of length consisted of two levers of the first order, and a powerful microscope situated at the extremity of the second lever. These levers were furnished with brass knife-edges resting upon glass. The connexion between the free extremity of the bar of iron and the first lever, and that between the two levers, was established by means of exceedingly fine platinum wires.

"The first lever multiplied the motion of the extremity of the bar 7·8 times; the second multiplied the motion of the first 8 times; and the microscope was furnished with a micrometer disequently each division of the micrometer passed over by the index indicated an increment of the length of the bar amounting to $\frac{1}{138528}$ of an inch.

"The quantities of electricity passing through the coil were measured by an accurate galvanometer of tangents, consisting of a circle of thick copper wire 1 foot in diameter, and a needle ½ an inch long furnished with a suitable index.

"The quantities of magnetic polarity communicated to the iron bar were measured by a finely suspended magnet 18 inches long, placed at the distance of 1 foot from the centre of the coil. This magnetic bar was furnished with scales precisely in the manner of an ordinary balance; and the weight required to bring it to a horizontal position indicated the intensity of the magnetism of the iron bar under examination.

"After a few preliminary trials, a great advantage was found to result from filling the tube with water. The effect of the water was, as De la Rive had already remarked, to prevent the sound. It also checked the oscillations of the index, and had the important effect of preventing any considerable irregularities in the temperature of the bar.

"The first experiment which I shall record was made with a bar consisting of two pieces of well-annealed rectangular iron wire, each 1 yard long, ¼ of an inch broad, and about ⅛ of an inch thick. The pieces were fastened together so as to form a bar of nearly ¼ of an inch square. The coil was placed in connexion with a single constant cell, the resistance being further increased by the addition of a few feet of fine wire. The instant that the circuit was closed, the index passed over one division of the micrometer. The needle of the galvanometer was then observed to stand at 7° 20′, while the magnetic balance required 0·52 of a grain to bring it to an equilibrium. It had been found by proper experiments that a current of 7° 20′ passing through the coil was itself capable of exerting a force of 0·03 of a grain upon the balance; consequently the magnetic intensity of the bar was represented by 0·49 of a grain. On breaking the circuit the index was observed to retire 0·3 of a division, leaving a permanent elongation of 0·7, and a permanent polarity of 0·42 of a grain. More powerful currents were now passed through the coil and the observations repeated as before, with the results tabulated below:—

Experiment I.

Deflection of galvanometer.	Tangent of deflection.	Elongation or shortening of bar.	Total elongation.	Magnetic intensity of bar.	Square of magnetic intensity divided by total elongation.
− 7° 20′	128	1·0 E.	1·0	−0·49	240
0	0	0·3 S.	0·7	−0·42	252
− 9 30	167	2·9 E.	3·6	−0·93	240
0	0	1·2 S.	2·4	−0·74	228
−14 48	264	5·9 E.	8·3	−1·42	243
0	0	3·8 S.	4·5	−1·00	222
−23 10	428	10·3 E.	14·8	−1·87	236
0	0	7·6 S.	7·2	−1·26	220
−47 25	1088	16·1 E.	23·3	−2·22	211
0	0	13·9 S.	9·4	−1·35	194
−58 50	1653	14·8 E.	24·2	−2·21	202
0	0	13·3 S.	10·9	−1·35	168"

Dr. Joule now reversed the current in the helix and found that a current which deflected the needle 6° 15′ *shortened* the bar 3·4 div., and that after the current was broken its magnetic intensity was found reduced from −1·3 (the permanent intensity previously given by 47° 25′, see preceding Table) to −·17. He then passed a current of 9° 55′; and this he found was sufficient, not only to remove the former minus polarity of the bar, but also to give it a permanent polarity of +·25, and yet to leave the bar with 6·6 of the elongation belonging to its previous minus polarity.

Taking Joule's observations while the current was passing around the bar, we have for the current of 6° 15′ a magnetic intensity of −0·12, and for the current of 9° 15′ a *plus* magnetic polarity of 0·57. We call attention to these results because subsequent experimenters* seem to be unaware of these observations of Dr. Joule, who here first shows that a feeble current will demagnetize and even reverse the polarity of a bar which has previously required a far more powerful current to give it its permanent magnetic charge. In the experiment given above, the ratio of the current-intensities of permanent magnetization and of demagnetization is 1088 to 175.

Dr. Joule now successively replaced the above bar by two others, and obtained with them similar results. He then deduces the following important law:—"From the last column of each of the preceding Tables we may, I think, safely infer that

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* Wiedemann, Pogg. *Ann.* vol. c. p. 235; also R. W. Wilson, "Demagnetization of Electromagnets," American Journal of Science, vol. iii. 3rd Series, p. 346.

the elongation is in the duplicate ratio of the magnetic intensity of the bar, both when the magnetism is maintained by the influence of the coil, and in the case of the permanent magnetism after the current has been cut off. The discrepancies observable will, I think, be satisfactorily accounted for when we consider the nature of the magnetic actions taking place. When a bar experiences the inductive influence of a coil traversed by an electrical current, the particles near its axis do not receive as much polarity as those near its surface, because the former have to withstand the opposing inductive influence of a greater number of magnetic particles than the latter. This phenomenon will be diminished in the extent of its manifestation with an increase of the electrical force, and will finally disappear when the current is sufficiently powerful to saturate the iron. Again, when the iron, after having been magnetized by the coil, is abandoned to its own retentive powers by cutting off the electrical current, the magnetism of the interior particles will suffer a greater amount of deterioration than that of the exterior particles. The polarity of the former may indeed be sometimes actually reversed, as Dr. Scoresby found it to be in some extensive combinations of steel bars. Now, whenever such influences as the above occur, so as to make the different parts of the bar magnetic to a various extent, the elongation will necesarily bear a greater proportion to the square of the magnetic intensity measured by the balance than would otherwise be the case.

"For similar causes the interior of the bar will in general receive the neutralization and reversion of its polarity before the exterior; and hence we see in the Tables that there is a considerable elongation of the bar after the reversion of the current, even when the effect upon the balance has become imperceptible, owing to the opposite effects of the interior and exterior magnetic particles."

Joule now experimented on a bar of unannealed iron, and on three bars of soft steel. As these bars had considerable degrees of retentive power, the anomalies occasioned by the above-described actions did not exist to any considerable extent, and they gave a confirmation of the law that the elongation is proportional, in a given bar, to the square of the magnetic intensity.

The next bar he experimented with was of moderately hardened steel. This bar was slightly increased in length every time that contact with the battery was broken, although a considerable diminution of the magnetism of the bar took place at the same time. He says:—"I am disposed to attribute this effect to the state of tension in the hardened steel, for I find that soft iron wire presents a similar anomaly when stretched tightly."

In a subsequent communication, contained in the same volume of the Philosophical Magazine, Dr. Joule gives accounts of numerous experiments made upon wires and bars of soft iron, cast iron, soft and hardened steel, subjected to various pressures and tensions while they were magnetized. As an example of the effect of *tension* on the phenomena, he states that in the case of a bar 1 foot long and $\frac{1}{4}$ of an inch in diameter, a tensile force of about 600 pounds caused all the phenomena of changes of length to disappear, even with a current which produced a deflection of 58° in the needle of the tangent-galvanometer; but when a current of 61° was passed around this bar, subjected to a tension of 1040 pounds, it *shortened* 2·8 divisions. With a tension of 1680 and the same current the bar shortened 4·5 divisions. Joule, from his experiments, deduces this law; viz. *In the case of tension the shortening effect is proportional to the current traversiny the coil multiplied by the magnetic intensity of the bar.* He further states that "it is extremely probable that the shortening effects are proportional, *cæteris paribus*, to the square root of the force of tension."

In the case of bars of cast iron he finds that their elongation is equal, if not superior, to those of soft iron when magnetized to the same degree; and an increase of tension in them does not produce half the retraction which is caused in soft iron bars in similar circumstances.

Bars of soft steel acted like the bars of iron; but the superior retentive powers of the former enabled him to trace better the elongating effects of the permanent magnetism, which diminished with the increase of tension and at last disappeared altogether; but with bars of perfectly hardened steel no sensible change in their lengths was produced by charges of *permanent* magnetism, and the *temporary* shortening effect of the coil was proportional to the magnetism multiplied by the current traversing the coil. The shortening effect did not in these cases sensibly increase with the increase of tension.

On subjecting bars of wrought and cast iron and soft steel to pressure, Joule found that it had no sensible effect upon the extent of their elongation. A hard steel cylinder a foot long, when submitted to the same experiments, with a pressure of 80 pounds, "suffered a diminution of length equal to 0·1 of a division of the micrometer, with a current capable of giving a magnetic polarity of 1·7."

At the termination of his paper Dr. Joule gives the following "*postscript.*" "I have already, in the former part of this paper, described an experiment which indicated that no alteration in the *bulk* of a bar of soft iron could be produced on magnetizing it. I thought, however, that it would be interesting to confirm the fact by an observation of the alteration of the dimensions of this purpose I took a piece of drawn iron gas-piping 1 yard long, $\frac{3}{16}$ of an inch in bore, and $\frac{3}{16}$ of an inch in thickness. A piece of thick covered copper wire was inserted into this tube and bent over the outside of it. The lower extremity of the iron tube being fixed, and the upper end being attached to the micrometrical apparatus, each division of which corresponded to $\frac{1}{138528}$ of an inch, I obtained . . . . results which show that the length of the tube was diminished in order to make up for the increase of its diameter, which in this instance was in the direction of the polarity. The quantity of the shortening effect, viz. 3·4, is, however, only one third of that due to the maximum elongation of soft iron bars as observed in the first section. This is probably owing to the grain of the iron being in cross directions with respect to the polarity in the two cases, and partly perhaps to the iron tube not being fully saturated with magnetism. The experiment is worth repeating, especially as it affords a means of studying the magnetic condition of closed circuits."

Remarking on the cause of the phenomena of elongation, Dr. Joule says:—"The law of *elongation* naturally suggests the joint operation of the attractive and repulsive forces of the constituent particles of the magnet as the cause of the phenomena. On the other hand, the fact that the *shortening effect* is proportional to the magnetic intensity of the bar multiplied by the current traversing the coil seems to indicate that in this case the effect is produced by the attraction of the magnetic particles by the coil. But then it will be asked why so remarkable an augmentation of the effect is produced by the increase of tension in the case of the soft iron bars. When we are able to answer this question in a satisfactory manner, we shall probably have a much more complete acquaintance with the real nature of magnetism than we at present possess."

This full account of Dr. Joule's remarkable research is here presented in order to give an exposition of our present knowledge of this subject, and clearly to set forth the relations which my own attempts bear to his labours. Here Joule, the discoverer of these phenomena, has given us almost all the knowledge we have up to this time possessed in reference to their characteristics and their laws. That a subject so fascinating should not have been eagerly followed up appears strange, especially so when it seems highly probable that the faithful study of these actions may one day give us an insight into the dynamic nature of electro-magnetization, and thus lead the investigator into a fruitful field of research.

No one can duly appreciate this work of Joule's until he attempts the confirmation of his results; then the difficulties of the research and the skill and acumen of this eminent physicist will be properly estimated.

Although the cognate discovery by our countryman Page, in 1837, that iron bars produce sound on their magnetization, has been carefully studied by Delezenne, De la Rive, Beatson, Marrian, and Wertheim, yet in the annals of science I have found only two experimental investigations, in addition to the one by Joule, on the phenomena of the elongation produced in iron rods on their magnetization. The first is by Wertheim, in the *Ann. de Chim. et de Phys.* 3 sér. vol. xxiii.; the second by Tyndall, contained in a paper entitled "On some Mechanical Effects of Magnetization," published in his 'Researches on Diamagnetism and Magnecrystallic Action,' London, 1870.

In Wertheim's memoir "On the Sounds produced in Magnetized Iron," all we find on the subject of the elongation of magnetized iron rods is the following:—"Here are the results of these experiments: the helix being placed so that its axis coincides with that of the bar, we do not observe any lateral movement, but only a very small elongation; this elongation rarely surpasses ·002 millim. [in rods about 970 millims. long], and although visible is hearly measurable; it is most pronounced when the helix [whose length was a litttle over ⅕ of that of the rod] encloses the extremity of the bar; it diminishes as the helix approaches the point [the centre] where the rod is clamped; and it is probable that when it is quite close to this point the elongation changes into a retraction; but I have never been able to observe the motion in this direction with any certainty. . . . . I have already remarked that it was not possible for me to measure this longitudinal traction; happily Mr. Joule has supplied that omission."

Dr. Tyndall opens his paper thus:—"Wishing, in 1855, to make the comparison of magnetic and diamagnetic phenomena as thorough as possible, I sought to determine whether the act of magnetization produces any change of dimensions in the case of bismuth, as it is known to do in the case of iron. The action, if any, was sure to be infinitesimal; and I therefore cast about for a means of magnifying it. . . . . I consulted Mr. Becker; and thanks to his great intelligence and refined skill, I became the possessor of the apparatus now to be described. . . . . The same apparatus has been employed in the examination of bismuth bars; and though considerable power has been applied I have hitherto failed to produce any sensible effect. It was at least conceivable that complementary effects might be here exhibited, and a new antithesis thus established between magnetism and diamagnetism."

The apparatus used by Dr. Tyndall consisted of two vertical brass rods firmly cemented into a block of stone. Between these rods, securely fixed in the stone, were placed the rods of iron whose elongation he desired to measure. On the vertical rods slid a transverse bar of brass carrying "a vertical rod of brass which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point passes against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates." The angular deflections of the mirror he determined by the method of Poggendorff—that is, by viewing in a telescope the divisions of a fixed scale reflected from the mirror.

Dr. Tyndall gives the following account of his experience with this apparatus:—"Biot found it impossible to work at his experiments on sound during the day in Paris; he was obliged to wait for the stillness of night. I found it almost equally difficult to make accurate experiments, requiring the telescope and scale, with the instrument just described, in London. Take a single experiment in illustration. The mirror was fixed so as to cause the cross hair of the telescope to cut the number 727 on the scale: a cab passed while I was observing; the mirror quivered, obliterating the distinctness of the figure, and the scale slid apparently through the field of view and became stationary at 694. I went upstairs for a book; a cab passed, and on my return I found the cross hair at 686. A heavy waggon then passed, and shook the scale down to 420. Several carriages passed subsequently; the figure on the scale was afterwards 350. In fact so sensitive is the instrument, that long before the sound of a cab is heard its approach is heralded by the quivering of the figures on the scale.

"Various alterations which were suggested by the experiments were carried out by Mr. Becker, and the longer I worked with it the more mastery I obtained over it; but I did not work with it sufficiently long to perfect its arrangement. Some of the results, however, may be stated here:—

	Figure of scale.
Bar unmagnetized . . .	577
Bar magnetized	470
Bar unmagnetized . . .	517

"Here the magnetization of the bar produced an elongation expressed by 107 divisions of the scale, while the interruption of the circuit produced only a shrinking of 47 divisions. There was a tendency on the part of the bar or of the mirror to persist in the condition superinduced by the magnetism. The passing of a cab in this instance caused the scale to move from 517 to 534; that is, it made the shrinking 64 instead of 47. Tapping the bar produced the same effect.

"The bar employed here was a wrought-iron square core, 1·2 inch a side and 2 feet long.

"The following Tables will sufficiently illustrate the performance of the instrument in its present condition. In each case are given the figures observed before closing, after closing, and after interrupting the circuit. Attached to each Table also are the lengthening produced by magnetizing and the shortening consequent on the interruption of the circuit:—

Circuit.	Scale 10 cells.		Circuit.	Scale. 20 cells.	
Open	647		Open	653	
Closed......	516	131 elongation.	Closed ...	465	188 elongation.
Broken ...	581	65 return.	Broken ...	579	114 return.
Open	637		Open	638	
Closed ...	509	128 elongation.	Closed ...	452	186 elongation.
Broken ...	579	70 return.	Broken ...	568	116 return.
Open	632		Open	632	
Closed ...	491	141 elongation.	Closed ...	472	160 elongation.
Broken ...	568	77 return.	Broken ...	561	89 return.

"These constitute but a small fraction of the numbers of experiments actually made. There are very decided indications that the amount of elongation depends on the molecular condition of the bar. For example, a bar taken from a mass used in the manufacture of a great gun at the Mersey Iron works, suffered changes on magnetization and demagnetization considerably less than those recorded here. I hope to return to the subject."

XLIV. *On the Intensity of Light &c.*

By HENRY HUDSON, *M.D., M.R.I.A.*

To the Editors of the Philosophical Magazine and Journal,

Glenville, Fermoy,
March 4, 1873.

GENTLEMEN,

I THINK Mr. Bosanquet could not have looked into the Astronomer Royal's excellent little work 'On the Undulatory Theory of Light' when he wrote (p. 217) that "The explanation given by Airy of the doubling of intensity . . . . can only be regarded as an illustration." Sir George writes (p. 20), "We shall assume the intensity of the light to be represented by c^2," and (in a note) adds, "We *must* take some even power of c to represent the intensity, since the undulation where the vibration is expressed by $-c\sin\left\{\frac{2\pi}{\lambda}(vt-x)+\mathrm{C}\right\}$ differs in no respect from that whose vibration is expressed by

$$+c\sin\left(\frac{2\pi}{\lambda}(vt-x)+\mathrm{C}\right),$$

except that it is half the length of a wave before or behind it."

It would appear, therefore, that the Astronomer Royal was influenced to adopt the "square" of the amplitude as the measure of intensity chiefly because the *even powers* of positive and negative quantities are (*algebraically*) identical.

I would suggest, however, that the true *physical* interpretation of the signs (+ and −) prefixed respectively to two perfectly similar vibrations is that "the coefficient (c) must be measured *in opposite directions* from the point of rest of the disturbed particle," which in fact constitutes the difference, by half a wave-length, of these two similar vibrations; and it is evident (algebraically as well as physically) that the combination of any two such vibrations must produce *zero* (i. e. *darkness* in the case of light), or in optical language "*interference*."

I would now submit my view of Sir George Airy's argument (in his note, p. 20) to the consideration of mathematicians. First, let us assume (with Sir George, p. 7) that c is the "maximum vibration of the disturbed particle;" in this case (the wave-length and amplitude being identical) it appears to me that the only effect of introducing C and D into two perfectly similar vibrations is (see 'Undulatory Theory,' p. 6) to "alter the origin of the linear measure from which x is reckoned," and that no conclusion as to the "influence of amplitude on intensity" can be deduced from such a change. Secondly (c and λ being still alike in both the new forms), if we consider c to represent merely the "actual distance of the disturbed particle from its place of rest," the introduction of C and D into the vibration-formula *may* also represent a "change of phase of the wave." But (inasmuch as c no longer represents the *maximum vibration*) it will not be possible to deduce any "influence of amplitude on intensity" from the formulæ even in this case. Musical men are aware that a "pizzicato note" from a stringed instrument, even with very moderate amplitude of vibration, can be heard at a considerable distance. *Suppose* now that such a sound wave (with 1 inch amplitude) becomes insensible at 200 feet: if the amplitude be reduced to half an inch, the distance at which the sound should no longer be audible ought to be 100 feet only if the *square* of the amplitude be the measure of its intensity, or at 141 feet if the *amplitude simply* be the correct indicator of intensity; or if the amplitude of the second wave were one third of an inch only, then the distances (according to the two hypotheses) would be about 67 feet and 115 feet respectively.

HENRY HUDSON.

P.S.—In such an experiment we have the advantage of dealing with a single wave.

XLV. *On the Definition of Intensity in the Theories of Light and Sound. By* ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*[*].

IN a note upon the subject of this paper, contained in the March Number of the Philosophical Magazine, Mr. Bosanquet expresses himself as follows:—

"Mr. Moon has not offered any answer to the remark made at the end of my paper of last November, although, if he understood it, it is conclusive in the case of light."

There are some truths so obvious, some arguments which appear so decisive, that one is apt to suppose that the mere statement of them will suffice to carry conviction to the mind even of an opponent. The argument I offered to Mr. Bosanquet appeared to me precisely of that character. As he, however, regards it in a different light, I am ready to meet him upon his own ground.

I have no intention to contest the substantial approximate truth of an experimental law so long established, so *à priori* all but certain, as Malus's rule of cosines; but I demur *in toto* to Mr. Bosanquet's conclusion that the adoption of the simple power of the amplitude as the measure of intensity in plane-polarized rays involves the assumption that $a(\sin\alpha+\cos\alpha)$ measures the intensity of the overlapping beams in the experiment which he discusses. So far is this from being the fact, that the latter assumption contradicts the former, as can readily be shown. For, suppose that at a particular point where the beams overlap, the oppositely polarized rays happen to be in the same phase, as they may be; they will then give rise to a single plane-polarized ray, whose intensity would be a according to the measure which I have proposed, and not $a(\sin\alpha+\cos\alpha)$, as the measure which Mr. Bosanquet thus gratuitously seeks to fix upon me would indicate.

The inconsistency of this proposed extension of my definition may be equally seen in the general case, where the oppositely polarized rays are in different phases. For, representing the rays by

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[*] Communicated by the Author.

$$\left.\begin{array}{l} a\cos\alpha\,.\,\sin X, \\ a\sin\alpha\,.\,\sin(X+D), \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

the first may be replaced by two oppositely polarized rays represented respectively by

$$a\cos^2\alpha\,.\,\sin X, \quad a\cos\alpha\sin\alpha\,.\,\sin X\,; \quad . \quad . \quad . \quad (2)$$

and the second by two similar rays represented by

$$a\sin^2\alpha\,.\,\sin(X+D), \quad -a\cos\alpha\sin\alpha\,.\,\sin(X+D)\,.\,. \quad (3)$$

Hence, combining the expressions for rays polarized in the same plane, we shall have in place of (1) *two* waves polarized in opposite planes, respectively represented by

$$a\,.\left\{(\cos^2\alpha+\sin^2\alpha\cos D)\,.\,\sin X+\sin^2\alpha\,.\,\sin D\,.\,\cos X\right\},$$
$$a\,.\cos\alpha\sin\alpha\,.\left\{(1-\cos D)\sin X-\sin D\,.\,\cos X\right\};$$

which may be written

$$a\sqrt{\cos^2\alpha+\sin^2\alpha\cos D)^2+\sin^4\alpha\,.\,\sin^2 D}\,.\,\sin(X+D_1),$$
$$a\,.\sin\alpha\cos\alpha\,.\sqrt{(1-\cos D)^2+\sin^2 D}\,.\,\sin(X+D_2);$$

or

$$a\sqrt{\cos^4\alpha+\sin^4\alpha+2\sin^2\alpha\cos^2\alpha\cos D}\,.\,\sin(X+D_1),$$
$$a\sin\alpha\cos\alpha\sqrt{2(1-\cos D)}\,.\,\sin(X+D_2).$$

Now we have just as much right to take the sum of the amplitudes of these two waves for the intensity at any point of the overlapping beams, as we have to take the sum of the amplitudes of the waves represented by (1) for the like purpose. A comparison of the results thus derivable, however, will show that they are incompatible, and consequently that Mr. Bosanquet's proposed extension of my definition of intensity in the case of oppositely polarized rays cannot be entertained.

Undoubtedly, however, I may be expected to state how I propose to estimate the collective effect of the oppositely polarized rays in the circumstances referred to; and this I shall have no difficulty in doing.

If this collective effect is capable of being expressed by a function of the intensities of the two waves when acting separately, whatever be the phases of the latter, we shall have

$$\text{intensity} = F\{a\cos\alpha),\ (a\sin\alpha)\}\,;$$

and if we can discover the form of F corresponding to any particular state of phase, we shall know the form of F for all varieties of phase.

Now, when the two rays are in the same phase, we have

$$a = \text{intensity} = \mathrm{F}\left\{a \cos \alpha), (a \sin \alpha)\right\};$$

therefore

$$\mathrm{F}\left\{(a \cos \alpha), (a \sin \alpha)\right\} = \sqrt{(a \cos \alpha)^2 + (a \sin \alpha)^2}.$$

Hence, in the case under consideration, the intensity will not be measured by the sum of the amplitudes, but by *the square root of the sum of the squares of the amplitudes of the component rays.*

It thus appears that the argument which Mr. Bosanquet puts forward as decisive against the simple power of the amplitude being taken for the measure of intensity in plane-polarized waves has, in fact, no bearing upon the subject.

The measures of intensity which I have proposed as applicable to plane and elliptically polarized light coincide in a remarkable manner.

For, when the component rays are represented by (1), the resulting ray will be elliptically polarized—the magnitude and position of the axes of the ellipse depending on a, which measures the intensity of the incident light, D which represents the difference of phase of the component rays, and α the inclination of the principal plane of the crystal to the plane of polarization of the beam originally incident upon it.

The absolute magnitude of either axis will always be proportional to a, while the position of the axes and their ratio to each other depend on D and α. Hence, so long as its *form* is unaltered, the circumference of the ellipse (*i. e.* the length of path described by a particle in a single undulation) will vary as a; and the intensity of the overlapping rays for the same form of vibration will also vary as a.

It thus appears that when from the consideration of plane-polarized we turn to that of elliptically polarized light, length of path of the particles is not sufficient to determine the intensity; the form of vibration must also be taken into account; but for a fixed form of vibration the intensity varies directly as the length of path of the particles.

The single argument I adduced against Mr. Bosanquet's view of the relation of the amplitude to the intensity was that, according to the latter, two equal vibrations in the same phase will give four times as much illumination as either separately. This argument, which appears to me irrefragable, Mr. Bosanquet regards as so utterly trifling that it is only from the consideration that it is "sometimes felt as a difficulty by learners" that he is induced to "just touch upon it."

Admitting, after some hesitation, "that two vibrations may be superposed with coincident phase," he proceeds as follows:—

"In these cases we must not treat each vibration as a cause in itself, invariable under all conditions. If we regard the two vibrations as unaltered by the superposition, we shall in general be wrong."

This line of defence cuts away the ground upon which rest the undulatory theories of light and sound. Those theories alike assume the application of the principle of the superposition of small motions in all cases with which they undertake to deal. The theory of interference assumes that the motion in a wave may be represented by the ordinate of a particle—and that where two waves are superposed in which the vibration is in the same plane, their joint effect is represented by the algebraical sum of the expressions for the ordinates corresponding to each wave taken separately.

This implies that the superposition of two equal waves in the same phase will produce, as regards any particle at a given time, twice the force acting upon it, twice the velocity impressed upon it, twice the space described by it, which would occur if either wave were destroyed.

What Mr. Bosanquet has to do is to reconcile this state of things with the production, under the same circumstances, of a quadruple amount of illumination or loudness.

The received theory in effect states that where two equal waves in the same phase act together, the effect of each in producing, at a distant point of the air or æther at a given time, effective force, velocity, and displacement will be precisely *the same* as either would have produced under the same circumstances if it had acted separately—and that if we have three or more such waves superposed, no alteration will occur in the separate action, so estimated, of each.

The received definition of intensity, on the other hand, implies that, coexisting with the state of things just described, we shall have, where two waves operate together, *each* wave producing *twice* the effect estimated by the amount of sound resulting from it which either would produce separately; where three waves act together, *each* will produce *three* times the effect, so estimated, which it would have produced separately; and so on.

To what purpose, I would ask, are we called upon to embarrass ourselves with these extravagant conclusions? The definition of intensity was invented for the special purpose of explaining the phenomena of interference, or, to speak more precisely, to determine the points of maximum and minimum intensity in interfering rays; and whether we take the simple power of the amplitude or its square as the measure of intensity, the positions of maximum and minimum intensity so resulting will be absolutely identical.

6 New Square, Lincoln's Inn,
March 15, 1873.

P.S.—Fresnel, in his "Mémoire sur la Diffraction de la Lumière" (*Mémoires de l'Académie*, Paris, 1826, p. 406), distinguishes between "l'intensité des vibrations" as depending on the simple power, and "l'intensité de la lumière" as depending on the square of the particle-velocity. Can any intelligible ground be assigned for this distinction?

XLVI. *On Diffraction*. *By* G. QUINCKE[*].

IN a searching investigation of the phenomena which occur in the inflection (diffraction) of light, I have arrived at results which deviate from the representations hitherto given, in several, and, I think, important points.

Some time since (see Pogg. *Ann.* vol. cxlvi. pp. 1–65, 1872), I treated theoretically the phenomena which are perceived when a point or a line of light is looked at, with a telescope or with the naked eye, through a diffraction-grating—that is, a combination of apertures of the same size and shape and at equal distances from each other. The theory therefore comprises gratings with opaque or transparent bars, as well as such as are cut with a diamond-point in a plane glass or metal plate. Besides the validity of Huyghens's principle, it was therein presupposed that a furrow-grating consists of depressions with little stair-like steps, one face of which is parallel to the untouched face of the plate.

For furrow-gratings the formulæ are much more complicated than for gratings with opaque bars. They show, in accordance with the experiments, that the luminous intensity with these gratings (which in practice are used in preference) depends very considerably on the dimensions of the furrows and on the substance with which they are filled up, whether the light be transmitted through the grating or reflected from it.

The investigation of reflected light affords this advantage, that the experiment can be better accommodated to the presuppositions of the calculation than with transmitted light. Symmetrically formed furrows, and elevation-gratings of the same material (which can be very completely produced galvanoplastically with the aid of some experimental artifices), exhibit the same properties when right and left are exchanged.

For the determination of the wave-lengths of light the so-called *maxima of the second class*, known to Fraunhofer*, are ordinarily made use of. The greater the wave-length, and the less the distance between two adjacent groups of apertures of the grating, the greater is the distance of these maxima from one another.

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[*] Translated from a separate impression communicated by the Author.

Besides these maxima, however, as we learn from experiment, other maxima, less luminous, make their appearance, which I have named *secondary*, and which the theory does not enable us to foresee. If m denotes a whole number, the secondary maxima are situated at $\frac{1}{m}$, $\frac{2}{m}$, &c. of the distance between two neighbouring maxima of the second class, or at the places where a grating with 2, 3, . . . m times the distance between the apertures or furrows would show maxima of the second class. Their situation relative to the maxima of the second class is, with the same grating, the same in transmitted or reflected light for diffraction in the most diverse substances. The incident rays may make any angle we please with the normal to the surface of the grating. Under otherwise like circumstances, however, the value of m may change with the colour.

The gratings were selected as various as possible; the distance between two adjacent groups of apertures varied between 0·2 and 0·0025 millim. The experiments were made upon gratings with opaque bars in air or water, with apertures in an opaque layer of soot, silver-collodion, silver, gold-leaf, or in iodide of silver on a glass plate, and furrow- or ridge-gratings cut in glass or metal.

I have now studied the diffraction of polarized light by these gratings.

If we look at a sodium-flame through a doubly refracting prism and a grating with vertical apertures or furrows, we see two series of flame-images, one over the other, polarized parallel and perpendicular to the principal diffraction-plane. Two flame-images, one above the other, corresponding to the same maximum of the second class, usually appear equally bright. Only in isolated spots, mostly with feebler intensity of light, do any differences appear. If we advance to flame-images of a higher order, sometimes the light polarized parallel, and sometimes that polarized perpendicular to the principal diffraction-plane may predominate.

Similar differences are observed in reflected light; and, indeed, here again furrow- and elevation-gratings of symmetrical form exhibit the same phenomena as soon as right and left are exchanged.

Slight differences in the shape of the apertures, or furrows, or elevations of a grating have a very considerable influence on the difference of intensity of the light polarized parallel and perpendicular to the principal diffraction-plane. The phenomenon changes with the colour of the flame, the substance in which the diffraction takes place, and the angle of incidence of the rays.

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* Gilbert's *Ann.* vol. lxxiv. p. 340 (1823).

Further, in front of the object-glasses of a collimator and an astronomical telescope, I placed two Nicol prisms, the azimuth of which could be determined with accuracy to minutes on vertical circles. The grating was placed between the prisms. In some instances the telescope was laid aside, and the eye looked directly through the analyzing Nicol at the grating. The slit of the collimator was usually illuminated with daylight.

When the Nicol prisms were crossed, the illuminated slit in the eyepiece of the telescope appeared black; on inserting the grating, it was illuminated, and the maxima or spectra of the second class became visible. The central image of the slit appeared variously coloured, according to the position of the Nicol prisms. Viewed with one eyepiece-prism, it mostly shows a dark streak in the spectrum, parallel to the Fraunhofer lines; and on rotating the analyzing Nicol to greater azimuths, with some gratings this streak travels towards the red, with others towards the blue. The latter case, where the component polarized parallel to the principal plane of diffraction is greater for the red than for the blue, is the more frequent.

The amount of rotation of the analyzer which carried the dark streak through the entire spectrum varied with the angle of incidence, the nature of the material of the grating-bars or furrows, the fineness of the grating, and the substance in which the diffraction took place. It varied from a fraction of a minute to $\frac{3}{4}^\circ$ in transmitted light.

In lateral spectra likewise, parallel to Fraunhofer's lines appear dark streaks, which with the rotation to greater azimuths, according to the grating and the spectrum, go from the red to the blue, or from the blue to the red. The rotation is very different with different gratings, and with different lateral spectra with the same grating, and may amount to 5° or more. With greater angles of diffraction the superposition of spectra of different orders disturbs the observation.

When several parallel gratings are inserted one behind another, very complicated phenomena enter, which have been partly investigated by Brewster* and Crova†. With a suitable arrangement of the gratings, the turning of the polarization-plane for a determined maximum of the second class can be increased.

Often the dark streaks do not appear in the spectrum until, simultaneously with the grating, a mica plate of $\frac{\lambda}{4}$ is put between the Nicol prisms in a suitable azimuth. The diffracted light is then elliptically polarized. For individual gratings the difference of phase of the components polarized parallel and perpendicular to the principal diffraction-plane can be determined by a Babinet compensator.

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* Phil. Mag. S. 4. vol. xxxi. pp. 22 & 98 (1866).

† *Comptes Rendus*, vol. lxxii. p. 855 (1871); vol. lxxiv. p. 932 (1872).

Still more striking than in transmitted light are the phenomena when, in the azimuth ±45°, linearly polarized light is reflected from a grating, especially a silvered furrow or elevation-grating. In the spectrum of the central image, or the side spectrum of the second class, with the Nicol prisms in a certain position, one or more dark streaks then make their appearance, which when the Nicols are rotated travel from one Fraunhofer's line to another or disappear. Their site varies with the form, distance, and material of the furrows or elevations, the angle of incidence, and the substance in which the diffraction takes place. Furrow- and elevation-gratings of symmetrical form again show the same phenomena. Deviations are to be accounted for by small differences in shape of the furrows or elevations, which very considerably influence the phenomenon.

Between crossed Nicol prisms, a grating shows in the transmitted or reflected light secondary maxima with some Fraunhofer's lines which without them are not perceived. They are variously coloured, according to the azimuth of the analyzing prism.

Different gratings show quantitative, but not qualitative differences, as I have found by numerous measurements, which will soon be given in another place, where the labours of other observers will also be described.

Abstracted from all theoretical considerations, the experiments showed:—

1. Linearly polarized light gives in general, after diffraction, light elliptically polarized.

2. The difference of phase and ratio of amplitude of the components polarized parallel and perpendicular to the principal diffraction-plane vary, the angle of incidence being the same, with the order of the spectrum, so that with an increasing angle of diffraction they may become greater or less. An increase or diminution, however, may be succeeded by a diminution or an increase, and so forth.

3. The increase or diminution is very different for different colours; and, under otherwise similar conditions, one colour may show an increase, another a diminution.

4. If the difference of phase of the two components polarized parallel and perpendicular to the principal diffraction-plane is small, in the diffracted light a rotation of the plane of polar-

ization is perceived, which with the same angle of incidence and the same spectrum of the second class is different in amount, and the absolute value of which may become greater or less as the wave-length increases. To an azimuth $+\alpha$ or $-\alpha$ of the incident light corresponds after the diffraction the same azimuth $+\beta$ or $-\beta$ of the transmitted or reflected light. For the directly transmitted or reflected rays (corresponding to the diffraction-angle 0°) the rotation of the plane of polarization may amount to a few minutes or several degrees—with the lateral maxima of the second class, to 90° or more. The more frequent case is where the amplitude polarized perpendicular to the principal diffraction-plane, or parallel to the lines (furrows) of the grating, is greater for blue than for red light.

5. A grating inserted between Nicol prisms or polarizing apparatus, when the incident light is white, imparts to the directly transmitted or reflected light similar colours to those shown by plates of crystal between polarizing arrangements.

6. Ratio of amplitude and difference of phase vary, under otherwise like conditions, with the inclination of the grating to the incident rays.

7. Ratio of amplitude and difference of phase change, for normal as well as for oblique incident rays, with the substance of which, with transmitted light the surface of the bars, with reflected light the furrows or elevations of the grating consist.

8. The amplitude-ratio and phase-difference change with the width of the apertures or the form of the furrows or elevations.

9. The finer the grating, or the more it is inclined to the incident rays, the greater, *cæteris paribus*, is the change produced by diffraction in the ratio of amplitude and the difference of phase of the light-waves polarized parallel and perpendicular to the principal plane of diffraction.

10. The light reflected from furrowed metallic mirrors directly in the principal diffraction-plane exhibits very nearly the same difference of phase as with smooth mirrors of the same material. The amplitude polarized parallel to the reflection- or principal diffraction-plane predominates still more over the amplitude polarized perpendicular to the plane of incidence than with unfurrowed metallic mirrors. The direct reflected light from furrowed metallic mirrors approaches nearer in its properties to that reflected from transparent substances than does that which is reflected from smooth unfurrowed metallic mirrors.

11. With gratings in other respects alike, the phenomena vary with the substance in which the diffraction takes place.

12. The secondary maxima, for which the theory does not account, exhibit the same remarkable behaviour towards polarized light as the maxima of the second class.

13. Furrow- and elevation-gratings of symmetrical form exhibit so nearly the same behaviour towards polarized light, if right and left are exchanged, that the phenomena may be regarded as identical.

For the explanation of these phenomena, I think it must be admitted that *difference of phase and ratio of amplitude of the light polarized parallel and perpendicular to the principal diffraction-plane depend on the diffraction-angle and also on the substance and the magnitude of the boundary between the heterogeneous parts of a grating upon which the unit of cross section of the incident light falls.*

This tells in favour of an influence of the molecules of the substance upon the oscillations of the æther particles, and the inadmissibility of Huyghens's principle at the margins of the apertures or furrows of a grating.

The theorems found by experiment for gratings with groups of apertures of like form, at equal distances from one another, must hold also for gratings with similar-formed groups of apertures at unequal distances from each other or for single groups of apertures, and even for heterogeneous particles distributed in a homogeneous base. Therein the distance and magnitude of these particles may be less than a wave-length.

Indeed polarized light exhibits the same behaviour towards single slits and furrows, or towards gratings with like-formed groups of apertures at unequal distances from one another, as, according to the searching investigations of Fizeau[*], it shows towards ordinary gratings. Further, similar are the phenomena of the polarization of the light of the sky which Arago[†], Babinet[‡], and Brewster[§] have observed, the polarization pointed out by Govi[||] and Tyndall[¶] in clouds of fine particles of dust and vapour, and the polarization of diffused light which occurs when diffraction is produced by very small heterogeneous particles distributed in water or other homogeneous transparent liquids or solids, as described particularly by Soret[**] and Lallemand[††].

All these experiments show that the light polarized parallel to the plane of diffraction may have greater, less, or the same intensity as that which is polarized perpendicular to the same plane, and consequently that the direction of the oscillations of the æther relative to the plane of polarization cannot be determined as the theories and theoretical considerations of Stokes[*], Holtzmann[†], Lorenz[‡], Lallemand[§], and Strutt[¶] have attempted to determine it, from the behaviour of diffracted light.

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[*] *Comptes Rendus de l'Acad. des Sci.* vol. lii. pp. 267, 1221 (1861).

[†] *Werke, deutsch von* Hankel, vol. vii. pp. 327, 359 (1824).

[‡] *Comptes Rendus*, vol. xi. p. 619 (1840).

[§] Ibid. vol. xx. p. 802 (1845), xxiii. p. 234 (1846).

[||] Ibid. vol. li. pp. 360, 669 (1860).

[¶] Phil. Trans. 1870, p. 348.

[**] *Arch. d. Sc. Phys.* vol. xxxv. p. 54 (1869), xxxvii. p. 148, xxxix. p. 1 (1870).

[††] *Comptes Rendus*, vol. lxix. pp. 189, 282, 917, 1294 (1869), lxx. p. 182 (1870), lxxv. p. 707 (1872).

XLVII. *On certain Early Logarithmic Tables.*

By Professor D. Bierens de Haan[**].

In reference to a very interesting Note of Mr. J. W. L. Glaisher's, I think I can give additional elucidation on some points. I have now before me the two works of Ezechiel de Decker, the first having the title:—

Eerste deel || van de nievwe || Telkonst, || inhovdende verscheyde || manieren van rekenen, waer || door seer licht konnen volbracht worden de Geo- || metrische ende Arithmetische questien. || Eerst ghevonden van Ioanne Nepero Heer || van Merchistoun, ende uyt het Latijn overgheset door || Adrianvm Vlack. || *Waer achter byygevoegt zijn eenige seer lichte manieren van Rekenen* || *tot den Coophandel dienstigh, leerende alle ghemeene Rekeninghen* || *sonder ghebrokens af veerdighen. Mitsgaders Nieuwe Tafels* || *van Interesten, noyt voor desen int licht ghegeven.* || Door Ezechiel de Decker, Rekenm^r. || Lantmeter, ende Liefhebber der Mathematische || kunst, residerende ter Goude. || *Noch is hier achter byghevoeght de* Thiende *van* || Symon Stevin *van Bruggke.* || Ter Govde, || By *Pieter Rammaseyn,* Boeck-verkooper inde corte || Groenendal, int Vergult A B C. 1626. || *Met Previlegie voor thien Iaren.*

Pages xii (not paged); A–Rr (page 1–308); a–q (128 pages, not paged); A–D (pages 1–27), quarto.

The first twelve pages contain, after the title, a copy of the privilege; a dedication to the States General, to the Councillors of Holland and West Frisia, to the Mayor and Sheriffs of the town of Gouda; a preface to the benevolent and scientific reader (Voor-reden tot den Goetwilligen ende Konstlievenden Leser); three sets of Latin verses by Patricius Sandaeus and Andreas Junius; the index (Register van alle de Hooftstucken); and the table of errata (de Druck-fauten salmen aldnus verbeteren).

The 39 following sheets, A–Rr (the sheet Cc has but four and gold), contain by J. Neper :—

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[*] Cambr. Trans. vol. ix. p. 35 (1851).

[†] Pogg. *Ann.* vol. xcix. p. 446 (1856).

[‡] Ibid. vol. cxi. p. 321 (1860).

[§] *Comptes Rendus,* vol. lxix. p. 190 (1869).

[¶] Phil. Mag. S. 4. vol. xli. p. 450 (1871).

[**] Communicated by the Author.

Eerste Boeck vande Tellingh door Roetjes, van het gebruyck der Telroetjes intgemeen (Rabdologiae liber I.) . pp. 1–40

Tweede Boeck vande Tellingh door Roetjes, van het gebruyck der Telroetjes in Meetdaden ende Werckdaden met behulp van Tafels (Rabdologiae liber II.) . . . . . pp. 41–88

Aenhangsel van het Veerdigh-ghereetschap van Menighvuldigingh (Appendix de expeditissimo Multiplicationis Promptuario) . . . . pp. 89–112

Van de Plaetselicke Telkunst (Arithmetica localis): pp. 113–148

And by Ez. de Decker,

Van Coopmans Rekeninghen (Mercantile Arithmetic): pp. 149–308

The 16 sheets that follow (a–q) contain Jaer- en Maent-Tafels van Interest teghen 5 (6, 7, 8, $7\frac{9}{13}$, $7\frac{1}{7}$, $6\frac{2}{5}$, $6\frac{1}{4}$) ten hondert. (Discount Tables.)

At the end we find a translation of Simon Stevin's la Disme in 27 pages; it has a title by itself (and is printed separately; at least I have a copy quite identical with it).

De || Thiende. || Leerende door || onghehoorde lichtigheyt alle re- || keninghen onder den Menschen noodigh val- || lende, afveerdighen door heele ghetal- || len, sonder ghebrokenen. || Door Simon Stevin || van Brugghe. || Ter Govde, || By Pieter Rammaseyn, Boeck- || vercooper, inde Corte Groenendal, int Duyts || Vergult A B C. || M.DC.XXVI.

De Decker, a surveying engineer, gave lessons in mathematics [Preface: "Terwyl ick inde Vermaerde stadt Gouda Professie doende van de Meetkonst ende Rekenkonst"]; but he did not understand the Latin language [Pref.: "Doch also ick inde Latijnsche sprake onervaren was"], and therefore had recourse to Adriaen Vlacq, who was at that time bookseller, it seems, under the firm of Pieter Rammaseyn; for in the copy of the Privilege, which is given to Vlacq himself ["conventeren en accorderen mits desen Adriaen Vlacq"] for ten years, it is decreed that in case of counterfeiting there be forfeited the sum of a hundred and fifty guilders, in order to give a third part to the officer that shall make the challenge, another third to the poor, and the last third to Adriaen Vlacq. This Vlacq, a young man of 26 years, who understood the Latin language, and was at the same time a good mathematician [Preface: "den konstlievenden Ionghman Adriaen Vlac, die hem doenmael met grooten yver inde Meetkonst oeffende"], translated for de Decker the 'Mirifici Logarithmorum Canonis Descriptio' of Joannis Neperi; but this seemed to take too high a flight for de Decker's purpose. He was better pleased with the translation of Joannis Neperi Rabdologia, which he gave in the volume just described. In the second volume he purposed to give the translation of Henrici Briggii Arithmetica Logarithmica and the Tables of Edmund Gunterus [Professor in Astronomie tot Londen]; but he cautions the reader to have some patience till that second volume be printed ["verwachten met patientie, tot dat het Tweede Deel voldruckt is"].

Now this second volume has never appeared; and by and by I will give a conjecture why not.

At the same time that the first volume appeared, at least with the same date [4 September int Jaer 1626] he gave another work with the title NIEVWE || TELKONST, || INHOVDENDE DE || LOGARITHMI VOOR DE GHE- || tallen beginnende van 1 tot 10000, ghemaeckt || van HENRICO BRIGGIO Professor || van de Geometrie tot Ocxfort. || MITSGADERS || *De Tafel van Hoeckmaten ende Raecklijnen door* || *het ghebruyck van Logarithmi, de Wortel zijnde van* || 10000,0000 *deelen, gemaeckt van* Edmund. Gun-|| tero, *Professor vande Astronomie tot Londen.* || Welcke ghetallen eerst ghevonden zijn van || JOANNE NEPERO Heer van Merchistoun: || *Ende* || *'t gebruyc daer van is met eenige Arithmeti-* || *sche, Geometrische ende Spherische Exempelen* || *cortelick aenghewesen,* || Door *Ezechiel de Decker,* Rekenmeester, ende || Lantmeter residerende ter Goude. || TER GOVDE, || By *Pieter Rammaseyn,* Boeck-verkooper inde || corte Groenendal, int vergult ABC. 1626. || *Met Privilegie voor thien Iaren.*

Pages viii (not paged); A–D (pages 1–51), A–M (169 pages not paged), A–F (91 pages not paged, in octavo). The first eight pages contain the title, the privilege (again to the name of Adriaen Vlack, as before), the preface, and three sets of verses by Andreas Junius (different from those in the first volume). In the preface de Decker rejects the logarithms of Neper, and adheres to those of Briggs ["om d'eerste Logarithmi te verwerpen, ende dese aen te nemen"]; and after speaking of his editing the second volume, that will become greater than the first one ["ende nadien dit grooter sal zijn als het Eerste Deel"], and therefore require some time for printing, announces that he is willing to give a Manual for some amateurs' sake that they may profit thereby in the mean time ["Maer alsoo eenige Liefhebbers verlanghen het gebruyck daer van te moghen sien, ende doch eenighe Handtboeckxkens van doen hebben, soo hebben wy dit doen Drucken"]. For this reason he gives the logarithms from 1 to 10,000, taken from the 'Arithmetica Logarithmica Henrici Briggij,' and a Table for Sines &c. of Edmund Gunter. The first Table occupies the sheets A to M in 169 pages, and has the Latin title:—

HENRICI BRIGGII || TABVLA || LOGARITHMORVM, || PRO NV-

MERIS AB VNITATE, || ad 10000. || GOVDAE, || Typis Petri Rammasenij. || M.DC.XXVI. (and not the Dutch one mentioned by Mr. Glaisher), and gives ten places of decimals with differences. The second Table occupies the last sheets, A–F, and has the Dutch title:—

EDMUNDI GVNTERI || TAFEL || Van Hoeck-maten ende || Raecklijnen, den Wortel zijnde van || 10000,0000 deelen.

These Tables are preceded (pages 1–51) by an Explanation [" Onderwijsingh hoemen de || Tafel HENRICI BRIGGII verstaen || sal om te ghebruycken "].

Now Adriaen Vlacq, being the printer of this smaller work, certainly was already occupied with the calculation of his Tables, that were published two years later (Gouda, 1628); and as these contained far more than the great work [groote werck] of de Decker promised or could contain, and as, in the second place, the last of the books before named was a manual that was very convenient for use, I think we might give these two reasons for the non-appearance of the second volume. Perhaps, also, logarithms being still nearly unknown, de Decker may have sold but few copies of his smaller work (at least it is now very scarce), and have thought it prudent not to commit his purse by editing the larger one. The not selling of this smaller work may have been the reason that a sufficient number of copies was sent to London, where they were added to a work of J. Wells, as Mr. Glaisher has shown.

I think I have shown that these small Tables were printed, and the English Latin text translated by Vlack, and that de Decker collected these works in his Nieuwe Telkonst; so that it may be surmised that Vlack, after having completed his own calculations and edited his 'Arithmetica Logarithmica' in 1628, regarded this, his great work, as completely eclipsing his first essays (which contained many errors), and so did not allude to them at all; for Vlack was not a boasting, but, on the contrary, a very modest man, as may appear in the title,

ARITHMETIQVE || LOGARITHMETIQVE || OV || LA CONSTRVCTION ET || VSAGE D'VNE TABLE CONTENANT || les Logarithmes de tous les Nombres de- || puis l'Vnité jusques à 100000, || ET || D'VNE AVTRE TABLE EN || laquelle sont comprins les Logarithmes des Sinus, || Tangentes & Secantes, de tous les Degrez & Minutes du quart du || Cercle, selon le Raid de 10,00000,00000. parties. || PAR LE MOYEN DESQUELLES ON RESOVLT TRES-FACI- || *lement les Problemes Arithmetiques & Geometriques.* || CES NOMBRES PREMIEREMENT || sont inventez par JEAN NEPER Baron de || Marchiston : Mais Henry Brigs Professeur de la || Geometrie en l'Vniversité d'Oxford, les a || changé, & leur Nature, Origine & || Vsage illustré selon l'inten- || tion du dit NEPER. || LA DESCRIP-

TION EST TRADVITE DV LATIN EN || *François, la première Table augmentée, & la seconde* || *composée par* Adriaen Vlacq. || DIEV NOVS A DONNÉ L'VSAGE DE LA VIE ET D'EN- || TENDEMENT, PLVS QV'IL N'A FAIT || PAR LE TEMPS PASSÉ. || A GOVDE, || Chez Pierre Rammasein. || M.DC.XXVIII. || *Avec Privilege des Estats Generaux.*

Pages viii. (not paged) contain the title, Preface au Lecteur and Fautes à corriger (which is generally missing); sheets a–g (pages 1–84) contain thirty chapters of explanation; sheets A–Kkk (720 pages not paged) contain Tafel der Logarithmi voor de ghetallen van 1 af tot 100000 (this title, not the French one in the copy at the British Museum, is that occurring in Miller's edition); sheets Lll–Sss (96 pages not paged) contain Canon Triangulorum sive tabula artificialium Sinuum, Tangentium et Secantium, Ad Radium 10,00000,00000, et ad singula scrupula Primi Quadrantis. It should be noticed that every sheet contains 12 pages.

My copy, as appears from a notice on one of the fly-leaves, was changed on March 26, 1667, for the second volume of de Decker's 'Nieuwe Telkonst;' so that it follows again that this latter work was very scarce.

Postscript.—I have found a further proof of the connexion between Vlack and de Decker not having been loosened by the former publishing his 'Arithmetica Logarithmica,' and thereby forcing the latter to suppress his "great work," the second volume of the 'Nieuwe Telkonst.'

The same de Decker gave, some years later, a treatise on Navigation:—

PRACTYCK || vande Groote || ZEE-VAERT: || beschreven door || Ezechiel *de* Decker, *Reker-mr.* || *ende Lant-meter, residerende* || *tot* Rotterdam. || TER GOVDE. By Pieter Rammazeyn, 1631, in octavo (small).

This work contains sheets A–D (the title, dedication, preface, and 60 pages not paged of introductory matter ["Verklaringhen vande Tafels sinuum, tangentium ende secantium; Handelingh vande platte driehoecken; Handelingh van de spherische dryehoecken]"); sheets 𝔄–𝔉 "Canones Sinuum, Tangentium et Secantium. Ofte Tafel van Hoeck-maten, Raeck-lijnen, ende Snijlijnen, den Radius zijnde 100000" (92 pages not paged; the tables proceed by 1′ and give 10 decimal-places; as to the logarithms, there is mentioned at the last page the Nieuwe Telkonst in octavo); sheets C–D*d* contain some explanations, and then Chapters I. to IX. of the Navigation (pages 1–304, where the last page bears the erroneous superscription "Achste Hooftstuck"); finally the Almanachs for 1631 to 1640 (pages 305–314), an Appendix [By-Hangsel] with nine Astronomical quespaged).

Of this work there exists a second, enlarged edition, in 4to, printed at Rotterdam, 1659.

This first edition being printed in 1631 by Vlack himself, the author residing then at Rotterdam, refutes, I think, the idea of Vlack and de Decker having quarrelled; neither is the assumption of a quarrel, as I think I have shown, necessary to explain the non-appearance of the "Groote werck," or the fact that Vlack does not mention de Decker, as the parts of the two volumes of the Nieuwe Telkonst that were translated from Neper, were Vlack's own work and not de Decker's.

Leyden, March 3, 1873.

XLVIII. *On Early Logarithmic Tables, and their Calculators.* *By* J. W. L. Glaisher, *B.A., Fellow of Trinity College, Cambridge*[*].

THERE are a few remarks in connexion with Prof. de Haan's paper (which he was kind enough to submit to me previously to publication) that it will be convenient to make in immediate juxtaposition with it. And, first, in reference to the paper itself I may recall to mind the circumstances that gave rise to it. My former communications, October and December (Supplement) 1872, contained descriptions of two works which had apparently dropped completely out of notice in regard to the history of logarithms, viz. De Decker's *Nieuwe Telkonst* and *Eerste Deel van de nieuwe Telkonst* (full titles given above) published at Gouda in 1626. The former of these contains logarithms; and in the prefaces of both are acknowledgments of the services rendered by Vlacq in their preparation. The most curious point, however, was, that in both there are allusions to a much larger work on logarithms, for which the reader was directed to wait patiently, using the *Nieuwe Telkonst* merely as a makeshift till its appearance. Now the only book to which these promises (supposing them to have been fulfilled) could apply was the *Arithmetica Logarithmica* of 1628, which, as is well known, was published by Vlacq alone, and contains not the slightest allusion to De Decker, or to any previous publication of logarithms at Gouda. The difficulty, therefore, is to account for Vlacq's total silence with regard to a work in the preparation of which he had had a share, and which contained an announcement either of the *Arithmetica* itself, or of some abandoned project which had been superseded by it. In either case the omission was equally remarkable; and I expressed an opinion "that Vlacq must have quarrelled with Decker, or for some other cause have had a set purpose to ignore his book entirely." The hypothesis of the quarrel rested on no other evidence except such as the foregoing facts afforded; and I am very glad Prof. de Haan, by showing that friendly relations existed between Vlacq and De Decker in 1631, has proved it to be untenable. He considers the smaller tables were printed by Vlacq, and merely collected by De Decker, and attributes the silence of the former in the *Arithmetica* to the fact that, in comparison with this work, he was rather ashamed of his earlier performances; while De Decker did not keep his word, because Vlacq occupied the ground he had intended to cover. The explanation is not very satisfactory (as, whatever the circumstances may have been, De Decker and the promises should not have been ignored); but it is perhaps the best that will ever be obtained. Prof. de Haan apparently grounds his statement that Vlacq was a bookseller under the firm of Pieter Rammaseyn on the fact that the privileges are made out to him*. At the time of writing my papers I was surprised, though I find that I have not alluded to the matter, at Vlacq's name being mentioned in both the privileges (while neither De Decker's nor Rammaseyn's appear at all in that of the *Nieuwe Telkonst*, where De Decker's alone appears on the titlepage); and I do not even now see the reason very clearly. With regard to the date when Vlacq became a bookseller, his own words are:—"Ignosce quæso, Benevole Lector, si nimis prolixus sum in narranda historiuncula vitæ meæ quam abhinc 26 annis institui: ex eo enim tempore, nescio quo fato, in librorum commercium incidi. In Hollandia, quæ mea patria est, tum temporis vivens, ex Anglia acceperam insignis Mathematici Henrici Briggii *Arithmeticam Logarithmicam*, Opus sanè aureum, et omnium inventionum in artibus Mathematicis meo judicio excellentissimam; in quo erant Logarithmi pro numeris ab unitate ad 20000 et a 90000 ad 100000. Cumque viderem Logarithmos pro numeris à 20000 ad 90000 ibi desiderari, animus mihi erat lacunam istam explere, quod et incredibili temporis brevitate solus peregi, ita ut D. Briggius aliique Mathematici in Anglia admirati fuerint, quod non solùm laborem istum, sed et operis promulgationem tam brevi tempore absolverim . . . . . . Cum libros istos imprimi curabam, Typographiam nondum habebam, neq; Bibliopola eram, sed esse cogebar, ut exemplaria excusa distribuerem; istaq; de occasione primum in Galliam, deinde etiam in Angliam profectus sum, ubi me rem meam bene acturum putabam" (*Joannis Miltoni Defensio secunda. . . . . Hag. Com.* 1654. *Typographus pro se-ipso*). These words, taken literally, imply that Vlacq was not connected with the bookselling trade until he began to print the *Arithmetica*, which must have been subsequently to 1626; but I do not think that great importance is to be attached to their exact meaning, as Vlacq here, as in the *Arithmetica*, ignores his early work performed in conjunction with De Decker; and as we know that the *Arithmetica* itself was also printed by Rammaseyn. Thus most likely either Vlacq had nothing to do with printing or bookselling till after the publication of this work in 1628, or he was connected with the trade in some way or other through Rammaseyn from the date of his (or De Decker's) earliest logarithmic work in 1626. The latter view is that adopted by Prof. de Haan, and is very likely correct; but the simple meaning of Vlacq's own words agrees better, I think, with the former. It is proper to state here that a list of names (with dates and places) of editors of logarithmic tables appended to a tract of Prof. de Haan's, "Iets over Logarithmentafels"[*], contains that of De Decker (Gouda, 1626); but this is, as far as I know, the only place in which any allusion is made to the work. It should also be added that the above paper on De Decker's works was written by Prof. de Haan before he had seen my second communication, "Supplementary Remarks, &c." (Phil. Mag. Dec. Suppl. 1872), which accounts for the partial recapitulation of the translation of a portion of the preface of the *Erste Deel van de Nieuwe Telkonst*. In regard to the life of Vlacq, Mr. W. Barrett Davis drew my attention to the following petition, printed in the Calendar of State Papers, Domestic Series, 1637:—

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[*] Communicated by the Author.

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* Since writing the above I have received a letter from Prof. de Haan, in which, in answer to my inquiry, he says, "I conclude that Vlacq was a bookseller in 1626 from the privilege having been given to him, although he was not the author of the work named on the titlepage. In general these privileges were granted with us to the booksellers themselves.

"Petition of Richard Whitacres to the same [Archbishop Laud]. One Hooganhuysen, a Dutchman, being heretofore complained of in the High Commission for importing books printed beyond the seas, was bound not to bring in any more. One Vlack has kept up the same agency, and sold books in his stead, and is lurking here, observing what is most useful and vendible, and causes it forthwith to be printed abroad. Petitioner having lately brought from Frankfort mart to Rotterdam four great vats of books to the value of £500, Hooganhuysen, upon untrue suggestions, caused them to be seized and sold to Vlack for £100. Vlack is now preparing to go beyond seas to avoid answering his late bringing over nine bales of books contrary to the decree of the Star Chamber, and procures some persons to pretend that he is indebted to them (as formerly Hooganhuysen did), thereby to get the books into their possession. Petitioner prays order to bring the bales to Stationers' Hall, there to remain till Vlack shall re-deliver to him the said four vats of books, or at least at the same price he bought them. Reference to Sir John Lambe to take three Commissioners' hands, and by warrant bring the books above-mentioned to Stationers' Hall, till the cause may be heard. 13th November, 1637" (Vol. ccclxxi. p. 94).

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[*] Amsterdam, 1862, reprinted from the *Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeeling Natuurkunde*, Deel xiv. This pamphlet, which I had not met with till Prof. de Haan recently sent me a copy, contains far the most complete list of writers on logarithms or publishers of logarithmic tables that I know of.

This series of the Calendar of State Papers is not as yet published beyond the beginning of 1639, so that it is not certain whether Vlacq's name will occur again; but we know, from his own account (Phil. Mag. Oct. 1872), that a compromise was effected. The case of Hooganhuysen (David van Hooganhosen as he generally appears) was repeatedly before the court for more than a year. He originally had James Bleau as his co-defendant; but the latter was discharged on June 26, 1634, and judgment against the former given on June 15, 1635, in the following words:—"Considering the ill-consequence and scandal that would arise by strangers importing and venting in this kingdom books printed beyond seas, it was ordered that Hooganhosen should not bring over or sell Mercator's Atlas or Atlas Major in English, and if such be brought over by any one they are to be seized." He was also subsequently (July 7, 1635) ordered to carry out his contract with regard to the delivery of Amesius upon the Psalms and other works to certain booksellers. It will be seen how well the quotation about Vlacq agrees with his own account. He was about ten years in London, and finally left on the breaking out of the civil war; so that he spent the years from 1632–1642 in England*, the first few very peaceably, but afterwards he was treated as he describes. The petition of Richard Whitacres was very likely the commencement of hostilities.

Of the books which Vlacq names as having been printed by himself between 1642 and 1648 at Paris, I have not been successful in seeing a copy that satisfies these conditions; but I have met with an edition of one of them, viz. *Hugonis Grotii de Imperio summarum Potestatum circa sacra Commentarius,* editio quarta . . . . printed by Vlacq at the Hague in 1651; the *De Jure Plebis* of David Blondel is added, also printed by Vlacq, with date 1652.

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* I take this opportunity of correcting a mistake of a name in my first communication (Oct. 1872). Dr. Johnston should be the well-known Dr. Juxon, who became Bishop of London in 1633, and as Archbishop of Canterbury in 1660 crowned Charles II. The Latin is *Joxstonius*, which, considering the *x* a misprint for *n* (a not uncommon interchange), I rendered Johnston. Vlacq, writing after an interval of fifteen years, must have only partially remembered the name.

In the Calendar of State Papers (Domestic Series), 1600–1638, Briggs's name occurs several times. He was a Commissioner of Sewers for Norfolk, Suffolk, Cambridgeshire, &c., and in conjunction with Sir Anthony Thomas, John Worsopp, Hildebrand Prusen, and others was an undertaker for draining the fens (Dec. 2, 1629). When in February 1625 the tides overthrew 1120 rods of bank in the neighbourhood of Yarmouth, Briggs was consulted with regard to the levels. He was also a member of a commission to effect the removal of several houses in Oxford; and his death is reported by the Vice-Chancellor (Jan. 27, 1631) as having occurred the previous day.

Under date Nov. 12, 1630, there is, in the handwriting of Archbishop Laud, a list of Master Printers of London, with a sum placed against each, headed "To S. Paul's" (viz. to the repair of St. Paul's). The Calendar states that the sums assessed run from £6 to George Miller, to £40 to William Jones. This is the only mention I have anywhere seen of the George Miller who printed the English (1631) edition of Vlacq (except the *tables* themselves, which were all printed at Gouda); William Jones was the printer of Briggs's *Arithmetica* of 1624.

In more than one place I have remarked that a good many of the English (Miller) copies of the *Arithmetica* contain the titlepages to the Tables in Dutch, and have inferred therefrom that Vlacq either did publish, or meditated the publication of a Dutch edition of his work (that is to say, of the Introduction to it; the tabular portion is the same in all editions); and this view is confirmed by the following extract from a letter of Briggs to Pell (MS. Birch, orig. 3495, now made 3498*). "My desire was to have those Chiliades that are wantinge betwixt 20 and 90 calculated and printed, and I had done them all almost by my selfe, and by some frendes whom my rules had sufficiently informed, and by agreement the busines was conveniently parted amongst us; but I am eased of that charge and care by one Adrian Vlacque, an Hollander, who hathe done all the whole hundred chiliades and printed them in Latin, Dutche, and Frenche, 1000 bookes in these 3 languages, and hathe sould them almost all. But he hathe cutt of 4 of my figures throughout; and hathe left out my Dedication, and to the reader, and two chapters the 12 and 13, in the rest he hathe not varied from me at all." (The whole letter is printed in the 'Letters on Scientific Subjects' published by the Historical Society of Science in 1841, under the editorship of Mr. Halliwell.)

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* The letter is addressed "To his very good and muche respected frende Mr. John Pell at Trinitie Coll. in Cambridge," and is sealed with what was, I suppose, Briggs's seal. The device may be thus described:—Draw a parabola with axis vertical upon a horizontal line terminated both ways by the curve as base. Divide this base into seven equal parts, and erect six ordinates at the points of section, terminated by the curve. Join one extremity of the base to the top of the third ordinate from the other extremity, and the top of the first ordinate to the top of the second ordinate from the other extremity, and render the figure symmetrical by joining the other corresponding points. Then, supposing these four lines passed through a point (in point of fact they do not), we should have a representation of the device, the motto under which is "IN VNVM." What the proposition referred to is, I do not know. I at first thought it was as described above, but found on investigation that, although in a drawing the lines pass well enough through a point, they do not really do so.

I wrote and asked Prof. de Haan if he knew of, or could find, any Dutch copy in the Libraries at Leyden; and he has informed me he has not been able to meet with any trace of any such having appeared. I think it likely that the copies originally intended for the Dutch edition were, when it appeared that there was little demand for them in Holland, sent to London to be appended to an English introduction, and sold over here. Whether George Miller bought the copies, or merely printed the Introduction for Vlacq is uncertain; but the latter supposition is in accord with Vlacq's having come to London in 1632 (the year of publication) as a bookseller.

No one can fail to notice the total absence of any feeling of irritation on Briggs's part against Vlacq for having anticipated him in the performance of a work he had so much at heart, and which he had nearly performed himself. This is in perfect agreement with every thing else that is known of Briggs; and there is no reason to doubt that he stated the simple truth when he closed his preface to Wright's translation of the *Canon Mirificus* with the words, "I ever rest a lover of all them that love the Mathematickes." Who Briggs's friends were who were helping him with the calculation can only be conjectured. Very likely Gunter may have been one; and there is reason to think that J. Welles, of Deptford, the author of the *Sciographia*, was another; for in a letter from the latter to Briggs, dated January 9, 1621, and printed in the 'Correspondence of Scientific Men of the Seventeenth Century, in the Collection of the Earl of Macclesfield,' 1841–1862, he speaks about the calculation of logarithms in a manner which implies that he was assisting Briggs in the computations needed for the *Arithmetica*, 1624. In reference to this work it is worth while to quote a sentence that occurs in a letter from Collins to Wallis under date Feb. 2, $166\frac{6}{7}$ (in the same collection):—"Mr. Briggs's *Arithmetica Logarith-*

mica being too numerous an impression, has been tendered about the streets at 1*s*. 6*d*. each. The like I say of Mr. Barrow's Euclid. Mr. Sutton and myself, as Mr. Marke well knows, have bought divers of them at 1*s*. a book, in quires." There is very little about the calculators of logarithms or their works in the Macclesfield collection; but it appears that in 1673 Flamsteed borrowed a copy of Vlacq's *Trigonometria Artificialis* from Collins. In a letter to Thomas Lydyat (July 11, 1623) printed in the Letters of the Historical Society of Science, Briggs speaks of being engaged on his logarithms; but it is sufficiently evident from other considerations that he must have been so employed at the time.

Birch, 4407, contains a MS. entitled "Imitatio Nepeirea. Sive Applicatio omnium (fere) regularum suis Logarithmis pertinentium ad Logarithmos M^ri^ Brigges. Ex propriâ* descriptione sui Canonis Mirifici. Impressâ Edinburgi Anno 1614." The tract, which is not in Briggs's handwriting, I should imagine, from internal evidence (though I have made no very careful examination of its contents), to have been written about 1623. It seems to be (as its name implies) a sort of parody, with Briggian logarithms, of Napier's *Canon Mirificus*; but does not appear to possess any particular value or interest.

April 16, 1873.

XLIX. *Notices respecting New Books.*

Celestial Objects for Common Telescopes. By the Rev. T. W. Webb. London: Longmans.

Report presented to the Board of Visitors of the Royal Observatory, Edinburgh. By C. Piazzi Smyth, *Astronomer Royal for Scotland.*

THE appearance of a third edition of the valuable epitome of practical amateur astronomy first mentioned is a strong evidence of the healthy condition of the science, considered apart from the ordinary work of established observatories. It is now some thirty years since an impetus was given to the labours of the amateur by the publication of the late Admiral Smyth's 'Celestial Cycle;' and there can be no doubt that the progress made in a knowledge of double and binary stars between its publication and the year 1859 is mainly attributable to the 'Bedford Catalogue,' which formed the second part of the 'Cycle,' and for which the author received the gold medal of the Royal Astronomical Society. Originally dedicated to Admiral Smyth, the work which forms the subject of this notice, in addition to most valuable information relative to the sun, planets, and comets, embodies in a condensed form the most important characteristics of double stars, clusters, and nebulæ; and we have only to refer to the edition which has just appeared to become acquainted with the fact that this condensed catalogue has been brought up to the date of publication by the industrious author, and thus supplies the hiatus created by the 'Bedford Catalogue' being out of print; indeed the 'Bedford' is now becoming a record of the past, exceedingly valuable as showing the state of this branch of astronomy in 1844, but inadequate to the wants of the working amateur in 1873.

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* The word which I take to be *propriâ* is a correction in a later hand of that originally written, which was perhaps *ejus*.

The first edition of the 'Celestial Objects' was marked by a feature almost unique, the only parallel being Tables of Lunar objects (mostly from Schröter) in the late Sir David Brewster's Supplementary Chapters to Ferguson's 'Astronomy,' published in 1821, accompanied by a map of the moon by Mayer, a predecessor of Schröter. Mr. Webb embellished his first edition with an admirable lunar map from Beer and Mädler's *Mappa Selenographica*, which for clearness of detail and facility of reference can hardly be surpassed, and gave the names of the 404 objects which are found on the German map, accompanied with short descriptive notices of the most important. It was the publication of this map and catalogue which drew that attention to the study of the moon's surface which has resulted in a revised impression of the map, containing ninety additional numbers referring to as many new names in the list. An appendix includes the latest information bearing on telescopes, observations of the Sun, Venus, the Moon, Jupiter, Comets, and the so-called fixed stars.

We apprehend that no amateur astronomer would willingly be without this valuable compendium of his science.

Turning from Amateur to Government work, we regret to find, from the Report of the Astronomer Royal for Scotland, that nothing additional is to be given to the Royal Observatory, Edinburgh. It appears that an extensive task is projected—that of forming a general catalogue of Edinburgh stars from the earliest days of the activity of the Observatory. The personal staff consists of the Astronomer Royal for Scotland and two assistants only, the daily duties being the computation of meteorological observations from fifty-five stations of the Meteorological Society of Scotland, the observation of stars for time, and its distribution electrically by time-ball, time-gun, and controlled clocks. A new time-gun has been established in Dundee, and a new controlled clock added to the primary series of the Observatory, the gift to the Edinburgh University of a private gentleman of liberal mind and intelligent interest in science. It is principally in connexion with the new equatorial, now nearly complete, that the Astronomer Royal for Scotland deplores the want of further aid from Government. Speaking of the pressing difficulties arising from want of funds, he says that the determination of Government places him in the position of an unfortunate artillery officer who should have received a big gun, of perhaps the most approved wrought iron and steel construction in itself, but without the means of moving it, without powder and shot, and yet should be expected by the public to be continually firing it with immense success at all sorts of objects throughout the whole year.

L. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311.]

March 6, 1873.—Sir George Biddell Airy, K.C.B., President, in the Chair.

THE following communication was read:—

"On the Vapour-density of Potassium."—Preliminary Notice. By James Dewar and William Dittmar.

Since the elaborate experiments of Deville and Troost on the vapour-densities of substances at high temperatures, little has been added to chemical science in this field of research. Doubtless this is in great part owing to the difficulty of any *one* student manipulating the complex apparatus necessary for the execution of the experiments. But the operations are greatly increased in difficulty when we select bodies that are readily inflammable in air and attack with facility glass and porcelain at the high temperatures to which they are exposed. This is the reason why the molecular weights of a most important class of elementary bodies, viz. the *alkali-metals* (although these are volatile at moderate temperatures), have remained to the present time undetermined. It was with the view of adding something to our knowledge in this department, that we recently undertook some experiments with potassium, the results of which we now beg leave to lay before the Society. The special difficulties we had to overcome are involved in the endeavour to answer the following questions:—

1. Is it possible to convert potassium into a gas of one atmosphere's pressure at any of the *constant* temperatures we can at present command?

2. Is it possible to generate *pure* potassium-vapour and to keep it from getting oxidized?

3. Supposing a definite volume of such vapour to have been procured, how can its *weight* be ascertained?

After a succession of failures, which we shall not detail, we at last succeeded in devising a workable process, which may be briefly described as follows:—

A cylindrical iron bottle of at least 200 cub. centims. capacity, of a thickness in the body ensuring sufficient rigidity at even a bright red heat, and provided with a well-ground inbent neck, pierced with a canal of about 2 millims. diameter, is employed as a generator and receptacle of the vapour.

A mass of about 20 kilogrs. of zinc contained in a plumbago crucible, which being placed in a forge-fire can be readily heated up to the boiling-point, serves as a bath.

The experiment begins by first deoxidizing the inside of the receptacle at a red heat by means of a current of dry hydrogen, which is continuously maintained until the bottle has cooled down below redness. At this stage about 200 grms. of pure mercury are introduced into the bottle, which is then inserted into the red-hot zinc, without, however, covering the upper extremity of the bottle. After ¾ of the mercury is distilled off (which is accomplished in a very short time), the neck is withdrawn; and while the mercury-vapours are still streaming out, an iron test-tube, previously prepared with great care and charged with 4–5 grms. of potassium, is dropped into the bottle, the neck reinserted, and, after the *whole* of the bottle has been immersed in the zinc, the blast of the forge is forcibly increased so as, in the shortest possible time, to bring the zinc into the state of boiling, proper arrangements being made for keeping the neck of the bottle red-hot. The potassium in a short time begins to volatilize, issuing in jets into the air and depositing caustic potash at the nozzle, which must be kept clear by means of an iron wire. As soon as the distillation of the potassium ceases, the nozzle is closed by means of a ground-in wire plug, at once immersed into a mass of mercury contained in a test-tube, and the bottle withdrawn to a proper support, on which it is allowed to cool.

After it has reached a manageable temperature, the bottle is inserted into a mass of recently boiled water, the wire plug withdrawn, and the hydrogen formed by the action of the water on the potassium pumped out, by means of a "Sprengel," into a eudiometer, to be measured.

In the experiments we have hitherto carried out, we have satisfied ourselves that the amount of mercury-vapour *not* swept out by the potassium is quite inappreciable; and as our object has been in the mean time to merely arrive at approximate results and to perfect our methods of manipulation, we have neglected the minute correction which, on account of that small remnant of mercury, ought, strictly speaking, to have been applied to the volume of the vapour as calculated from the capacity of the bottle in the cold, the coefficient of expansion of iron, and the temperature (1040° Deville) at which the vapour was measured.

The results of our observations conclusively show that the density of potassium-vapour, as produced in the process described, cannot exceed 45 times that of hydrogen, and that therefore the molecule of potassium consists of *two atoms* (K_2).

We intend to prosecute our research in other directions, proposing to ascertain, if possible, the densities of the *iodides* of cæsium, rubidium, and potassium, these being, according to Bunsen's experiments, the most volatile of the haloids of the alkali-metals.

March 13.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

The following communication was read:—

"Note on Supersaturated Saline Solutions." By Charles Tomlinson, F.R.S.

In the year 1866, M. Gernez and M. Viollette published each a memoir on supersaturated saline solutions*, in which the same conclusions are arrived at, namely: (1) that the only nucleus capable of suddenly crystallizing any one of such solutions is a salt of the same kind as that dissolved; and (2) that all bodies, solid, liquid, or aëriform, which apparently act as nuclei, are really contaminated with a hydrate of the salt that forms the supersaturated solution.

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* Annales Scientifiques de l'École Normale Supérieure, tome 3e, Année 1866, pp. 167 and 205. I am indebted for this reference to the courtesy of M. l'Abbé Moigno.

I cannot refrain from expressing my admiration at the unwearied skill and patience with which these two memoirs were prepared. The experiments were repeated by hundreds, and under a large variety of circumstances, so that it seems scarcely possible to entertain any doubt as to the validity of the conclusions arrived at. I had not seen these memoirs until long after the publication of my second paper on this subject*; or I should have hesitated in offering it to the Royal Society without special reference to them. What I did see was a very brief abstract of M. Gernez's memoir in the 'Comptes Rendus;' and to this I refer in my first paper†, quoting the experiments and the decisive objections of M. Jeannel in opposition to M. Gernez's conclusions.

The experiments of Mr. Liversidge‡ are identical in principle with those of M. Viollette, and some of them mere variations, such as the proof that bodies greedy of water and capable of being hydrated do not produce crystallization; only M. Viollette made use of calcined sulphate of copper instead of calcic chloride &c.§ Moreover no fresh proofs are wanted as to the non-nuclear action of the modified salt or of the anhydrous salt on supersaturated solutions of Glauber's salt.

Recently MM. Gernez‖ and Viollette¶ have each, on the occasion of the publication of my third paper (namely, that written in conjunction with M. Van der Mensbrugghe**), called in question the integrity of my experiments, M. Gernez insisting that the oils and other liquids employed by me contain salts of the same kind as those of the solutions they apparently acted on.

Such an assertion as this seems to me to be very difficult of proof; and it seems equally bold to assume that the air of an open garden in the country contains salts of various kinds, watching their opportunity to get into my flasks and vitiate the results of my experiments.

I desire to invite special attention to a statement made in my second paper—the more so because, without desiring for a moment to call in question the negative results obtained by MM. Gernez and Viollette, namely that pure oils &c., as used by them, whether in the form of films or globules, do not cause the solutions to crystallize, the method indicated opens a new process for ascertaining whether there is really any other nucleus except a salt of the same kind. The statement referred to is as follows:—

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* Phil. Trans. 1871, p. 51.

† Phil. Trans. 1868, p. 660. The reference to M. Jeannel is in the abstract of this paper contained in the 'Proceedings of the Royal Society,' May 28, 1868, p. 405.

‡ Proceedings of the Royal Society, vol. xx. p. 497.

§ On the surface-tension theory, it is not inaccurate to say that absolute alcohol robs the solution of water, and so determines crystallization, since the mixture of alcohol and water degrades the tension of the solution.

‖ Comptes Rendus, vol. lxxv. p. 1705.

¶ *Ibid.* vol. lxxvi. p. 171.

** Proceedings of the Royal Society, vol. xx. p. 342.

"A solution of two parts of Glauber's salt to one part of water was boiled and filtered into three flasks, which were covered with watch-glasses and left until the next day. A drop of castor-oil was then placed upon the surface of each: it formed a lens which gradually flattened; but there was no separation of salt, even when the flasks were shaken so as to break up the oil into small globules. . . . . If, while the flask is being turned round, a sudden jerk be given to it, so as to flatten some of the globules against the side into films, the whole solution instantly becomes solid" [*].

I desire on the present occasion to describe a number of experiments in which this process has been observed. But anticipating the objection that the various oils that have been long on my shelves, and already used in former experiments, may really contain various kinds of saline nuclei, I procured from a wholesale house a number of fresh specimens of oil. These were the oils of lavender, bergamot, cajuput, sesame, rape, almonds, olive, and sperm. I did not allow these oils once to enter my laboratory, but kept them in my library, and never opened the phials except in the open air.

The flasks and dropping-tubes were washed in strong sulphuric acid, and rinsed in tap-water, the tubes being kept immersed in clean water in the open air. The solutions were filtered into the flasks; and these, being covered with small beakers, were reboiled until steam issued from the orifice.

The flasks were then taken into my garden, and when cold an oil was dropped upon the surface of each solution. If the oil formed a well-shaped lens, there was no separation of salt; and in many cases, instead of restoring the small beaker to its place, a well-fitting cork was driven into the neck, and the flasks were thus left for a time, on some occasions extending to the next day.

In order to avoid the objection that during the shaking crystallization might be produced by some of the solution splashing against the cork, many of these experiments were repeated in a pear-shaped flask, nearly 12 inches in height, and containing about 2 or 3 ounces of the solution. Most of the experiments were tried in globular flasks of 5 ounces capacity, with straight cylindrical necks 4 inches high.

Experiment 1. Sodic sulphate—1 salt, 1 water. Solution in tall flask covered with a small beaker. Next day oil of sweet almonds was dropped into it, and the flask corked. After about an hour a circular motion was given to the flask, so as to disperse the oil into minute globules through the solution, giving it the appearance of an emulsion. The flask was left at rest during about an hour, then suddenly shaken, so as to rattle the solution against the side, when all at once, as if with a flash, it became solid.

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[*] Phil. Trans. 1871, p. 55.

sulphate hydrate was derived from one of the following sources:—(1) from the air, (2) from the oil, (3) from the cork, or (4) from the side of the flask. (1) It could not be derived from the air, either of the laboratory or of the garden, because the solution remained liquid long after the flask had been corked. (2) It could not have been derived from the oil, because this was dispersed through the solution in myriads of globules, without any nuclear action, and the flask was left to repose for an hour after the oil had been so dispersed. (3) Nor could any nucleus have been derived from the cork, because the solution never touched it. Nor could a minute speck of the sodic sulphate hydrate have fallen from the cork; for the latter had been put into hot water, out of which it was taken the moment it was put into the flask. It could not have been derived from any of the hot water from the cork streaming down the side of the flask to the solution, because sodic sulphate in solution is in the non-nuclear anhydrous state, and also because two hours had elapsed between the corking of the flask and the solidification of the solution. (4) A crystal could not have been derived from the walls of the flask, because the solution had been briskly boiled in it, so that steam escaped with considerable force from the neck after the small beaker had been put on. Hence I am compelled to fall back on my former statement [*]—namely, that the oil acted as a nucleus by the flattening of one or more of the globules against the wall of the flask into the form of film.

A globular flask, containing the same solution and the same oil, was corked, and the oil dispersed in globules. It was left some hours, with occasional shaking, so as to rattle the solution against the side. The rattling motion was purposely less energetic than in the former case, to avoid splashing against the cork. This solution crystallized after it had been left a short time at rest; and it did so in large flat crystals, described in my second paper, very different from the minute radial action noticed when crystallization sets in from a point [†].

A similar flask, containing the same solution, was treated with oil of bergamot, a drop of which was allowed to trickle down the side of the flask. As soon as the oil touched the solution, crystallization set in, the radiant-point coinciding with such point of contact, and the whole surface was covered with fine lines diverging from this point alone.

In another case a drop of the oil of rosemary was deposited on the centre of the solution. On shaking the flask, the solution assumed the solid state.

Two flasks, containing the same solution, crystallized under the influence of oil of lavender.

Five flasks, with the same solution, crystallized under the action of sperm-oil.

Experiment 2. Sodic sulphate—2 salt, 1 water. In three flasks the solution crystallized on the addition of olive-oil, the first immediately, and the other two after being shaken.

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[*] Phil. Trans. 1871, p. 52 *et seq.*

[†] *Ibid.* p. 54.

Experiment 3. Sodic sulphate—3 salt, 1 water. This solution crystallized under the action of the oils of bergamot, almonds, colza, rape, and sesame.

Experiment 4. Sodic sulphate—3 salt, 1½ water, in six flasks, all of which crystallized under the action of sesame oil.

Experiment 5. Potash-alum—8 salt, 3 water. The solution crystallized suddenly, as with a flash, on being shaken up with oil of olives. This is the more remarkable, as the usual action of a nucleus is to produce a solitary octahedron, which grows rapidly until the solution becomes solid. This last effect was produced by a less violent shaking of the solution in contact with oil of lavender.

Experiment 6. Ammonia-alum—8 salt, 7 water. On shaking the solution with olive-oil it suddenly became solid, and of an opaque chalky white.

This selection from a large number of experiments may be sufficient to answer my present purpose—namely, to show that some supersaturated saline solutions really do crystallize under the action of other nuclei than a salt of the same kind as that of the solution operated on.

My experiments were performed in the open air, at temperatures between 30° and 50° F. At comparatively low temperatures the shaking of the flask containing the solution sometimes produced a copious liberation of anhydrous salt, which rapidly combines with water and forms the 7-atom hydrate*; but on allowing the flask to rest for some time, it generally happens that the solution becomes solid, and the 7-atom salt opaque white.

I may perhaps be allowed to state that my experiments, conducted as they were in the open air, were delayed by the rainy weather of last year. There is, however, this advantage in wet weather, that the saline particles said to exist in the air are washed down and brought into solution, in which condition they are not nuclear, as I have already shown in the case of sodic sulphate, alum, and one or two other salts*. On the other hand, fine weather has its advantages; not only are the surfaces of the solutions more active and the evaporative force stronger, but hydrated salts, said to exist in the air, part with their water of crystallization so readily as to reduce them to the non-nuclear condition. This is especially the case with sodic sulphate. I have already shown that supersaturated solutions of this salt may be exposed to the air, both in fine and wet weather, for a long time without crystallizing †, as well as the fact just noticed ‡, that a solution of a salt does not act as a nucleus to its supersaturated solution. Moreover, if sodic sulphate exist in the dusty air of a room, it cannot retain the hydrated form during many minutes, but must rapidly pass into the anhydrous, in which it is no longer a nucleus.

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* The view adopted by me that the supersaturated solution of sodic sulphate contains the anhydrous salt in solution, and that this is first thrown down on lowering the temperature, agitating, &c., has been objected to, on the ground that, according to Löwel, it is the 7-atom hydrate that is really in solution and is deposited on cooling. There are numerous proofs that it is the anhydrous salt which is really in solution; these I have collected in two papers, contained in the 'Chemical News' of 3rd and 10th December 1869. That the 7-atom hydrate is built up on the anhydrous salt may, I think, be shown by an experiment. Two flasks containing a solidified solution of sodic sulphate of the same strength are heated over a spirit-lamp; one of the flasks is constantly turned round on the ring of the retort-stand until the whole of the salt has entered into solution; it is then boiled, closed, and set aside. The other flask, during the heating, is allowed to remain at rest over the flame for a short time, so as to liberate a portion of the anhydrous salt. This flask is also boiled, although the operation is interrupted by violent bumpings. It is closed and placed by the side of the other flask, under the same conditions. When both flasks are cooled down to the temperature of the air (say about 50°), the flask containing the anhydrous salt will contain a crop of the 7-atom hydrate, built upon the anhydrous deposit. The other flask will have no crystalline deposit at all.

Seeing, then, that in the case of sodic sulphate, which is said to be always present in the air of rooms, and, according to MM. Gernez and Viollette, even in that of the country, the chances are that it is most likely to be present either in the effloresced condition or in solution, and equally non-nuclear in both, I cannot help hinking that too much importance has been given to this part of the subject; for if it be true, we are reduced to the dilemma, pointed out by M. Jeannel §, that there must be floating in the air specimens of all kinds of salts that form supersaturated solutions and crystallize by the introduction of a solid nucleus, whereas there are some such salts which cannot exist in the presence of the oxygen or of the ammonia of the air.

March 27.—Sir George Biddell Airy, K.C.B., President, in the Chair.

The Bakerian Lecture.—"On the Radiation of Heat from the Moon, the Law of its Absorption by our Atmosphere, and its variation in amount with her Phases." By the Earl of Rosse, D.C.L., F.R.S., &c.

In this paper is given an account of a series of observations made in the Observatory of Birr Castle, in further prosecution of a shorter and less carefully conducted investigation, as regards many details, which forms the subject of two former communications || to the Royal Society.

The observations were first corrected for change of the moon's distance from the place of observation and change of phase during the continuance of each night's work; and thus a curve, whose ordinates represented the scale-readings (corrected) and whose abscissæ represented the corresponding altitudes, was obtained for each night's work. By combining all these, a single curve and table for reducing all the observations to the same zenith-distance was obtained, which proved to be nearly, but not quite, the same as that found by Professor Seidel for the light of the stars. By employing the table thus deduced, and also reducing the heat-determinations obtained on the various nights for change of distance of the sun, a more accurate phase-curve was deduced, indicating a more rapid increase of the radiant heat on approaching full moon than was given by the formula previously employed, but still not so much as Professor Zöllner's gives for the moon's light.

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* Chemical News, February 4, 1870, p. 52.
† Proceedings of the Royal Society, 1871, p. 41.
‡ Chemical News, February 4, 1870, p. 52.
§ Ann. de Ch. et de Ph. 4th ser. vol. vi. p. 166.
|| Proceedings of the Royal Society, vol. xvii. p. 436, vol. xix. p. 9.

By employing Laplace's formula for the extinction of light in our atmosphere, the heat-effect in terms of the scale-readings was deduced, and an approximation to the height of the atmosphere attempted.

From a series of simultaneous measurements of the moon's heat and light at intervals during the partial eclipse of November 14, 1872, when clouds did not interfere, it was found that the heat and light diminish nearly if not quite proportionally, the minimum for both occurring at or very near the middle of the eclipse, when they were reduced to about half what they were before and after contact with the penumbra.

GEOLOGICAL SOCIETY.

[Continued from p. 314.]

November 20, 1872.—Prof. P. Martin Duncan, F.R.S., V.P., in the Chair.

The following communications were read:—

1. "On the Geology of the Thunder-Bay and Shabendowan Mining Districts on the North Shore of Lake Superior." By H. Alleyne Nicholson, M.D., F.G.S., &c.

The author described the general characters of Thunder Bay, which is almost landlocked on the south-east by the bold promontory of Thunder Cape and a series of islands which form a continuation of this. The rocks immediately surrounding Thunder Bay belong to the "Lower and Upper Copper-bearing series" of Canadian geologists. The latter, consisting of sandstones, shales, limestones, marls, and conglomerates, chiefly of a red or reddish colour, with interstratified traps, is regarded by the author as probably of Lower-Silurian age, in accordance with the opinion of Sir Wm. Logan. The "Lower Copper-bearing series" is also very varied in character; it is traversed by trap dykes, and contains several well-marked interstratified traps. It is penetrated by two sets of mineral veins, containing great abundance of silver. The majority of these run along the strike of the beds in a general E.N.E. and W.S.W. direction; the remainder are transverse, running nearly N. and S. Of the latter the most important is the "Silver-islet vein," which is 3 or 4 feet in width, and consists of quartz with native silver and galena; picked specimens of the stuff have assayed from £1000 to £2000 per ton. This vein has been worked for about two years, and has proved remarkably productive. Of the former series the most important is the "Shuniah vein," which runs along at a disance of 1½ to 2 miles from the north shore of Thunder Bay, the mines in which, although quite in their infancy, promise excellent results. Its wid this 22 feet; and the vein-stuff consists mainly of calc-spar. The silver is present in the native form and as sulphide. The vein traverses hard black shales, but does not run exactly along the strike of the beds; it may be traced for several miles towards the east.

The country between Thunder Bay and Lake Shabendowan, along the "Dawson Road," is of an undulating character; and the surface of its fundamental rocks everywhere exhibits unmistakable evidences of glaciation, the general direction of the striæ being N. and S., but with the occasional occurrence of a minor set of grooves running nearly E. and W. The greater part of the country is thickly covered with drift, composed of rocks which appear to have travelled from north to south.

The rocks passed over between Thunder Bay and Lake Shabendowan are described by the author as, 1, the shales and traps of the "Lower Copper-bearing series;" 2, a range of syenitic and gneissic rocks, probably of Laurentian age; 3, a great series of rocks belonging to the Huronian group, consisting of greenish or grey slates, with bands of gneiss and trap dykes, and bedded green traps with great masses of greenish, grey, or drab-coloured slates, the whole presenting a close resemblance to the green slates and porphyries of the English Lake district. The slates, in the author's opinion, are bedded felspathic ashes.

The author described the general characters of Lake Shabendowan, and stated that from the foot of the Lake for about 15 miles westward there is a succession of trappean rocks, beyond which, to the head of the lake, distant 13 miles, the country is occupied by Huronian slates like those between the lake and Thunder Bay. These slates extend for an unknown distance north-west of the head of the lake, and contain numerous veins, having an E.N.E. and W.S.W. direction, conformable with the strike of the beds; and some of them are auriferous. The vein-stuff is quartz containing copper pyrites; the gold is contained in the copper pyrites, or disseminated in very minute grains through the quartz. Several of these veins are being worked; and their peculiarities were noticed by the author.

2. "Note on the Relations of the supposed Carboniferous Plants of Bear Island with the Palæozoic Flora of North America." By J. W. Dawson, LL.D., F.R.S., F.G.S.

The author referred to Dr. Heer's paper on the Carboniferous Flora of Bear Island (see Q. J. G. S. vol. xxviii. p. 161), and stated that the plants cited by Dr. Heer as characteristic of his "Ursa stage," are in part representatives of the American flora belonging to what the author has called the "Lower Carboniferous Coal-measures" (Subcarboniferous of Dana). He considered that the presence of Devonian forms was due either to the mixture of fossils from two distinct but contiguous beds, or to the fact that in these high northern latitudes there was an actual intermixture of the two floras. He dissented altogether from Dr. Heer's identification of these plants with those of the Chemung group, or with those of the Middle Devonian of New Brunswick.

3. "Further Notes on Eocene Crustacea from Portsmouth." By Henry Woodward, Esq., F.G.S.

In this paper, after referring to his former communication on Crustacea from the Lower Eocene deposits at Portsmouth (Q. J. G. S. vol. xxviii. p. 90), the author gave a full description of *Rhachiosoma bispinosa*, one of the new species described in it, the materials being furnished by several fresh specimens, which show the whole structure of the animal. The new points include the description of the limbs, the anterior border of the carapace, the lower surface of the body in both sexes, and the maxillipeds.

The author also characterized, under the name of *Litoricola*, a new genus of Shore-crabs allied to *Grapsus*, from the same deposits. Of this genus he described two new species, *L. glabra* and *L. dentata*.

4. "On a new Trilobite from the Cape of Good Hope." By Henry Woodward, Esq., F.G.S.

The Trilobite described in this paper is from the Cock's-comb Mountains at the Cape of Good Hope, and was preserved in a nodule, the impression retained in which, when broken, furnished the most instructive details as to its structure. Each of the eleven thoracic segments was furnished with a long median dorsal spine, giving to the profile of the animal a crested appearance; on each side of this the axis of the segment bears two or three tubercles, and the ridge of the pleura four or five tubercles. The tail is terminated by a spine more than half an inch in length; and all the spines are annulated. For this Trilobite the author proposed the name of *Encrinurus crista-galli*, although with some doubt as to the genus, the head being only imperfectly preserved.

5. "On an extensive Landslip at Glenorchy, Tasmania." By S. H. Wintle, Esq.

In this paper the author described the effects of an extensive landslip from the northern face of Mount Wellington, in Glenorchy, about 5 miles from Hobart Town. It took place during the night of the 4th June, 1872, after a rainfall of 4½ inches in twenty-four hours. The *débris* descended nearly 2000 feet, into the bed of the rivulet of Glenorchy. By the force of the accompanying torrent great quantities of huge trees, some of them 200 feet long, were piled up in vast heaps, mixed with boulders, agricultural implements, fences, and other objects. The trees were deprived of bark, branches, and roots. The Carboniferous limestone forming the bed of the rivulet was exposed by the washing of the torrent for more than two miles; natural sections showed the blue shelly limestone alternating with beds of mudstone and shales. At one part the author found both banks of the rivulet lined with small, sharply angular fragments of dioritic greenstone from the summit of the mountain;

the beds displayed in a section close to the base of the great landslip, above which is a smooth surface of greenstone, covered with prostrate trees and immense blocks of greenstone half buried in yellow clay and sludge. The whole neighbourhood was described by the author as presenting evidences of former landslips. The author further described the appearances presented by the upper winding part of the gully traversed by the torrent, and, in conclusion, noticed certain results of similar phenomena as displayed in the same district. The paper was illustrated by three stereoscopic views.

LI. *Intelligence and Miscellaneous Articles.*

A NEW DETERMINATION OF THE VELOCITY OF LIGHT.

BY M. A. CORNU.

I HAVE the honour to present to the Academy the definitive result of my researches relative to the determination of the velocity of light, undertaken three years since. In a preceding communication I have succinctly described the method of observation, which, in principle, is that of the toothed wheel, due, as well as the improvements which have been made in it, to M. Fizeau. Of the latter I will mention the electrical registry of the velocity of the mechanism —which it is necessary to ascertain at each instant in absolute value, because it is with it that the velocity of light is directly compared. By the regularity and concordance of the results of my first essays I was led to hope that, by repeating the experiment between two new stations at four times the distance from each other, I should be able to obtain a determination sufficiently precise to decide between the two values given, the one by the old astronomical data (308000 to 310000 kilometres in a second), the other (298000) by the experiments of Foucault with the rotating mirror. I am happy to announce that the accuracy I expected ("probably to less than a hundredth") has been even exceeded; and the question of the absolute value of the velocity of light appears to me to be decided in favour of the lower number, as will be seen from the numerical results which I have obtained.

I will first briefly describe the arrangements. The observing-station is in an upper room of the pavilion of the École Polytechnique; the other, in the chamber of one of the barracks on Mont Valérien. At the former station are fixed the observing-telescope (aperture 180 millims., focal distance 2·4 metres), the toothed wheel and its motive mechanism, the illuminating system, the apparatus for registering the velocities, the electric wires, &c. The opposite station contains only the reflecting collimator, composed of an objective (aperture 110 millims., focal distance 1·2 metre) mounted as a telescope and with a small plane mirror of silvered glass in the focal plane.

I describe in detail in the memoir the precautions to be taken in order to adjust in the same right line the optic axes of the two apparatus, and to place the surface of the mirror in the focal plane of the collimator. This second condition must be fulfilled very accurately; otherwise the loss of light on the return would be considerable. I succeeded in fulfilling it completely by using as a reticule the silver pellicle partially raised on certain points of the mirror: the precision is then determined entirely by the defining-power of the telescope.

Among the improvements introduced in the course of these researches, I will mention the construction of the motor of the toothed wheel. Froment's motor, with helicoidal tooth-range, was given up, as it necessitated too great a motive force. I notably simplified the arrangement by utilizing some clockwork mechanisms sold under the name of *roulants carrés* (the sides from 12 to 15 centims.); the escapement and wheelwork are taken away, and the ratchet-wheel of the escapement is replaced by a lighter wheel with finer teeth: I used for this purpose three patterns, wheels with 104, 116, and 140 teeth.

By fitting a powerful spring in the barrel, I have been able to attain velocities of from 700 to 800 turns in a second. Finally, as a complement, on the axle of the minute-hand I disposed an electric cam (necessary for the registration of the velocity of rotation of the mechanism), a brake (to regulate the velocity at will), and a second barrel (permitting the toothed wheel to be turned in the contrary direction). This last arrangement is useful for the elimination of certain systematic errors which might result from the mechanism itself.

I shall not dwell upon the description of an experiment. The observer, attentive to the variations of intensity of the return-light, transmits electric signals to the recorder—that is, a cylinder covered with blackened paper, on which three electromagnets cause to be traced respectively the signals from the seconds-clock, from the cam of the mechanism, and from the key managed by the observer. In general he notes the successive disappearances of the light, which correspond to velocities of the toothed wheel, varying as the series of the odd numbers. Thanks to the working of the brake, he can at will produce an acceleration or retardation of the motion of the mechanism, or maintain a velocity sensibly constant during some seconds.

Notwithstanding the disadvantages of the atmosphere of Paris, I often obtained a very intense return-light with the oxyhydrogen lamp, or even with a simple petroleum-lamp. The total number of my observations exceeds a thousand; they are registered in the form of graphic traces, which I have the honour to lay before the Academy. The work of reduction is rather tedious; so I have taken up only those observations which are most complete and were made under satisfactory circumstances; their number amounts to about 650. A uniform method of calculation permitted me to deduce from the traces the time which the light occupied in accomplishing the double of the distance between the two stations. This distance, carefully determined, was found to be 10310 metres, with a probable error of less than 10 metres in excess or defect—that is to say, an approximation to within one thousandth part. I effected this measurement myself, with the aid of a little triangulation. I took advantage of the circumstance that the three salients of bastions No. 1, No. 2, and No. 5 of the fortifications of Mont Valérien could be seen from the belvedere of the École Polytechnique. Taking, with a good azimuth-circle, the angles subtended by those three points, I was able, by the calculation of the proper segments, to obtain the data necessary for ascertaining the distance between the two stations. The dimensions of the fortress having been accurately measured by the engineers, I obtained from the Dépôt des Fortifications the distances between the necessary points, which enabled me to dispense with measuring a base.

I found, besides, at the Prefecture of the Seine, two determinations of the distance from certain points on Mont Valérien to the Panthéon (one derived from the operations for the Register, the other from those of the Commission for the Plan of Paris). The mean of the three closely agreeing values thus obtained gave the number above adopted. If a subsequent geodesic operation should furnish more exactly the distance between the two stations, it will be very easy to calculate the correction to be applied to my results.

The following Table gives the result of the definitive calculations; the values of the velocity of light, expressed in kilometres per second, are classed according to the order of the occultations of the return-light which furnished them.

Order 1.	Order 2.	Order 3.	Order 4.	Order 5.	Order 6.	Order 7.
—	302600	297300	298500	298800	297500	300400
—	(17)	(236)	(376)	(480)	(91)	(27)

The numbers in parentheses express the relative *weight* of the corresponding values. They were formed by dividing by 10 the product of the number of observations into $2n-1$ (n being the order of the occultation) and the coefficient 1, 2, 3, or 4, according as the remark in the experiment-book was "pretty good," "good," "very good," or "excellent," according to the state of the atmosphere.

The compound mean gives 298400. Multiplying this by the index of refraction of the air, 1·0003, we obtain the number 298500 kilometres per second as the value of the velocity of light *in vacuo* deduced from the totality of my observations. I reckon that this number approaches within $\frac{1}{300}$.

The accordance of this result with Foucault's is not uninteresting; and it should be remarked that Foucault's experiments required verification, not only because the details of the observations and procedure have not been published, and therefore have undergone no discussion, but also because the rotating-mirror method is exposed to grave objections (into the statement of which I shall not now enter), while M. Fizeau's method is free from those objections.

Astronomers, also, will find in this new determination of the velocity of light an important confirmation of the value of the sun's parallax 8″·86, which is obtained by comparing this number with the constant of the aberration. It is the value which M. Le Verrier has found again by three series of observations relative to the motions of the planets, particularly Mars and Venus. The importance, then, for astronomy, of the precise determination of the velocity of light cannot be too much insisted on.

In conclusion, I think I am right in affirming, as M. Fizeau announced immediately after his first researches, that the same experiments could, without much more difficulty, be repeated, under favourable atmospheric and topographical conditions, with stations from 20 to 30 kilometres apart. Then, with the aid of a special geodesic operation, I doubt not that we could obtain a determination of the velocity of light approximate within less than a thousandth part. I have the greatest desire to attempt this experiment, and should deem it a great honour if the Academy would receive the project favourably. It is desirable, for the honour of French science, that those grand labours commenced by Rœmer at the Observatory of Paris, simplified and continued by French savants, should be completed in France with all the precision which comports with their importance in the light of physics and astronomy. —*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 338–342.

NEW EXPERIMENTS ON SINGING FLAMES. BY FR. KASTNER.

If two flames of suitable size be introduced into a glass tube and both placed at two thirds of the length of the tube from its lower end, they will vibrate in unison. The production of the phenomenon continues as long as the flames are kept separate; but the sound ceases as soon as the two are put in contact.

I took a glass tube 55 centims. in length, 41 millims. in exterior diameter, and 2·5 millims. in thickness. Two separate flames, produced by the combustion of hydrogen gas issuing from burners of suitable construction, placed at 183 millims. from the base gave the sound of FA natural.

As soon as, with the aid of very simple mechanism, the flames are brought together, the sound is suddenly interrupted. If we vary the position of the flames in the tube, leaving them still separate, above the third part of the length the sound diminishes as far as the middle of the tube, beyond which all sound ceases; below the same point, on the contrary, the sound augments as far as the quarter length of the tube. If at this place the flames are brought together, the sound does not cease immediately, as the two flames can continue to vibrate as a single one.

The interference of singing flames is only produced under special conditions. It is of importance that the length of the tubes be in accordance with the number of the flames; the height of the flame exerts only a limited action upon the phenomenon, while the form of the burners plays an important part.

of a millimetre in the cylindrical part of the tube at the temperature θ_3; let this be u.

With these data the entire calculation can be effected.

§ 2.

Two formulæ especially present themselves from which we may endeavour to judge of the behaviour of superheated vapour. Let p, v, and t be the values of pressure, volume, and temperature (Celsius) of a determined weight of superheated vapour; we can either put

$$pv=\phi(273+t),$$

where ϕ is then a variable whose law of variation must be sought, or we can put

$$pv=C(E+t),$$

where E is such a variable, but C is a constant dependent on the quantity by weight of the vapour.

The latter formula was applied by Fairbairn and Tate* to their observations on steam (to be considered further on). I have not chosen it, because, with the proportions of my experiments, the results calculated after this formula must have given less striking numbers than those calculated according to the first. We shall easily be convinced of this from the following.

Thus, if we take the formula

$$pv=\phi(273+t),$$

it is only when the vapour exhibits the behaviour of a perfect gas that ϕ becomes a constant depending on the weight of the vapour. At the same temperature t, for dry air, we have

$$PV=R(273+t),$$

where P and V denote pressure and volume, and R is a constant depending on the quantity of air employed. This equation presupposes, of course, the full validity of Mariotte and Gay-Lussac's law.

From the two equations we get

$$p-P=(273+t)\left(\frac{\phi}{v}-\frac{R}{V}\right).$$

Putting $$p-P=H,$$

we get

$$\phi=v\left(\frac{H}{273+t}+\frac{R}{V}\right),$$

according to which I have calculated my observations.

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* Phil. Trans. 1862, p. 591.

H is the difference (reduced to 0°) of the levels of the mercury in the tube at each experiment, and so the proper object of the measurements. In the calculations, H is given in millimetres.

v, R, and V are obtained in the following manner from the mercury-weighings:—

If we imagine the tube divided into two exactly equal parts, and if W_2 is the volume of each part at 0° expressed in the weight of mercury at 0°, at the temperature t of observation it becomes $W_2(1+\beta t)$, if β is the coefficient of expansion of the glass. If, now, during the observation made for the temperature t the weight of the mercury in the vapour-half of the tube is found to be W_1, this occupies the space $W_1(1+\gamma t)$, where γ is the known coefficient of expansion of mercury at that temperature. The vapour-volume v at the temperature t is evidently equal to the difference,

$$v=W_2(1+\beta t)-W_1(1+\gamma t).$$

But, from the mercury-weighings mentioned in the preceding section, we obtain

$$W_2=\frac{x}{2}\cdot\frac{1+\gamma\theta}{1+\beta\theta}.$$

$W_1(1+\gamma t)=W_2(1+\beta t)-y[1+\gamma\theta_1][1+\beta(t-\theta_1)]$, if for the temperature t we first select exactly that temperature of observation at which we at the same time noted down the position of the mercury about the diamond-strokes.

Just so

$$V=W_2(1+\beta t)-(W-W_1)(1+\gamma t);$$

and, finally,

$$R=\frac{ap_0z[1+\gamma\theta_2][1+\beta(\theta-\theta_2)]}{1+\alpha\theta},$$

where α is the well-known value 0·003663.

It will at the same time be seen that all the space-determinations are effected with the same unit, mercury at 0°.

For any other t, v and V acquire other values only so far as W_1 changes with t; while W_2 and W remain constant for all values. On account, however, of the cylindricity of the part of the tube which here comes into consideration, we must put

$$\frac{dW_1}{dt}=-\text{const.}\frac{dH}{dt};$$

that is, according to No. 5 of the above mercury-weighings, remembering that H is given in height of mercury at 0°,

$$\frac{dW_1}{dt}=-u(1+\gamma\theta_3)\frac{dH}{dt}=-c\frac{dH}{dt}.$$

Therefore, W_2, W, R, and c once calculated for a series of observations, the rest of the calculation is performed with the two formulæ

$$\phi = [W_2(1+\beta t) - W_1(1+\gamma t)] \left\{ \frac{H}{273+t} + \frac{R}{W_2(1+\beta t)-(W-W_1)(1+\gamma t)} \right\}$$

and

$$\frac{dW_1}{dt} = -c\frac{dH}{dt}.$$

It will now be seen from the following that the first term in the right-hand brackets for ϕ, on account of the small value of H in the greatest number of my series of observations, is decidedly the smaller of the two bracketed terms. An at all events small error in the determination of t would, on this account, have absolutely no influence; hence this formula was especially advantageous for the calculation of the experiments.

§ 3.

Before communicating the observations on vapours, I will first cite two tests of accuracy to which I subjected the method. First, I filled a tube on both sides with dry air; constant values of ϕ must evidently then be obtained. The two following Tables contain the results from two tubes. On their arrangement nothing more need be added after what has been said.

TABLE I.

$W_2=307\cdot6$. $W=224\cdot5$. $R=414\cdot674$. $c=0\cdot88$.

t.	H.	W_1.	v.	V.	ϕ.	p.
°	millims.					millims.
30·7	+46·35	89·59	217·75	172·1	557·90	778
42·5	46·45	89·5	217·7	171·9	557·21	
52·3	46·45	89·5	217·65	171·7	556·73	
65	46·65	89·31	217·7	171·3	557·04	865

TABLE II.

$W_2=309\cdot6$. $W=224\cdot5$. $R=387\cdot472$. $c=0\cdot88$.

16·3	55·8	81·24	228·25	166·05	576·64	731
28·2	56·1	80·98	228·4	165·55	577·11	
42·4	56·3	80 8	228·5	165·1	577·05	
58·7	56·4	80·71	228·4	164·7	576·17	837

The constancy of ϕ is sufficiently attained; it is seen, however, within what limits small variations occur.

In the second the question was, to realize a case in which a variable ϕ might be surely expected. I endeavoured to make use of my earlier experience*, according to which, at not high temperatures small quantities of aqueous vapour exhibit extraordinary power of adhesion to glass and mercury. I put into a tube a portion of dry mercury, and filled up one side with dry air; into the other side I then poured a quantity of somewhat moist mercury: the rest of this side was likewise filled with air and hermetically sealed. I now heated it strongly, in order to expel the water from the mercury and so fill the air above it with aqueous vapour. A sufficient quantity of vapour actually entered the space which the air occupied, as the following series of measurements show; they contain the corresponding values of *t* and H.

TABLE III.

t.	H.	*t*.	H.	*t*.	H.
°	millims.	°	millims.	°	millims.
41·6	−113·5	18·3	−104·8	64·9	−107·8
51·7	−115·4	14	−105·3	64·7	−107·3
61·9	−116·9	21	−104·5	70·5	−107·7
71·5	−116·4	25·1	−104	70·7	−107·8
78·3	−112·7	30·1	−103·2	76·1	−108·7
83·7	−111·3	35	−103·5	76	−109
90	−111·2	40·3	—104	75	−108·7
93	−111	46·2	−104·8	80·5	−109·5
85	−110·2	32·7	−103		
78·3	−109·6	50·4	−105·5		
13·6	−106·3	60·1	−106·9		
26·1	−103·9	70·5	−108·4		
34·8	−103·3	80·4	−109·9		
48·2	−105·1	89·8	−110·9		
58·1	−106·7	94·4	−111		
48·2	−105·1	83·6	−109·8		
41	−103·9	73·6	−108·4		
29·8	−102·7	64·1	−107·1		
26·5	−103·5	54·9	−105·4		
		54	−105·2		

This Table shows that as far as 90°, during the first heating the air on the side of the moist mercury exerted a considerably less pressure than afterwards, when the steam expelled by the heating cooperated.

In the numbers which were obtained after this high temperature had once been reached, such an agreement is recognized in the repeated ascending and descending series, that greater could not be expected if adhesion of the aqueous vapour took place.

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* Pogg. *Ann.* vol. cxxxvii. p. 602.

These numbers are arranged in the following Table according to the temperatures, and, with their mean values (given under the heading "H corrected"), made use of for the calculation of the values of ϕ. In another column the variation of ϕ for each 10° of change of temperature is given in percentages of ϕ—thus, $\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$.

Table IV.

$W_2=390$. $W=400$. $R=144$. $c=0{\cdot}722$.

t.	H.	H corrected.	W_1.	*v*.	V.	ϕ.	$\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$.	*p*.
°	millims.							millims.
13·6	−106·3	−105·7	245·73					
14	−105·3	−125·7	245·73	143·9	235·5	34·97	11	70
18·3	−104·8	……	245·08	144·3	234·7	36·62		
21	−104·5	……	244·86	144·4	234·45	37·36	9	
25·1	−104	……	244·5	……	……	……		
26·1	−103·9	−103·7	244·28	……	……	……	8	
26·5	−103·5	−103·7	244·28	144·8	233·8	39·04		
29·8	−102·7	−102·9	243·71	……	……	……	9	
30·1	−103·2	−102·9	243·71	145·25	233·15	40·40		
32·7	−103	−103·1	243·85	……	……	……	1·5	
34·8	−103·3	−103·4	244·07	……	……	……		
35	−103·5	−103·4	244·07	144·7	233·4	40·70	1·2	
40·3	−104	……	244·5	144·1	233·7	40·96		
41	−103·9	−104	244·5	……	……	……		
46·2	−104·8	……	245·08	……	……	……		
48·2	−105·1	……	245·29	……	……	……	0·8	
48·2	−105·1	……	245·29	……	……	……		
50·4	−105·5	−105·2	245·37	142·9	234·4	41·30		
54	−105·2	−105·3	245·44					
54·9	−105·4	−105·3	245·44					
58·1	−106·7	……	246·45					
60·1	−106·9	……	246·59	141·5	235·5	41·08		
64·1	−107·1	……	246·74					
64·7	−107·3	−107·6	247·1					
64·9	−107·8	−107·6	247·1					
70·5	−107·8	−108	247·39					
70·5	−108·4	−108	247·39	140·1	236·1	41·40		
70·7	−107·7	−108	247·39					
73·6	−108·4	……	247·68					
75	−108·7	……	247·89					
76	−109	−108·9	248·04					
76·1	−108·7	−108·9	248·04					
78·3	−109·6	……	248·54					
80·4	−109·9	−109·7	248·62					
80·5	−109·5	−109·7	248·62	138·5	237·15	41·19		
83·6	−109·8	……	248·69					
85	−110·2	……	248·98					
89·8	−110·9	……	249·48					
90	−111·2	−111	249·55					
93	−111	……	249·55					
94·4	−111	……	249·55	137·05	237·85	41·57		111

The values of ϕ therefore hold good for air containing a certain quantity of aqueous vapour, which at low temperatures partially adheres and in various proportions at various temperatures. It is seen what great changes this adhesion, consequently the variable going out of play of a certain quantity of vapour, occasions in the values of ϕ belonging to low temperatures.

The method has accordingly been proved by a twofold test to be perfectly applicable; and this is the direct confirmation of an antecedently justified expectation.

§ 4.

The vapours which I investigated by this method were sulphide of carbon, chloroform, and ethylic alcohol. The chloroform was a small, carefully preserved remainder of the preparation made use of in my earlier experiments; the two others were newly prepared. In the following the Tables on the vapour of sulphide of carbon shall first be given. In the last column the vapour-pressures p are still shown; they are calculated from ϕ by the formula

$$p=\frac{\phi(273+t)}{v}.$$

It may further be remarked that the *absolute* values of ϕ and p are affected by at any rate small errors in the above-mentioned weighings of the individual volumes of the tubes, which cannot be excluded; and therefore in this point the possibility of small errors must be borne in mind. The *course* of the values of ϕ, however, which is alone of importance, depends essentially upon very exactly observed values of H. Where several series of observations gave somewhat differing numbers for the latter values, I have noted down, in the column "H corrected," the numbers made use of for the calculation; I have also inserted in this column notices of the cessation of saturation. Here, however, according to the Tables, the difference is never more than 0·1 millim.

Sulphide of Carbon.

TABLE V.

$W_2=378·4$. $W=408$. $R=67·425$. $c=0·824$.

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
27·2	+5·75	……	202·9	174·75	172·5	71·65	123
41·2	5·85	……	202·82	174·45	172·1	71·59	129
55·8	5·95	……	202·74	174·1	171·6	71·56	135
62	6	……	202·69	174	171·4	71·56	138
71·5	6·1	……	202·61				
72	6·1	……	202·61	173·8	171	71·60	142
74·5	6	6·1	202·61				
88·2	6·2	……	202·53	173·4	170·4	71·59	149

TABLE VI.

$W_2=503·8$. $W=572$. $R=124·956$. $c=1·087$.

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
11·2	−31·1	......	283·1	220·3	214·45	104·26	134
18	−31·3	......	283·32	219·8	214·4	104·39	138
25·4	−31·7	......	283·75	219·05	214·55	104·31	142
26·6	−31·7	......	283·75	219	214·5	104·41	143
34·3	−31·9	......	283·97	218·5	214·4	104·66	147
38·2	−32·1	......	284·19	218·1	214·5	104·56	149
44·8	−32·35	......	284·46	217·6	214·5	104·61	153
55	−32·6	......	284·73	216·9	214·35	104·88	159
57·2	−32·7	......	284·84	216·7	214·4	104·83	160
59·2	−32·7	......	284·84	216·6	214·3	104·98	161
64·9	−33	......	285·16	216·2	214·4	104·89	164
80·4	−33·2	......	285·38	215·2	213·8	105·56	173
89	−33·6	......	285·82	214·45	214·1	105·25	178

TABLE VII.

$W_2=1110$. $W=1251$. $R=230·241$. $c=2·408$.

12·6	+7·3	......	593·52	515·5	451·4	276·11	153
18	7·4	......	593·28	515·3	450·6	276·40	156
19·8	7·4	......	593·28				
25·8	7·55	7·5	593·04				
30·2	7·45	7·5	593·04	514·5	449·25	276·40	163
32·2	7·55	7·5	593·04				
42·1	7·55	......	592·92				
53·5	7·7	......	592·56				
56·3	7·6	7·7	592·56	512·9	446·4	276·53	178
57·5	7·7	......	592·56				
74·6	7·8	......	592·32				
76·3	7·9	7·8	592·32	511·6	444·3	276·54	189
87·6	7·85	......	592·2				
94·7	7·85	......	592·2	510·3	442·4	276·47	199

TABLE VIII.

$W_2=1023·7$. $W=1160$. $R=209·6$. $c=2·76$.

9·3	+15·2	......	529·6	493·45	392·5	290·07	166
19·7	15·25	......	529·46	492·85	391·4	289·61	172
24·6	15·3	......	529·32				
28·7	15·3	......	529·32	492·35	390·5	289·23	177
34	15·3	......	529·32				
35·5	15·4	......	529·05				
43·3	15·5	......	528·77	491·9	388·6	289·42	186
50·1	15·65	15·6	528·5				
53·6	15·55	15·6	528·5	491·4	387·4	289·34	192
64·2	15·8	......	527·94				
65·2	15·8	......	527·94	491·2	385·8	289·81	199
79·3	15·95	......	527·53	490·6	384·1	289·88	208

TABLE IX.

$W_2=386·4$. $W=404$. $R=146·78$. $c=0·743$.

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
16·6	− 2·4	......	231·23	154·6	213·3	105·10	197
28·3	− 2·4	......	231·23	154·25	213	105·14	205
39·7	− 2·3	......	231·15				
49·2	− 2·2	......	231·08	153·7	212·4	105·16	220
57	− 2·2	......	231·08				
65·8	− 2·1	......	231·01				
76·5	− 2	......	230·86	153	211·6	105·25	240

TABLE X.

$W_2=525·4$. $W=576$. $R=134·187$. $c=1·02$.

17·6	+ 7·3	7·26	250·9	273·9	199·5	191·07	203
25·2	7·2	7·26	250·9	273·7	199·15	191·07	208
25·6	7·3	7·26	250·9				
32	7·25	7·26	250·9				
34·8	7·15	7·26	250·9				
45	7·2	7·26	250·9				
45·2	7·25	7·26	250·9	273	198·2	191·06	223
48·5	7·25	7·26	250·9				
55·5	7·3	7·26	250·9				
67·5	7·4	7·26	250·9				
77·7	7·3	7·26	250·9	271·95	196·7	191·15	246

TABLE XI.

$W_2=281·4$. $W=367$. $R=181·432$. $c=0·903$.

16·6	+ 4·8	......	200·71	180·2	214·75	155·23	250
28·3	5	......	200·53	180·1	214·35	155·43	260
39·7	5·1	......	200·45	179·9	214	155·45	270
49·2	5·15	......	200·4	179·7	213·8	155·37	278
53·5	5·05	5·15	200·4				
57	5·15	......	200·4	179·5	213·6	155·27	285
65·8	5·2	......	200·35	179·3	213·4	155·19	293
71·2	5·3	5·25	200·31				
76·5	5·2	5·25	200·31	179	213·1	155·09	303

TABLE XII.

$W_2=1029·7$. $W=1149$. $R=509·402$. $c=2·452$.

36·6	+12·7	......	564·35	462·5	442·1	551·88	369
45	12·8	......	564·1	462·1	441·2	552·13	380
51	12·8	......	564·1				
55	12·9	......	563·85	461·6	440·1	552·44	393
63	13	......	563·61				
70·3	13	......	563·61	460·7	438·6	552·65	411
78·6	13·2	......	363·12	460·1	437·5	553·59	423

TABLE XIII.

$W_2=574.\quad W=621.\quad R=175{\cdot}824.\quad c=1{\cdot}166.$

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
30·6	+ 94·4	saturated.					
33	still quite	saturated.					
36·6	+105·9	......	183·5	389·8	134·1	644·41	512
45·2	106·2	......	183·15	390	133·2	644·96	526
55·6	106·6	......	182·7	390·25	132·1	646·02	544
64·6	106·9	......	182·33	390·45	131·1	647·28	560
67·4	106·9	......	182·33	390·4	130·9	646·98	564
79·4	107·3	......	181·9	390·6	129·7	648·40	583

TABLE XIV.

$W_2=730{\cdot}1.\quad W=864.\quad R=441{\cdot}582.\quad c=1{\cdot}527.$

51·6	+ 24·85	......	338	389·9	200·1	890·28	741
61	24·9	......	337·92	389·5	199·3	892·04	765
72	25·05	......	337·7	389·3	198·2	895·61	794
80·7	25·2	......	337·47	389·1	197·3	898·57	817

TABLE XV.

$W_2=391{\cdot}9.\quad W=316.\quad R=537{\cdot}183,\quad c=0{\cdot}853.$

54·3	+ 6·9	......	155·74	235·15	230·6	552·74	769
63	7·1	......	155·57	235·15	230·25	553·58	791
70·4	7·3	......	155·4	235·2	229·9	555·56	810
80·2	7·45	......	155·27	235·1	229·6	555·01	834
90·1	7·55	......	155·19	235	229·3	555·43	858

TABLE XVI.

$W_2=570.\quad W=447.\quad R=766{\cdot}94.\quad c=1{\cdot}168.$

55·8	+65·5	just above saturation.	143·48	425·85	264·2	1321·03	1020
62	65·7	......	143·25	426	263·7	1322·52	1040
71·5	66	......	142·9	426·2	263	1324·45	1061
74·5	66	......	142·9	426·2	262·85	1324·51	1080
88·2	66·4	......	142·43	426·5	261·8	1327·83	1125

TABLE XVII.

$W_2=719·6$. $W=759$. $R=1709·13$. $c=1·656$.

t.	H.	H corrected.	W_1.	Vapour-volume. v.	Air-volume. V.	ϕ.	Vapour-pressure. p.
°	millims.						millims.
70·8	+10·3	covered.					
71·7	13·05	without a breath.	410·18	305·4	367·5	1431·88	1616
77·5	13·05	......	410·18	305	367·2	1430·97	1644
83·6	13·45	13·4	409·6	305·3	366·4	1435·59	1677
84	13·35	13·4	409·6				
89·5	13·5	......	409·44	305·1	365·9	1436·49	1706
96·1	13·9	......	408·77	305·4	364·9	1447·93	1743

TABLE XVIII.

$W_2=669$. $W=724$. $R=1525·47$. $c=1·525$.

83·6	+76·8	strongly covered.					
84·4	78·6	......	306	359·7	246	2309·63	2295
84·9	78·6	......	306				
87·6	78·55	78·6	306				
89·5	78·6	......	306	359·5	245·7	2309·97	2329
90·8	78·6	......	306	359·4	245·6	2309·95	2338
96·8	78·6	......	306				
97·3	78·6	......	306	359·2	245·2	2310·95	2382

TABLE XIX.

$W_2=425$. $W=487$. $R=874·93$. $c=0·938$.

88	+73·7	strongly covered at the top.					
89	76·75	......	193·45	229·3	127·6	1620·89	2559
91·2	76·8	......	193·4	229·3	127·5	1621·86	2576
98	77·1	......	193·12	229·46	126·92	1629·48	2635

The preceding Tables show that in the individual series of observations the vapour-volumes were approximately constant. Heated in these constant spaces, the vapour shows for the lower pressures, up to about 300 millims., constant values of ϕ, while for higher pressures ϕ increases in a slight degree with rising temperature; but in these cases the augmentation of ϕ is decidedly spread uniformly over the whole interval of the set of observations, and is by no means more prominent in the first degrees of the superheating. In several series it is evident that immediately from still observed saturation onwards, only just this small alteration of ϕ makes its appearance. The following Table gives the variations of ϕ in percentages of the then subsisting value of ϕ for the lowest temperature. The variations are taken from the temperature- and respective pressure-intervals, and are calculated for 10° of further heating. In some cases, in which somewhat different values of $\frac{\Delta\phi}{\Delta t}$ appeared to result for the first part of the observations, the same expression $\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}$ was calculated separately for this part, and the numbers are enclosed in brackets in the Table. But it will be seen that in four instances out of the five this value is smaller than that derived from the entire interval of the observations; so that the above assertion can decidedly be maintained. If in isolated places a value of H different by 0·1 millim. were taken (and that, according to what has been said, must be conceived to be the limit of the errors of observation here possible), the course of the values of ϕ would appear perfectly regular in the given direction.

TABLE XX.

Limits of pressure.	Limits of temperature.	$\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}$.
millims.		
123– 149	27°·2–88°·2	0
134– 178	11·2–89	+0·12
153– 199	12·6–94·7	0
166– 208	9·3–79·3	0
(„ – 192	„ –53·6	–0·06)
197– 240	16·6–76·5	0
203– 246	17·6–77·7	0
250– 303	16·6–76·5	0
369– 423	36·6–78·6	+0·07
(„ – 411	„ –70·3	+0·04)
512– 583	36·6–79·4	+0·15
741– 817	51·6–80·7	+0·32
(„ – 794	„ –72	+0·29)
769– 858	54·3–90·1	+0·14
(„ – 810	„ –70·4	+0·32)
1020–1125	55·8–88·2	+0·16
1616–1743	71·7–96·1	+0·3
(„ –1706	„ –89·5	+0·18)
2295–2382	84·4–97·3	+0·05
2559–2635	89 –98	+0·59

Before further discussing the results, the observations on the two other vapours may be added. On the first of these, chloroform, from the small quantity of my material, I could only execute four series. The numbers are contained in Tables XXI.–XXIV.

Chloroform.

TABLE XXI.

$W_2=659·4.$ $W=673.$ $R=325·307.$ $c=1·605.$

t.	H.	H corrected.	W_1.	Vapour-volume. *v.*	Air-volume. V.	*φ*.	Vapour-pressure. *p.*
	millims.						
38°	+25·2	decidedly saturated.					
41·2	37·1						
42·2	37·9	still a breath.					millims.
42·9	38	clear.	325·16	332·4	309·55	389·83	370
45·3	38·2	......	324·84	332·6	309·1	389·95	373
48·8	38·25	......	324·76	332·5	308·8	389·80	377·5
58·6	38·6	......	324·2	332·7	307·85	390·29	389
70	39	......	323·56	332·9	306·7	390·95	403
79·8	39·25	......	323·16	332·85	305·8	391·11	415
90	39·65	......	322·52	333·05	304·6	392·07	427

TABLE XXII.

$W_2=438·6.$ $W=432.$ $R=606·982.$ $c=1·275.$

62	− 9·9	saturated.					
63	− 8·9	......	258·39	177·9	263·7	404·77	765
64	− 8·9	......	258·39	177·9	263·65	404·86	767
68·1	− 8·8	......	258·26	177·9	263·45	405·29	777
80·1	− 8·4	......	257·75	177·95	262·65	407·01	807
90·1	− 8·15	......	257·43	177·9	262·1	407·99	833

TABLE XXIII.

$W_2=549·5.$ $W=494.$ $R=1231·89.$ $c=1·259.$

73·8	−20	much saturated.					
74·8	−14·6						
75·4	−11·25	appearance of a breath, but uncertain.	313·11	233·15	367·15	774·75	1158
75·9	−11·05	clear.	312·86	233·4	366·9	776·26	1160
79·2	−11	......	312·8	233·3	366·75	776·35	1172
94·9	−10·3	......	312·41	233	366	777·61	1228

TABLE XXIV.

$W_2=498.$ $W=485.$ $R=1226·468,$ $c=1·18.$

90	+58·4	saturated.					
91·6	59	......	251·61	243·3	261·8	1179·16	1767
92·8	59·05	......	251·55	243·35	261·7	1179·75	1773
94·7	59·2	......	251·37	243·45	261·5	1181·00	1784
97·3	59·3	......	251·25	243·5	261·3	1182·35	1798
98	59·35	......	251·19	243·5	261·2	1182·22	1801

The proportions here are exactly the same as with sulphide of carbon. The following Table XXV. corresponds to Table XX. (for sulphide of carbon).

Table XXV.

Limits of pressure.	Limits of temperature.	$\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}$.
millims.		
370- 427	42°·9-90°	+0·15
(„ - 415	„ -79·8	+0·12)
765- 833	63 -91	+0·29
(„ - 777	„ -68·1	+0·25)
1158-1228	75·4-94·9	+0·2
(1160- „	75·9- „	+0·09)
1767-1801	91·6-98	+0·4

With respect to the variation shown by the two numbers taken from Table XXIII., the remark there made in the column "H corrected" may serve to explain it.

Things assume a different form with alcohol; for here phenomena of adhesion are very distinctly recognizable. For example, the following are the numbers, arranged in the order of time, of one series of observations, together with the notes taken from my journal:—

Table XXVI.

t.	H.
	millims.
98°·5	−2·65
95·8	−2·85

At 95°, on the alcohol side a very feeble breath seems already to form on the glass, of condensed particles of vapour.

	millims.
92°·9	−3·15
90	−3·65
88·2	−7·1 decidedly saturated.
90·1	−3·55
93·2	−3·25

During the heating from 90°·1 to 93°·2 a narrow dull ring is seen to have formed round the uppermost layers of the mercury on the alcohol side, proceeding from vapour-particles precipitated during the previous cooling from 95° to 90°, which are still retained. During the continued heating, gradually larger vesicles of vapour are seen to form there, consequently below the level of the mercury.

°	millims.
95·1	−2·95
97·1	−2·95
97·6	−2·95

There is now evidently a somewhat smaller quantity of vapour in action than at the commencement of the experiments; and this is recognizable in the feebler tension of the vapour.

The influence of this adhesion shows itself also in the series of the following Tables for alcohol. I have on each occasion been able to convince myself of this by simply observing the tubes narrowly: to a practised eye the difference in behaviour between such an adhering substance and one that does not adhere, in the vicinity of saturation and in the transition to saturation, is not to be mistaken. It only remains surprising that in my earlier investigations I found alcohol not to be one of the number of adhering substances. It may, however, be remarked that then and now I investigated two different preparations, and it is quite conceivable that the slightest shade of difference in preparing the material may call forth this property. Tables XXVII.–XXXIII. contain the observations on

Alcohol.

TABLE XXVII.

$W_2=597·1$. $W=668$. $R=51·465$. $c=1·2545$.

t.	H.	H corrected.	W_1.	Vapour-volume. *v.*	Air-volume. V.	*φ.*	Vapour-pressure. *p.*
°							
20	{ considerable negative H.						
36·2	+39·7	saturated.					
38	45·1						
39·5	48·7						millims.
43	52·3	……	277·98	317·6	204·7	132·41	132
52·5	53·5	……	276·48	318·75	202·6	133·36	136
61·5	55·6	……	273·85	321	199·3	135·77	143
82·5	56·4	……	272·85	321·35	197·2	135·84	149

TABLE XXVIII.

$W_2=700·5$. $W=761$. $R=299·388$. $c=1·633$.

t.	H.	H corrected.	W_1.	Vapour-volume. *v.*	Air-volume. V.	*φ.*	Vapour-pressure. *p.*
54·6	negative.	{ much saturation.					
58·6	+7·5	……	381·59	315·9	318·1	304·46	320
60	7·8	……	381	316·3	317·6	305·57	322
67·3	8·4	……	380·12	316·9	316·15	307·72	330
70·5	8·4	……	380·12				
78·2	8·7	……	379·63	316·85	315·1	308·90	342
88	8·9	……	379·3	316·6	314·2	309·38	353
92·1	8·8	8·9	379·3				
98	8·9	……	379·3	316·1	313·7	309·26	363

TABLE XXIX.

W_2=708·5. W=404. R=1143·34. c=1·706.

t.	H.	H corrected.	W_1.	Vapour-volume, *v.*	Air-volume. V.	ϕ.	Vapour-pressure. *p.*
°	millims.						millims.
77·9	+ 1	covered.					
78·8	4·5	covered. a trace.					
79·5	8·1	......	227·44	479·15	530·75	1039·12	764
80·1	5	5·1	227·44				
80·7	5·2	......	227·27	479·3	530·55	1039·94	767·5
82	5·4	......	226·93	479·6	530·2	1041·52	771
84·4	5·8	......	226·25	480·25	529·5	1044·79	777
86	5·9	......	226 08				
87	6·1	......	225·73	480·7	528·9	1047·29	784
88·3	6·3	......	225·39				
90	6·4	......	225·22	481·15	528·3	1049·78	792
92·5	6·8	......	224·54				
93	6·8	......	224·54	481·8	527·6	1053·04	800
96·4	7·05	......	224·11				
98	7·15	......	223·94	482·25	526·9	1055·74	812

TABLE XXX.

W_2=621·2. W=401. R=846·97. c=1·479.

t.	H.	H corrected.	W_1.	Vapour-volume, *v.*	Air-volume. V.	ϕ.	Vapour-pressure. *p.*
78·7	+14·3	saturated.					
79·2	15·9	do.					
79·5	scarcely a breath.						
80·8	16·55	......	174·99	444·9	393·1	979·42	778
80·8	16·45	16·55	174·99				
82	16·75	......	174 69	445·15	392·75	980·97	782
83	16·95	......	174·4	445·4	392·4	982·57	785
84·8	17	......	174·32				
86	17·2	......	174·03	445·65	392	984·24	793
87·4	17·2	......	174·03				
88·9	17·6	......	173·44	446·3	391·3	987·72	801
90·7	17·8	......	173·14	446·6	390·95	989·39	805
93·8	18	......	172·84	446·8	390·55	990·89	818
97·2	18·2	......	172·55	447·1	390·2	992·45	826

TABLE XXXI.

W_2=597·1. W=685. R=809·849. c=1·388.

t.	H.	H corrected.	W_1.	Vapour-volume, *v.*	Air-volume. V.	ϕ.	Vapour-pressure. *p.*
81·5	−56·2	saturated.					
82·1	−54·8	......	372·35	220·4	331·7	504·10	812
82·4	−54·8	......	372·35				
83	−54·35	......	371·73				
84·4	−54·35	......	371·73	220·9	331·05	506·79	820
87·4	−54·1	......	371·38	221·1	330·6	508·42	829
89·3	−53·9	......	371·1	221·3	330·2	509·84	835
91·8	−53·8	......	370·90	221·3	330	510·45	841
97·7	−53·6	......	370·68	221·2	329·4	511·85	858

TABLE XXXII.

$W_2=685$. $W=400$. $R=1023·76$. $c=1·515$.

t.	H.	H corrected.	W_1.	Vapour-volume. *v*.	Air-volume. V.	*φ*.	Vapour-pressure. *p*.
°	millims.						
80·8	+12·7	much saturated.					
82·5	20·3	still a trace.					
83	21·2	doubtful.					millims.
83·6	21·9	not a breath.	175·2	506·5	458·2	1167·37	819
84	21·9	……	175·2	508·5	458·15	1167·46	820
89·5	22·4	……	174·44	509·2	457·25	1171·54	834
96·1	23·3	……	173·08	510·5	455·7	1179·10	853

TABLE XXXIII.

$W_2=597·1$. $W=635$. $R=809·849$. $c=1·388$.

88·2	− 7·1	saturated.					
90	− 3·65	……	299·57	293·95	257·5	921·53	1138
90·1	− 3·55	……	299·43	294·05	257·35	922·46	1139
92·9	− 3·15	−3·2	298·95	294·5	256·75	926·34	1151
93·2	− 3·25	−3·2	298·95				
95·8	− 2·85	measured during cooling.	298·46	294·8	256·1	929·95	1163
98·5	− 2·65		298·18	295	255·7	932·21	1174
95·1	− 2·95	measured during heating.	298·6	294·7	256·3	928·82	1160
97·1	− 2·95		298·6				
97·6	− 2·95		298·6	294·7	256·15	929·38	1169

The values for $\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$ are given in Table XXXIV.

TABLE XXXIV.

Limits of pressure.	Limits of temperature.	$\frac{100}{\phi} \cdot \frac{10\Delta\phi}{\Delta t}$.
millims.		
132– 149	43° –82°·5	+0·47
320– 363	58·6–98	+0·4
(„ – 830	„ –67·3	+1·2)
(330– 363	67·3–98	+0·16)
(342– 363	78·2–98	0)
764– 812	79·5–98	+0·9
778– 826	80·3–97·2	+0·8
(785– „	83 – „	+0·7
812– 858	82·1–97·7	+0·99
(835– „	89·3– „	+0·47)
819– 853	83·6–96·1	+0·8
1138–1174	90 –98·5	+1·36
(1151– „	92·9– „	+1·13)

with the numbers standing above each (which correspond to a series of observations) it is obvious that the behaviour of alcohol vapour totally differs from that of the two other vapours studied. The values of $\frac{\Delta\phi}{\Delta t}$, near saturation, are decidedly greater for the first degrees of further heating, in consequence of adhesion; for the higher intervals of temperature they partially become considerably smaller. We see further that even in the little pressures between 132 and 149 millims., where in my earlier investigations* I found the vapour still entirely in the gaseous state, a high positive value of $\frac{\Delta\phi}{\Delta t}$ is shown. The phenomena of adhesion, therefore, here completely mask the behaviour of the vapour.

On this account, in our further discussion of the results obtained, the alcohol numbers must be altogether neglected. Where adhesion comes into play, there we have to do with variable quantities of vapour, and therefore with ratios utterly impossible to calculate.

This leads us to a remark upon the investigation of steam (in the same direction) by Fairbairn and Tate. These experimenters, when experimenting on the specific volumes of steam from the lowest point of saturation upwards†, preferred small increments of heat (a few degrees Fahrenheit), and believed they had found coefficients of expansion of considerable magnitude; but in a subsequent memoir‡ they traced the relations more accurately, since even to themselves the previous results did not appear sufficiently reliable.

They thus arrived at this result—that, from the limit of saturation, steam heated within a space of constant dimensions exhibits for the first two degrees a considerably higher coefficient of expansion than air, while from that point upwards the coefficient is the same for steam and for air. Translated into my formula of calculation, this would signify that ϕ increases considerably with the temperature for the first degrees, and then becomes constant. I will leave out of consideration the fact that the second part of this proposition is not strictly the expression of their observations, and merely point out, with respect to the first part, that in their experiments the initial pressures of the steam amounted to less than half an atmosphere. This corresponds to initial temperatures of at most a few more than 70 degrees Celsius; and at such temperatures, according to my earlier experiments on steam*, adhesion always comes into play. Fairbairn and Tate's result is therefore itself quite intelligible, but throws no light upon the exact behaviour of steam. Moreover these experimenters would not possibly, by their method, have attained any results even for not adhering vapours; for they fall into the oft committed and as often exposed error of having the upper part of the pressure-measuring mercury column in the heated bath, and the lower part in the temperature of the room.

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* Pogg. *Ann.* vol. cxxxvii. p. 28.

† Phil. Trans. 1860, p. 185.

‡ Phil. Trans. 1862, p. 591.

§ 5.

Confining ourselves, then, to the consideration of the values of $\frac{\Delta\phi}{\Delta t}$ found for sulphide of carbon and chloroform, these hold, as we have said, for an approximately constant vapour-space. Now with low pressures the quotient $\frac{\Delta\phi}{\Delta t}$ appears $=0$; that is, there the superheated vapour experiences exactly the same pressure-expansion for constant volume as does the dry air with which it was compared. Hence the question mentioned at the commencement, and started by my earlier experiments, is now decided thus—that the superheated vapour under small pressures can certainly possess smaller coefficients of expansion for constant volume than 0·003663, so far as such smaller coefficients can belong also to dry air, but that a still smaller expansion of vapour is not to be admitted. According to Regnault†, however, dry air under these small pressures shows decidedly smaller coefficients than 0·003663—for example, with an initial pressure of 110 millims. the value 0·003648; so that my former results (which were directed only to comparison with the number 0·003663) are certainly hereby confirmed, and at the same time have the limits of their validity prescribed in the behaviour of dry air.

For higher pressures (that is, where at the boundary of saturation, according to my former experiments, vapour shows a considerable departure from the gaseous state) the quotient $\frac{\Delta\phi}{\Delta t}$ obtains truly a small, but yet an undeniable positive value. Consequently the pressure-expansion of vapour contained within a constant volume is there greater than that of dry air; and indeed we shall not be wrong if we suppose that, the greater the vapour-pressure becomes, so much greater is the positive value of $\frac{\Delta\phi}{\Delta t}$. This is sufficiently indicated by Tables XX. and

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[*] Pogg. *Ann.* vol. cxxxvii. p. 602.

[†] *Mém. de l'Acad.* vol. xxi. p. 110.

XXV.; and with this my earlier experiments are in accord: it must be remembered that the variations of vapour-density there found are, in their significance, inversely proportional to the variations of ϕ which we have now obtained.

It results further from the present observations (which often extended over a wide interval) that this slight variation of ϕ is by no means confined to the first two degrees of superheating above the point of saturation, but is, on the average, uniformly distributed over the entire interval. Of course this is to be understood thus—that the gradual transition of ϕ to its constant maximum value, the occurrence of which when the heating is continued within a constant space has just been made certain by our present experiments, only ensues very slowly. It is true that the positive value of $\frac{\Delta\phi}{\Delta t}$ will diminish and finally become *nil*, but the latter only with a very extreme degree of superheating.

The lower limit to which the superheating must certainly be carried, in order to arrive at constant values of ϕ, can be approximately fixed. For example, for a temperature of saturation of 90°, according to my previous formula the purely saturated vapour would have a density equal to $0·0595\sqrt{273+90}$ or 1·13 the constant density finally attained in the gaseous state. It is true that, for sulphide of carbon and chloroform, this formula was not pursued quite up to that temperature; but at 50° the ratio 1·07 was directly found; so that, according to all the experience then acquired, the above number for 90° is certainly not far from correct. Accordingly, for purely saturated vapour, the value of the function ϕ would be less than the constant value to which ϕ continually more and more approaches, in the ratio of 1 : 1·13. If now such vapour is heated in a constant volume, for every 10 degrees of superheating ϕ increases at the commencement, according to Tables XX. and XXV., to about 1·004 of its value. And since the value of $\frac{\Delta\phi}{\Delta t}$, in accordance with what has been observed, must subsequently become smaller, we may therefore say that, in order to attain the final constant value of ϕ, a superheating of more than $\frac{130}{4}.10$ degrees, or more than 325°, is here required.

In like manner for an extreme temperature of 50°, at which the total deviation of ϕ amounts to 1·07 its final value, Tables XX. and XXV. indicate on the average an initial increase of $\frac{\Delta\phi}{\Delta t}$ to 1·002 for 10° superheating. In this case, therefore, for the attainment of the gaseous state a greater superheating is required than $\frac{70}{2}.10$ or 350°.

We thus see that, wherever a sensible deviation of purely saturated vapour from the gaseous state has place, a very considerable elevation of temperature is necessary in order by superheating in constant volume to make it pass into the gaseous condition.

For practical application we hence obtain the not unimportant result, that, at least so long as the limits of these experiments are not exceeded (therefore to about 4 atmospheres pressure), only trifling errors are entailed by putting the coefficient of pressure-expansion for a constant volume simply equal to the coefficient of expansion of air, provided the limits of the temperatures under consideration are not too wide, say not more than 50°.

For the theoretical side of the question, however, such a trifling augmentation of ϕ is perfectly sufficient to cause the proportions to appear quite different from what they would be with ϕ constant. To see this it is best to keep in view the two directions in which I have followed the variability of the vapour-densities. The earlier experiments give the variableness which occurs when at a constant temperature the vapour expands its volume; the present when, the volume remaining constant, the temperature is raised. In both ways the gaseous state is gradually reached. Now in the latter this takes place with singular slowness. Nevertheless we can as yet only say further that, in the cases discussed, fully twice the absolute temperature of the vapour is necessary. Looking at the variations occurring when the first way is taken, my earlier experiments show that in individual cases an increase of the volume to four or five times may be required in order to attain the gaseous state.

Accordingly, if in

$$pv=\phi(273+t)$$

we regard ϕ as a function of v and t, we have the partial differential quotients $\left(\frac{d\phi}{dv}\right)_t$ for constant temperature and $\left(\frac{d\phi}{dt}\right)_v$ for constant volume to be equally taken account of, and must not neglect the latter on account of the small values of $\frac{\Delta\phi}{\Delta t}$ which have been obtained.

Lastly, the numerical value of the coefficient of pressure-expansion of vapours, which is not so directly to be learned from the variations of ϕ, may be fixed, and compared with the numbers given by Regnault for gases, in order to obtain points of support on this side also. For the coefficient of pressure-expansion with constant volume we have the well-known general formula

$$\alpha=\frac{\left(\frac{dp}{dt}\right)_v}{p-t\left(\frac{dp}{dt}\right)_v}.$$

Now, from the formula introduced for vapours

$$pv=\phi(273+t),$$

we obtain for constant volume

$$\left(\frac{dp}{dt}\right)_v=\frac{273+t}{v}\left(\frac{d\phi}{dt}\right)_v+\frac{\phi}{v}.$$

Therefore for vapours

$$\alpha=\frac{(273+t)\left(\frac{d\phi}{dt}\right)_v+\phi}{pv-(273+t)\left(\frac{d\phi}{dt}\right)_v t-\phi t};$$

that is, considering that $pv=\phi(273+t)$,

$$\alpha=\frac{1+\frac{273+t}{\phi}\left(\frac{d\phi}{dt}\right)_v}{273-\frac{273+t}{\phi}\left(\frac{d\phi}{dt}\right)_v t}.$$

When $\left(\frac{d\phi}{dt}\right)_v=0$, this value becomes $\frac{1}{273}=0{\cdot}003663$; for positive quotients of $\left(\frac{d\phi}{dt}\right)_v$ it is greater. Putting, for example, $t=90°$, and, as before, $\frac{1}{\phi}\left(\frac{d\phi}{dt}\right)_v=0{\cdot}0004$ (corresponding to Tables XX. and XXV., of which the values $\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}$ are now to be divided by 1000), we get

$$\alpha=\frac{1+0{\cdot}1452}{273-0{\cdot}1452\,.\,90}=0{\cdot}0044.$$

Under a pressure of about three atmospheres, to which in the case of sulphide of carbon this number would belong, Regnault* finds for carbonic acid the coefficient of pressure-expansion 0·0038. For carbonic acid, according to the formula

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* *Mém. de l'Acad.* vol. xxi. p. 112.

(equivalent to the above)

$$\frac{1}{\phi}\left(\frac{d\phi}{dt}\right)_v=\frac{273\alpha-1}{(1+\alpha t)(273+t)},$$

putting $\alpha=0{\cdot}0038$, we should obtain

$$\frac{1}{\phi}\left(\frac{d\phi}{dt}\right)_v=0{\cdot}00008,$$

or, corresponding to the values of the Tables,

$$\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}=0{\cdot}08.$$

Accordingly sulphide of carbon shows in this case only about five times the variableness of vapour-density shown by carbonic acid gas.

Similarly, for the temperature of 50°, using the mean values of the Tables for this temperature

$$\frac{100}{\phi}\cdot\frac{10\Delta\phi}{\Delta t}=0{\cdot}2,$$

with sulphide of carbon

$$\alpha=0{\cdot}00395.$$

We see that in all there are perfectly comparable deviations from the gaseous laws to which, in this direction, sulphide of carbon and carbonic acid are subject.

On the second coefficient to be mentioned in connexion with vapours, viz. the coefficient of space-expansion with constant pressure, after the present results at least something general must be said. For it the general formula holds:—

$$\alpha=\frac{\left(\frac{dv}{dt}\right)_p}{v-t\left(\frac{dv}{dt}\right)_p},$$

which, when

$$pv=\phi(273+t)$$

becomes

$$\alpha=\frac{1+\frac{273+t}{\phi}\left(\frac{d\phi}{dt}\right)_p}{273-\frac{273+t}{\phi}\left(\frac{d\phi}{dt}\right)_p t}.$$

When, now, from a determined initial point, ϕ is traced for heating in constant volume, we arrive at a higher final expression with the final temperature than when we reach the same temperature, after starting from the same point, under constant pressure. With constant temperature, however, according to my earlier experiments, ϕ augments as the pressure diminishes. Consequently for the same limits of temperature

$$\left(\frac{d\phi}{dt}\right)_p > \left(\frac{d\phi}{dt}\right)_v.$$

Hence the value of α is universally greater for constant pressure than for constant volume.

Physical Laboratory of the Polytechnic,
Aachen, June 5, 1872.

LIII. *On Duplex Telegraphy.* *By* Oliver Heaviside, *Great Northern Telegraph Company, Newcastle-on-Tyne**.

[With a Plate.]

DUPLEX TELEGRAPHY, the art of telegraphing simultaneously in opposite directions on the same wire, which was first performed by Dr. Gintl in 1853, and subsequently engaged the attention of so many inventors, until lately seemed never likely to be carried out in practice to any extent. According to the very practical author of 'Practical Telegraphy,' "this system has not been found of practical advantage;" and if we may believe another writer, the systems he describes "must be looked upon as little more than feats of intellectual gymnastics—very beautiful in their way, but quite useless in a practical point of view." However, notwithstanding these unfavourable reports as to the practicability of duplex telegraphy, the experience of the last year has negatived them in a striking manner, and made the so-called "feats" very common-place affairs. Circuits worked on a duplex system are now established in various parts of the United Kingdom—not to mention the United States, where the resurrection of these defunct schemes took place—and continue to give every satisfaction. There seems little reason to doubt that this system will eventually be extended to all circuits of not too great a length, between the terminal points of which there is more than sufficient traffic for a single wire worked in the ordinary manner—that is to say, only one station working at a time.

I propose in this paper to give a short account of the theory of duplex telegraphy by the principal methods, and to describe two other methods, which are, I believe, entirely original.

To begin at the beginning. Prior to 1853, it is said to have been the current belief of those best qualified to judge, that to send two messages in opposite directions at the same time on a single wire was an impossibility; for it was argued that the two messages, meeting, would get mixed up and neutralize each other more or less, leaving only a few stray dots and dashes as survivors (after the manner of the Kilkenny cats, who devoured one another and only left their tails behind). However, Dr. Gintl effectually silenced this powerful argument by going and doing it.

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* Communicated by the Author.

In order to be able to receive messages from another station, it is necessary for the receiving instrument to be in circuit with the line; and in order to send to another station, the battery must be in circuit. Hence, in order to receive and send at the same time, both the sending and receiving apparatus must be in circuit together. This can be arranged by making one continuous circuit between the two earths, and including the line and all the apparatus at each station. But if nothing further were done, the receiving instruments would be worked both by the received and sent currents; and if both stations worked at once, inextricable confusion would be the only result. Now, evidently, if the effect of the sent currents on the sending-station's instrument can be neutralized, the "feat" is accomplished. There are many ways of doing this. Dr. Gintl surmounted the difficulty in what was, to say the least, a very ingenious manner, although, from a modern point of view, it was decidedly clumsy. He made his key, while being depressed to send a current to the line through his own relay, at the same time close a local circuit, including a coil of wire outside the principal coils of the relay, in such a manner that the current in this local circuit (which contained an independent battery) circulated round the cores of the electromagnets in the opposite direction to the current going out to the line; and by placing a rheostat in this local circuit he was able to vary the strength of the local current, so that the effect of the out-going current on the relay was exactly neutralized. The relay then responded only to currents coming from the opposite station, which, of course, passed through the inner coils alone. Did both stations depress their keys simultaneously, the current in the batteries, inner coils, and the line was that due to both batteries; but in each relay as much of this current as was due to the corresponding battery was neutralized by the local current. The line-current might even be nothing, which would happen if each station had equal batteries and the same poles to earth. Then the relays would be worked entirely by the local current.

But local circuits are nuisances, and it is not to be wondered at that this method of Gintl's never came into practical use. But the possibility of the "feat" having been once demonstrated, it was not long before another and much superior method was introduced. It was discovered about the same time in 1854 by Frischen and Siemens-Halske, and may be called the *differential method.* It is represented in its simplest form in Plate VII. fig. 1. The relay at each station is wound with two coils of equal length; and the connexions with the battery and the line are made in the same manner as if each station were taking a test of the resistance of the line with the differential galvanometer. The resistance r, then, at station A equals the whole resistance outside station A; and r' at station B equals the whole resistance outside station B. Then, when the battery E is in circuit, as its current divides equally between the two coils of the instrument $g\,g$, the latter is unaffected; but that half of the current which passes to the line necessarily influences the instrument $g'\,g'$ at the other station, since the whole of it passes through one coil, and then divides between the other coil and the battery E′. Thus each station does not work its own relay, but only that of the opposite station, and the conditions of duplex working are satisfied.

It is upon this system that nearly the whole of the existing methods of duplex telegraphy are founded. As the object is to prevent out-going currents from working the sending-station's instrument, it is plain that there may be many modifications having for object the easier production of balances under different circumstances—as by varying the distance of one or both coils from the armature instead of altering the resistance r (fig. 1). There are also a few small points to be attended to before this system can be considered perfect. First, it is necessary for the external resistance to be as constant as possible, in order that the currents sent by a station (say A) may never affect its own instrument. But this external resistance includes B's apparatus; and B's battery is sometimes in and sometimes out of circuit. A variation in the external resistance will therefore be caused unless the transmitting apparatus is so arranged that a resistance equal to that of the battery is substituted for it when the latter is not in circuit. Again, there should be no interval of time during which neither the battery nor this equivalent resistance is in circuit. These things can generally be arranged with little difficulty. Thus, taking the case of the simplest transmitting instrument (the common Morse key), consisting of merely a lever with a front and back contact, the equivalent resistance may be connected with the back, and the battery-pole with the front contact; and the interval of disconnexion may be avoided by the use of suitable springs, or other means, by which the front contact is made just before (or practically at the same time as) the back contact is broken, and *vice versâ.* There will then be only a very much smaller interval of time during which the received currents can pass both through the battery and its equivalent resistance. The application of this to more complicated instruments (as, for instance, Wheatstone's automatic transmitter) is not at first sight so evident; but I have done it in a very simple manner, which it is unnecessary to describe. On long lines, or with high-speed instruments, an attention to these *minutiæ* is desirable; but on short lines and with common Morse apparatus they are superfluous.

It is not essential, though sometimes desirable, to use differentially wound instruments. Most telegraph instruments are constructed with two separate coils of wire, each on its own core. By connecting the battery to the wire joining these coils, we have a differential arrangement, and frequently all that is needed. In fact, if the armature is polarized, as in most relays, the result is the same as if they were differentially wound. With an unpolarized Morse direct-writer, however, the effect of the outgoing currents would not be completely neutralized. This is of little consequence, as the spring which draws the armature from the electromagnets may have a tension given it that only the received currents can overcome. The rheostats r and r' (fig. 1) may even be dispensed with and a direct earth-connexion substituted, provided the external resistance be not too great.

Quite recently another system has been brought forward, undeniably the most perfect, which may be called the *bridge duplex*, its principle being that of Wheatstone's bridge. To whom the idea first occurred of using this arrangement for duplex telegraphy is unknown to me. It has been claimed by Mr. Eden, of Edinburgh; but it has been patented by Mr. Stearns, of Boston, U.S., who also patents a number of plans, all depending on the differential system before described.

The arrangement for the bridge duplex is shown theoretically in fig. 2. a, b, c and a', b', c' are resistances, g and g' the receiving instruments, and f and f' the batteries. By the well-known law of the balance, when $a:b=c:d$, where d is the whole external resistance between station A and the earth at B, the electromotive force E will cause no current in g; and similarly for station B. The circumstance that the out-going currents do not pass through the receiving instruments is very important, as it allows any description of existing instruments to be used, and without any alteration. As in the differential plan, it is not always indispensable to adhere rigidly to the conditions which give theoretical perfection.

Although the signals sent by station A are only received at B, and *vice versâ*, and it is convenient to assume that the currents producing these signals actually come from the opposite station, yet it does not always happen that such is the fact. To take an extreme case. Let all the apparatus at each station, and

$2l=310$.

Distance to the centre.	1 plate.	2 plates.	3 plates.	5 plates.	6 plates.	8 plates.	12 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
155	12·5	22·1	28·0	35·5	37·0	38·4	37·8	35·4
145	8·5	15·0	20·7	30·7	30·2	30·5	34·0	32·3
125	4·0	7·6	16·7	21·2	24·3	23·5	25·0	24·0
105	1·5	4·5	10·2	15·0	18·0	18·5	18·0	16·9
85	0·6	2·0	7·5	10·0	12·7	15·6	14·2	10·8
55	……	0·6	2·5	5·0	7·1	7·0	7·6	4·5
35	……	……	1·4	3·0	3·5	3·2	3·0	1·4
15	……	……	0·2	0·5	0·5	0·7	1·5	0·8

$2l=250$.

Distance to the centre.	1 plate.	2 plates.	3 plates.	4 plates.	7 plates.	11 plates.	30 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.	grms.	grms.
125	14·5	24·6	28·2	28·6	31·6	27·6	29·6	29·6
105	10·0	12·7	16·2	18·7	18·2	19·7	19·9	20·9
75	3·0	7·0	8·6	11·2	11·2	11·2	11·2	10·4
25	……	1·0	1·5	1·4	1·4	1·4	1·4	1·2

$2l=200$.

Distance to the centre.	1 plate.	2 plates.	3 plates.	4 plates.	6 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.	grms.
100	10·0	20·0	26·5	25·0	25·0	24·0
90	6·0	13·5	13·5	20·5	20·8	19·4
70	3·2	8·9	11·2	12·4	13·0	11·7
50	2·0	5·0	7·5	6·9	7·0	6·0
20	0·4	1·0	1·7	1·4	1·5	0·9

$2l=100$.

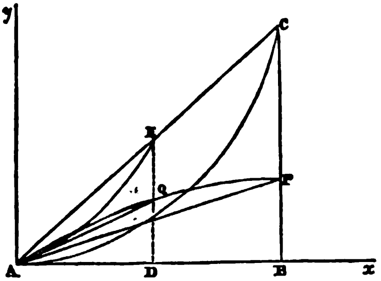
Distance to the centre.	1 plate.	2 plates.	3 plates.	5 plates.	Calculated.
millims.	grms.	grms.	grms.	grms.	grms.
50	7·6	9·7	11·4	12·7	12·0
40	4·7	7·0	7·7	7·8	7·6
30	3·4	5·5	5·5	5·5	4·3
20	1·5	2·5	2·0	1·8	1·06
10	…	1·5	0·4	0·4	0·4

III. It is difficult to say at what number of plates the maximum is reached, since it is only slowly arrived at; but it is evident that the number is greater the greater the length. Three or four are required for 100 millims., six to eight for 200, F is proportional to the magnetic intensity at each point, and that we can put $F=I^2$; then .

$$I=\frac{k}{\sqrt{l}}x, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$I_l=k\sqrt{l}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Equation (3), which may be written $I_l^2=k^2l$, shows that the magnetic intensity at the extremity of the normal pile varies as the ordinates of a parabola A Q P tangent in A to the y-axis; and from equation (2) we gather that the intensity on the different points of a bar of length $2l$ is figured by a right line which makes with the x-axis an angle whose tangent is $\frac{k}{\sqrt{l}}$. For $l=AB$, this line is A P; it would be A Q for a pile terminated at the point D.



VII. This total M is the area of the triangle A B P, or

$$l\times k\sqrt{l}=kl^{\frac{3}{2}}.$$

If a be the width of the plates, and e their thickness, and if we neglect the augmentations of intensity produced at the corners and angles of the pile, this quantity must be multiplied by the perimeter $2(a+ne)$, n being the number of plates; we have therefore

$$M=2(a+ne)kl^{\frac{3}{2}}.$$

VIII. When a contact is placed under the magnet, all free magnetism disappears if the contact is large enough and contains a sufficient quantity of iron. The whole of the magnetism M, therefore, is concentrated upon the surface of adhesion, which I name S. Its intensity there (that is, the quantity of magnetism on the unit of surface) is $\frac{M}{S}$; and the carrying-force will be $\frac{M^2}{S^2}$; for the whole surface it will be $\frac{M^2}{S^2}S$ or $\frac{M^2}{S}$.

It follows from this that the carrying-force is inversely as the surface of contact—which is correct, provided that all free magnetism has disappeared, but ceases to be true if S falls below certain proportions. It is on this account that cylindrical contacts are generally employed, and not plane ones. Substituting for M its value, the carrying-force P is

$$P=\frac{4(a+ne)^2k^2l^3}{S}.$$

IX. The weight of the magnet is equal to that of a plate, $2aeld$, multiplied by the number of plates, which is proportional to their length, and may be expressed by ml; it is therefore $\pi=2mael^2d$. Consequently the ratio of P to π, which measures the carrying-force as a function of the weight of the magnet, will be

$$\frac{P}{\pi}=\frac{2(a+ne)^2}{Smaed}k^2l,$$

or approximately, neglecting ne against a,

$$\frac{P}{\pi}=H\frac{al}{S}k^2.$$

This ratio will be proportional to the length and to the width of the plate, and inversely as the surface of contact.

X. There are two points which I have not examined in what precedes: they are the question of armatures, and the influence of the thickness of the plates. On the latter point I have ascertained the following:—

The power of a plate increases considerably with its thickness, but less rapidly than the thickness, so that there is a limit beyond which it remains stationary; but a plate of thickness 1 has less power than two others of $\frac{1}{2}$ thickness, which are much less powerful than three plates the thickness of each of which is $\frac{1}{3}$; and, generally, the difference increases with the number of the layers of which a pile of a given thickness is composed. I have thus been induced to employ ribands of steel; and as commerce supplies them abundantly and regularly, of excellent metal, I had only to superpose them in sufficient number to construct normal magnets and attain the extreme force, at the same time considerably diminishing the weight. I have thus obtained magnets carrying twenty times their own weight. I shall soon exceed this limit, thanks to the kind cooperation of M. Bréguet, and thanks also to my excellent and devoted foreman, Cyprien Bollé.

LV. *On the Law of Gaseous Pressure.* *By the* Hon. J. W. Strutt, *late Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

Absence from England has prevented my seeing Mr. Moon's last paper (Phil. Mag. Feb. 1873) until lately. I question whether there would be any use in continuing the controversy, as we seem to have hardly any common ground for argument; but Mr. Moon asks me one or two definite questions which I ought not to leave unanswered.

I am asked whether I still "consider that Boyle's law has been experimentally proved in the case of motion." If forced to give a categorical answer, I must say yes; for the simple reason that all the experiments on gases ever made relate to the case of motion. Since my first note on this subject, Mr. Moon has explained, or at least admitted my assertion, that the absolute velocity of a gas is not material; so that in fact by motion he means *relative* motion of the various parts of a gas. I am free to admit that the experiments by which Boyle's law is established do not extend to the case of relative motion.

Mr. Moon finds a *reductio ad absurdum* of the received law of pressure in an argument relating to the behaviour of air confined under a piston when a weight is placed on the latter, and contends that the fallacy consists in the false assumption of the received law. I took some pains to point out that it is not merely the truth or falsehood of Boyle's law (as extended) which is at issue, but that Mr. Moon's argument, if valid at all, goes the length of proving that it is impossible, without self-contradiction, even to conceive a medium whose pressure shall (under all circumstances) vary as the density. My position is that the fallacy or absurdity lies not in the premises, but in the argument, which I applied to prove that a body would not fall when its support is removed. Mr. Moon does not admit the parallelism of the two paradoxes, and asks me to point out exactly where the fallacy lies. To enter into a complete explanation would be to write a dissertation on the principles of the differential calculus, for which this is certainly not the place; but I may say that (in my opinion) the error lies in the omission of the word *finite.* There can be no finite change of pressure without a finite displacement, nor can there be a finite displacement without a finite change of pressure. This much is admitted; but it does not follow that there is never a finite displacement or change of pressure. In fact the quantities *become finite* *together*, or at least may do so, for all that this argument proves to the contrary.

I am, Gentlemen,
Your obedient Servant,
JOHN W. STRUTT.

May 15, 1873.

LVI. *On some Improvements in Electromagnetic Induction Machines.* *By* H. WILDE, *Esq.*[*]

[With a Plate.]

SOON after my announcement (in 1866) of the discovery that electric currents and magnets indefinitely weak could, by induction and transmutation, produce magnets and currents of indefinite strength[†], a number of electricians suggested other methods by which this principle could be exhibited and more powerful results obtained than those which I had described.

The most interesting as well as the most useful of these suggestions was to augment the magnetic force of the elementary magnet, by transmitting the direct current from the armature of a magneto-electric or an electromagnetic machine through wires surrounding its own permanent or electromagnet, in such a direction as to intensify its magnetism, until, by a series of actions and reactions of the armature and the magnet on each other, an exalted degree of magnetism in the iron or steel was obtained.

This idea seems to have occurred to several electro-mechanicians almost simultaneously in England, Germany, and America. In a letter to the 'Engineer' newspaper of July 20, 1866, Mr. Murray, after referring to my experiments, writes that he wishes to point out a variety of the principles embodied in the machine I had described, which, he says, is so obvious that it cannot fail to be hit upon by some inventor before long, and warns any one whom it may strike against patenting the idea, seeing that he had already constructed a machine upon the plan. Mr. Murray then states that "whereas Mr. Wilde, beginning with an ordinary magneto-electric machine, uses the current obtained from it to charge a powerful electromagnet, and from this obtains a second and more powerful current, which, used in like manner, produces one still more intense, I, using only a single machine, pass the currents from its armatures through wires coiled round the permanent magnets in such a direction as to intensify their magnetism, which, in its turn, reacts upon the armatures and intensifies the current."

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[*] Communicated by the Author, having been read at a Meeting of the Literary and Philosophical Society of Manchester, April 15, 1873.

[†] Proceedings of the Royal Society, April 26, 1866. Phil. Trans. vol. clvii. (1867). Phil. Mag. S. 4. vol. xxxiv. p. 81.

Whether it be that electricians are not in the habit of reading engineering journals, or that information communicated to them in the form of letters is lost in the plethora of printed matter of all kinds which engages the attention of the reading public, Mr. Murray's warning to inventors against patenting his idea has been disregarded, as a patent was taken out on December the 24th of the same year by C. and S. A. Varley for "improvements in the means of generating electricity," wherein is described a machine consisting of two electromagnets and two bobbins. The bobbins are mounted on an axle, on which also a commutator is fixed; the ends of the insulated wire surrounding the bobbins are connected with this commutator, and through it with the insulated wire of the electromagnets, forming the whole into one electric circuit. Before using the apparatus, an electric current is sent through the electromagnet for the purpose of securing a small amount of permanent magnetism in the iron core of the electromagnet. On revolving the axle, the bobbins become slightly magnetized in their passage between the poles of the electro-permanent magnets, generating weak currents in the insulated wire surrounding them. The effect of the current passing through the electromagnets is to increase their magnetism and magnetize in a higher degree the bobbins when passing between the poles of the electromagnets; and in this way the electromagnets and the bobbins act and react on each other, causing the circulation of increased quantities of electricity.

Another patent for the same idea was taken out by C. W. Siemens, F.R.S., on January the 31st, 1867, as a communication from Dr. Werner Siemens, of Berlin. The invention is described as having for its object the obtaining of powerful electric currents without the aid either of large batteries or of permanent magnets, by the following method:—A movable keeper or armature surrounded with a coil of insulated wire is arranged in front of the poles of an electromagnet; and after rotatory motion is imparted to the armature, a magnetic impulse is given to the electromagnetic arrangement by the momentary insertion of a galvanic battery into the circuit, which steadily and rapidly augments simultaneously with an increasing electric current in the coils. For reproducing a current after the machine is arrested no fresh impulse from the battery is needed, because the residual or permanent magnetism of the electromagnet is sufficient to commence inductive action.

Although private letters addressed from one person to another ought never to be received as evidence in questions affecting the priority of scientific discovery or invention, yet, for the purpose of showing the interest which attached to my investigations wherever science is cultivated, and also that when principles are once discovered the similar trains of reasoning of different persons lead to similar results, I will here mention that in the month of November 1866 I received a letter from Mr. Moses G. Farmer, of Salem, Mass., U.S.A., on the subject of my researches, and stating that he had built a small machine in which a current from a thermo-electric battery excites the electromagnet of my machine to start it, and after the machine is in action a branch from the magneto-electric current passes through its own electromagnet, and this supplies the magnetism required.

The next and last instance of the repetition of this idea to which I shall refer (though my list is not yet exhausted), is that communicated to the Royal Society, February 14, 1867, by Sir Charles Wheatstone, in a paper "On the Augmentation of the Power of a Magnet by the reaction thereon of currents induced by the Magnet itself"*. After pointing out that he had constructed the electromagnetic part of his machine according to my description, Wheatstone states that he first excites the electromagnet by any rheomotor, and, after removing it from the electromagnet, the circuits of the armature and electromagnet are joined to form a single circuit. The electromagnet, retaining a slight residual magnetism, is therefore in the condition of a weak permanent magnet. The motion of the armature occasions feeble currents in the coils thereof, which, after being rectified in the same direction by means of a rheotrope, pass into the coils of the electromagnet in such a manner as to increase the magnetism of the iron core; the magnet having thus received an accession of strength, produces in its turn more energetic currents in the coil of the armature; and these alternate actions continue until a maximum is obtained.

I have now enumerated, with some degree of tediousness and, to prevent misunderstanding, as nearly as possible as they have been described, instances where the idea of augmenting the force of a magnet by currents induced by itself has been repeatedly suggested. This enumeration I should have deemed somewhat unnecessary were it not that a prominent worker in science, whose genius and attainments entitle his opinions to a high degree of respect, has described the contrivance (in a manner to produce in the minds of those interested in education an erroneous impression) as a new principle in electric science, a great step in magneto-electricity, and the discovery of Messrs. Siemens and Wheatstone†.

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* Proceedings of the Royal Society, vol. xv. p. 369.

† Notes of a course of seven lectures on Electrical Phenomena and

That this contrivance suggested itself independently in different countries to the several experimenters above mentioned, there is as little reason to doubt as that the similar animistic ideas and customs found amongst the primitive races in various parts of the world are of independent origin; but such repetitions exclude the contrivance from the rank of a discovery in science; and it is, as Mr. Murray justly designates it, an obvious variety of the principles embodied in the machine I first described before the Royal Society.

At the time when this method of exciting an electromagnet was brought prominently forward by Messrs. Siemens and Wheatstone, I directed attention to the fact (which would seem to have escaped the notice of these electricians, as they omitted to mention it) that machines constructed as they had described them are incapable by themselves of producing powerful electric currents, as the whole energy of the machine is expended in exciting its own electromagnet*. Besides this, the actual amount of electricity circulating round the electromagnet is really very small; and when the circuit is opened for the purpose of applying the current to some useful purpose, the magnet immediately discharges itself and resumes its neutral condition till the continuity of the metallic circuit is reestablished.

While the current transmitted from the armature of a magneto-electric or an electromagnetic machine, as I have said, is incapable of directly producing powerful electrodynamic effects, such current may be usefully employed to excite the electromagnets of other machines in accordance with my original method. Some idea of the smallness of the quantity of electricity requisite for this purpose will be formed from the fact, that the full power of the 10-inch machine is developed when its electromagnet is excited by the current from four pint Grove's cells.

This machine has been in constant operation for some time past for the electro-deposition of metals from their solutions; and its electromagnet is now excited by its own residual magnetism in the following manner:—A small magnet-cylinder (3·5 inches diameter and 14 inches long) is bolted to the top of the 10-inch cylinder, so that the sides and axis of the former are parallel with the similar parts of the latter. The cylinders are separated for a space of three quarters of an inch by a packing of brass, and consequently act upon each other by induction through the intervening space, instead of by contact as in ordinary me-

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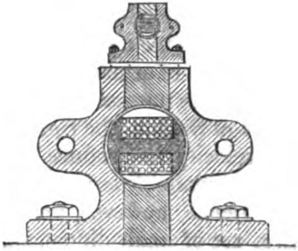
Theories, delivered at the Royal Institution of Great Britain, 1870, by John Tyndall, LL.D., F.R.S., "desired by persons interested in education."

* Proceedings of the Literary and Philosophical Society of Manchester, vol. vi. p. 103.



thods of magnetization. The annexed figure, representing an end view of the cylinders with the armatures in section, will make this arrangement of the cylinders pretty clear to those who are familiar with the construction of these machines.

The residual or permanent magnetism of the large electromagnet with its cylinder is very considerable, being many times greater than that of the four small permanent magnets with which it was originally excited*. The coils of the small armature are placed in connexion with those of the great electromagnet; and when the armature is rotated, the magnet-cylinders act and react on each other until the electromagnet is excited to the highest degree of intensity. By this arrangement of the armatures and cylinders, the minor current for exciting the electromagnet is kept distinct from the major current from the larger armature, which may be coiled for currents of high or low tension according to the purpose for which they are required.

It is essential for the attainment of a high degree of magnetism in an electromagnet excited by magneto-electricity, that the continuity of the armature and electromagnetic circuits should be preserved during the change of contacts from one segmental part of the commutator to the other. For this purpose the segments are made to overlap each other for a short distance, so that the metallic rubbers or brushes for taking off the current bear on adjoining segments simultaneously at the point of no current, and, in so doing, form two closed metallic circuits for a brief interval, which may be represented by the numeral 8, the upper part of the figure representing the armature circuit, and the lower part that on the electromagnet; but when the armature is at that part of its revolution when the current begins to rise in intensity, the coils of the armature and electromagnet form one continuous circuit, which may be represented by the cipher O. The importance of keeping the circuits closed in the manner described was not sufficiently observed in my earlier experiments, and necessitated the employment of much more powerful currents for exciting the electromagnets than were afterwards found to be necessary.

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[*] The small scale upon which my experiments have been repeated by physicists has in some instances given rise to the idea that the residual magnetism of an electromagnet is a lower degree of permanent magnetism than that which originally formed the basis of my augmentations.

So far as I have communicated the results of my investigations on the principle of accumulative action in electrodynamics, they have been obtained with machines designed with reference to the peculiar form of armature contrived by Dr. Werner Siemens, of Berlin. While possessing several advantages in point of efficiency over that of Saxton, the Siemens armature requires to be driven at a high velocity to produce a succession of currents sufficiently rapid to be available as a substitute for the voltaic battery. Little inconvenience, however, arises from the high speed when the armatures are of small dimensions; but as the dimensions increase it becomes necessary to lower the speed, and the large machines are consequently not proportionately powerful in comparison with the smaller ones. Besides this, the advantage possessed by this form of armature, in having the moving mass of metal near the axis of rotation, is neutralized as the dimensions increase, by the excessive heat generated by the magnetization and demagnetization of the iron. It would also be convenient in some circumstances to drive a machine direct from the crank or flywheel of a steam-engine without the intervention of multiplying gearing.

Considerations of this nature led me, towards the end of 1866, to propose to myself the construction of an electromagnetic machine with multiple armatures, which should remove the inconveniences inherent in those hitherto constructed by producing a greater number of currents for one revolution of the armature axis. Since that time I have been engaged, with more or less interruption, in carrying out this design, and have at length constructed a machine the performance of which surpasses all my previous essays in this direction in regard to power and efficiency, and with a considerable reduction in the quantity of the materials employed*.

The machine in which these results are embodied is represented in Plate VIII. figs. 1 and 2. In these views $A_{\prime\prime}$, $A_{\prime}$ are the two sides of a circular framing of cast iron, firmly fixed together by the stay rods $B_{\prime\prime}$, $B_{\prime}$ and the bridge $C_{\prime}$. A heavy disk, $D_{\prime\prime}$, of cast iron is mounted on a driving shaft $E_{\prime\prime}$ running in bearings fitted to each side of the framing. One of these bearings, $F_{\prime\prime}$, is carefully insulated from the framing by suitably formed pieces of ebonite, and also from the shaft by a cylinder of the same substance. Through the side of the disk and parallel with its axis sixteen holes are bored, at equal angular distances from each other, for the reception of the same number of cores or armatures, G,. The cores project about two inches through each side of the disk, and are held firmly in their places by screws tapped through its periphery. Around each inside face of the circular framing, and concentric with the driving-shaft, sixteen cylindrical electromagnets are fixed at the same angular distance from each other and from the centre of the shaft as the iron cores round the disk; the two circles of magnets consequently have their poles opposite each other, with the disk and its circle of iron cores revolving between them. The ends of the cores are terminated with iron plates of a circular form, which answer the double purpose of retaining the helices surrounding the cores in their places, and overlapping for a short distance the spaces between the poles of the electromagnets. The closing of the *magnetic* circuits of the electromagnets and armatures for a short distance, like the closing of the *electric* circuits for a brief interval at the point of no current, has a marked influence on the power of an electromagnetic induction machine,—both contrivances conspiring, simultaneously, to maintain the magnetic intensity of the electromagnets during the rise and fall of the magneto-electric waves transmitted through the helices.

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* To afford myself leisure to carry out my designs on a large scale without being involved in questions affecting the priority of my results, a general description of the improvements which form the subject of this paper was deposited in due form with the Commissioners of Patents, London, December 1866 and March 1867, and with the Ministère de l'Agriculture et des Travaux publics, Paris, June 1867.

The cylindrical bar magnets are each coiled with 659 feet of copper wire 0·075 of an inch in diameter, insulated with cotton; the helices are grouped together to form a fourfold circuit 2636 feet in length, and are joined up in such a manner that adjacent magnets in each circle, as well as those directly opposite in both circles, have north and south polarity in relation to each other. A charge of permanent magnetism was imparted to the system of electromagnets by the current from a separate electromagnetic machine.

The armatures, although formed of sixteen pieces of iron, are, by projecting through both sides of the disk, thirty-two in number. The length of insulated wire on each armature is 116 feet; and the thickness is the same as that on the electromagnets. These helices are divided into eight groups of four each, and coupled up for an intensity of 4 × 464 feet. One of the groups is used for producing the minor current for exciting the circle of electromagnets, while the remaining groups are joined together for a quantity of seven, and an intensity of four for the production of the major current from the machine. The aggregate weight of wire on the electromagnets is 356 lbs., and on the armatures 26 lbs. The helices for exciting the electromagnets are connected with the commutator H, while those producing the major current are placed in connexion with the rings I, K, or in place thereof with another commutator, according as the alternating or the direct current from the machine is required.

The strength and proportions of the several parts of the machine enable it to be driven with advantage from 300 to 1000 revolutions per minute.

At the medium velocity of 500 revolutions per minute the major current will melt eight feet of iron wire 0·065 of an inch in diameter (No. 16, B. W. G.), and will produce two electric lights in series, each consuming carbons half an inch square at the rate of three inches per hour.

When driven at a velocity of 1000 revolutions (equivalent to 16,000 waves) per minute, the current will fuse twelve feet of iron wire 0·075 of an inch in diameter (No. 15, B. W. G.). As soon as the heating or fusing of an iron wire of a given length and section comes to be an acknowledged measure of powerful electric currents, as well as a method of comparison between the power of electromotors of different kinds, as it must ultimately be, the significance of this result will be fully realized.

At the velocity of 1000 revolutions per minute the light from two sets of carbons in series is unendurably intense, as well as painful to those exposed to its immediate influence. Estimated on the basis afforded by the performance of the excellent magneto-electric-light machines of MM. Auguste Berlios and Van Malderen, who have made a careful study of the photometric intensity of the electric and oil lights, the power of the new machine is equal to that of 1200 Carcel lamps, each burning 40 grammes (1·408 oz. avoird.) of oil per hour, or of 9600 wax candles. The amount of mechanical energy expended in producing this light is about 10 indicated horse-power.

A comparison between the power of the new machine and that of the 10-inch machine will show that while the current from the former fuses twelve feet of iron wire 0·075 of an inch in diameter, the current from the latter fuses only seven feet of wire 0·065 of an inch in diameter, and is consequently only about half as powerful as that from the new machine. Besides this, the quantity of copper used in the construction of the new machine is about 3½ hundredweight, and of iron 15 hundredweight, while the weight of these metals in the 10-inch machine is 29 and 60 hundredweight respectively. In other words, we have in the new machine a double amount of power with less than one fourth the amount of materials employed in the construction of the 10-inch machine.

Another advantage possessed by the new machine is the great reduction of temperature in the armatures by their rapid motion through the air, which acts much more efficiently than the circulation of water through the magnet-cylinder. By increasing the diameter of the electromagnetic circles conjointly with the number of electromagnets and armatures, the angular velocity of the machine may be so diminished that it may be driven directly from the crank of a steam-engine, concurrently with an increase of electric power proportionate to the number of electromagnets and armatures in the electromagnetic circles.

While the excitation of the electromagnets of the machine by the current from several of its armatures is attended with some advantages where portability is required, yet, as provision has to be made for keeping the major and minor currents separate from each other, the commutator arrangements become somewhat complicated, and faults in either of the circuits are not so readily localized as when a separate exciting machine is employed. In those cases, therefore, where conveniences for driving separate machines are at hand, and when the power of several of them is required simultaneously, as in large electro-depositing establishments, some advantage will be gained by using a separate machine of suitable power to excite the electromagnets of several machines, when the currents from the whole of them may be utilized, and a commutator on the axis of each machine will be dispensed with.

In my paper "On a Property of the Magneto-electric Current to control and render Synchronous the Rotations of the Armatures of a number of Electromagnetic Induction Machines"*, I stated that this property would be available when the machines were used for the electro-deposition of metals from their solutions. It has, however, been found that the small resistance presented by depositing solutions to the passage of the currents prevents this property from manifesting itself (in accordance with what I stated in my paper respecting the effect of joining the poles with a good conductor); and it is only when the machines are employed for the production of electric light or other purpose where the external resistance is considerable, that this electromechanical function of the current comes into useful operation.

Before concluding my description of this further development of the principle of electromagnetic accumulation, I consider it a duty I owe to myself as well as to science that I should not allow to pass unnoticed the views and statements of certain writers respecting the place and value of my investigations in the history of natural knowledge. The peculiar good fortune which enabled me to follow up the discovery of a great principle to such brilliant results has contributed, accidentally in some instances, to establish the idea that these results are an expansion of Faraday's discovery of magneto-electricity rather than a distinct step in electrical science. A brief glance at the history and progress of electricity and magnetism will suffice to show

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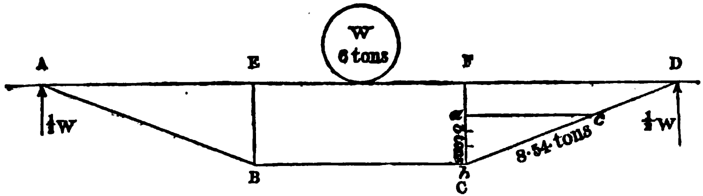
* Proceedings of the Literary and Philosophical Society of Manchester, December 15, 1868. Phil. Mag. S. 4. vol. xxxvii. p. 54.

$$\frac{\text{cube of length in feet} \times \text{load in lbs.}}{\text{breadth in inches} \times \text{cube of depth in inches} \times \text{constant}}.$$

The Table registers the "constant" needed for reducing this formula to numbers for different materials. Now this constant is merely Young's "modulus" in disguise; *e. g.*, in the case of wrought iron the registered "constant" is 64,221, which is equivalent to a "modulus" of nearly 28,000,000 lbs. per square inch. There are several objections to this way of treating the deflection of beams; the chief is that it leads the learner to regard deflection as a subject by itself, whereas it is closely connected with simple extension and compression, produced by moderate forces. A reader who had not obtained the knowledge elsewhere would not suppose that there was any connexion between the constant just mentioned as given in Table 39, and the experimental results registered in the early part of Table 8.

There is another point which may be noticed, and which will be best introduced by extracting a part of p. 209. The figure "is," says the author, "a skeleton diagram of the form of trussed beam used for overhead travelling cranes..... To ascertain the strain upon



the top beam of such a structure, which is generally of timber, find the weight acting at the centre; multiply that weight by half the span of the truss, and divide by the depth of the truss..... Let the beam be 20 feet span, and required to carry 6 tons at the centre; the depth of the truss is usually made $\frac{1}{8}$ of the span, and in this example is 2 ft. 6 in. The strain along the top bar will be $10 \times 6 \div 2\frac{1}{2} = 24$ tons. The compressive strength of timber may be taken at 6000 lbs. per square inch, and the safe working stress at $\frac{1}{10}$ of that amount, namely 600 lbs., and the sectional area of the material will be $24 \times 2240 \times 600 = 89\frac{3}{5}$ sq. in." The author adds that the stress on C D is 8·54 tons. We have no objection to make to the conclusion that about 90 square inches is a proper section for the beam for practical purposes; nor will we do more than notice that it is a little hard on the reader to expect him to find out for himself the reason of the rule by which the result is obtained. But we think the author ought to have noticed, and perhaps to have insisted on the point, that it is highly improbable that the stresses will have in reality the values assigned to them. To show this we will take a rather extreme case, and suppose that the beam is 3 in. wide and 30 in. deep, that A B and C D have nuts at the end by which they can be slightly lengthened and shortened, and that the weight of the truss can be neglected. If the beam were not trussed, the deflection at the middle point would be, according to the timber employed, between a half and a quarter of an inch, and at E and F slightly less. If, then, we suppose A B and C D lengthened sufficiently to allow of the points E and F drooping to this extent, the compressive stress along A D instead of being 24 tons, will be exactly nothing, except so far as the upper part of the beam is compressed in the act of bending. If, now, A B and C D are gradually shortened, the component members of the truss will be brought into a state of stress, the amount of the stress being determined by the degree in which the droop at E and F is reduced. At each turn of the nuts the stresses will be varied; and when the operation is completed, the amounts of these stresses will in themselves be determinate, but will depend upon the extent to which the process of screwing up has been carried. If two such trusses were placed side by side, under apparently similar circumstances, it is quite possible that their actual states of stress might be materially different.

In spite of these defects, and some others which our limits will not allow us to notice, the book contains very much that is valuable, and we can cordially recommend it to the notice of students of mechanical science.

British Rainfall, 1872. *Compiled by* G. J. SYMONS. London: E. Stanford, Charing Cross.

The well-earned character of this very useful annual volume is fully maintained in the present issue, and not only so, but it contains three features of unusual interest—an important article on the measurement of snow, an examination of the alleged periodicity of rainfall synchronous with the period of sun-spot frequency, and a comparison of rainfall in 1872 with the three years of great rainfall, 1848, 1852, and 1860. In the article on the periodicity of rainfall, Mr. Symons enters at some length into the researches of Messrs. Meldrum and Lockyer, and quotes an elaborate paper by Dr. Jelinek to the effect that taking a broad view of the subject, the evidence is conflicting—a result that Mr. Symons had previously arrived at. Dr. Jelinek urges the importance of obtaining still further materials for estimating the connexion between sun-spot frequency and depth of rain, and advises that the consideration of the data should be proceeded with quietly and without prejudice. At the close of the article Mr. Symons gives a list of some links in the chain which binds us to our central source of light and heat, in which we find "auroræ accordant with sun-spot prevalence." This reminds us that there is another link not mentioned by Mr. Symons, viz. the diurnal inequality of magnetic declination. Professor Loomis has furnished the 'American Journal of Science and Arts' with an interesting paper on the synchronous variations of the three phenomena—sun-spot frequency, mean diurnal range of magnetic declination, and auroral displays, from 1776 to 1872, illustrated with the curves of each. From the third feature above quantities for that year, as compared with the other three, fell at fifty-six stations out of eighty-two, of which forty-five gave the maxima for the years 1848 to 1872. In the year 1852 sixteen stations furnished the greatest falls, fifteen being maxima from 1848 to 1872. In 1848 the largest falls were at eight stations, five being maxima as above. In 1860 there were only two stations at which the falls were the greatest for the four years named; there were no maxima at any of the eighty-two stations in 1860. Mr. Symons gives a supplementary Table of seventeen stations, at each of which the maximum rainfall occurred on other years than the four wet years discussed. The figures quoted from, and the quantities recorded in the Table of comparison mentioned show that in the majority of cases 1872 was *generally* the wettest year on record.

Weekly Weather Reports issued by the Meteorological Office.

We are glad to find that the Meteorological Office has resumed the issue of Weekly Summaries of the Weather in Western Europe. It is intended to accompany the chart issued on Wednesdays by a summary of the weather during the previous week, so that each day's map will be more readily intelligible when examined by the light thrown upon it by the summary, the course of a depression more easily traced, and the general character of the weather much better understood than it can be from the mass of figures comprised in the daily reports. There is, however, one feature which we are desirous should be introduced; it is a publication of monthly barometric curves at selected pairs of stations some distance apart from each other, as, for example, Haparanda and Biarritz, Christiansund and Scilly, Cuxhaven and Valencia, Toulon and Stornoway. If each pair of curves were projected on the *same* line of abscissæ, the ordinates would show at a glance the direction and amount of the gradients for the period of projection, say, one month. We have no doubt that one or two pairs of curves introduced monthly would greatly facilitate certain meteorological inquiries, and indicate the course to pursue in reference to other stations.

LX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 394.]

December 4, 1872.—Prof. P. Martin Duncan, F.R.S., V.P., in the Chair.

THE following communications were read:—

1. "On the Tremadoc Rocks in the neighbourhood of St. David's, South Wales." By Henry Hicks, Esq., F.G.S.

The author stated that Tremadoc rocks occur in three distinct places near St. David's, namely in Ramsey Island, at the north end of Whitesand Bay, and over a considerable tract of country about 5 miles east of St. David's. They rest conformably on the Lingula-flags, and are about 1000 feet thick in Ramsey Island. The author noticed the fossils found in these deposits, nearly all of which are of new species, and stated that the palæontological evidence proves these rocks to be nearly allied to, if not identical with, the lower part of the Tremadoc rocks of North Wales. The Upper Tremadoc rocks of North Wales seem to be represented at St. David's by the so-called Arenig rocks which overlie the deposits described in the present paper. The new species described by the author are as follows:—

Neseuretus (g. n. Trilob.) *ramseyensis, quadratus, recurvatus,* and *elongatus.*
Niobe menapiæ and *solvensis.*
Theca Davidii.
Bellerophon ramseyensis and *solvensis.*
Palasterina ramseyensis.
Dendrocrinus cambrensis.
Ctenodonta menapiensis and *cambrensis.*
Palæarca Hopkinsoni and *oboloidea.*
Glyptarca (g. n.) *primæva* and *Lobleyi.*
Davidia (g. n.) *ornata* and *plana.*
Modiolopsis ramseyensis, Homfrayi, solvensis, and *cambrensis.*

He also noticed the occurrence in the deposit of *Lingulella Davisii,* M'Coy, *Lingula petalon,* Hicks, *Obolella plicata,* Hicks, *Orthis Carausii,* Salt., and *menapiæ,* Hicks, and *Eophyton explanatus,* Hicks.

2. "On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire." By the Rev. O. Fisher, M.A., F.G.S.

The author stated that this paper was founded upon one read by him before the Society in May last, but subsequently withdrawn, in consequence of his obtaining information which necessitated a change of opinion upon certain points. The new portion related chiefly to those nodules which had been regarded as belonging to *Porospongia* or *Scyphia,* the fenestrated structure shown in sections of which the author now identified with the structure of *Ventriculites,* as described by Mr. Toulmin Smith, the whole arrangement, and especially the presence of an octahedral figure at the nodes where the fibres of the framework intersect one another, being in favour of this determination. The author described the peculiarities of these octahedra, and dwelt particularly upon the fact that these sections of phosphatic nodules showed clearly that the fibres are really tubular, and not, as Toulmin Smith supposed, solid.

3. "On the Ventriculitidæ of the Cambridge Upper Greensand." By W. Johnson Sollas, Esq., Associate of the Royal School of Mines, London.

A collection of supposed sponges found in the Cambridge Upper

Greensand had been in part referred to the genera *Scyphia* and *Porospongia*, and in part left unidentified. An examination of sections of these forms by the microscope had revealed all the details of Ventriculite structure; and a careful comparison with Mr. Toulmin Smith's descriptions and specimens had resulted in the identification of those examined with some four of Mr. Smith's species; thus *Scyphia tessellata* was shown to be equivalent to *Ventriculites tessellatus* (or, more correctly, *V. texturatus*), *Porospongia ocellata* to *V. cavatus*, and other unnamed forms to *V. quincuncialis* and *V. mammillaris* respectively. The occurrence of ventriculite-structure in coprolitic material presents a favourable opportunity for a fresh inquiry into its nature; accordingly the author described the minute characters of the hexaradiate elements of which the skeleton is composed, and the combinations of these hexradiates with one another. Abnormalities occur sometimes by the hexaradiates becoming heptaradiate or pentaradiate, and sometimes by some of their rays bending quite away from their normal course. The whole of the skeleton fibre is distinctly tubular. Since the Ventriculite fibres have now been found fossilized in chalk, flint, and calcic phosphate, there can be little doubt that they were keratose, and not siliceous in their nature. If this be so, we have a difference between Vitrea and Ventriculitidæ of ordinal value at least, and we must look for allies to the Ventriculites among the horny sponges. *Verongia* resembles Ventriculites in the simple hollow cavity of its fibre and the nonspiculate character of its skeleton; *Darwinella* offers a resemblance in its hexaradiate horny spicules, and *Spongionella* in the regular arrangement of its fibres. These three genera are indices of the directions in which the Keratosa tended to vary. At a very early period great variation occurred among the Keratosa, which already, at the time of the Weisse Jura, had evolved such highly symmetrical specialized forms as the Ventriculites. These, with their contemporary variations, such as Verongioid forms, lived on in great numbers throughout the Mesozoic period, with the close of which the Ventriculites altogether disappeared; while their nearest allies dwindled down to the dwarfed and rare genera *Verongia*, *Darwinella*, and perhaps *Spongionella*.

LXI. *Intelligence and Miscellaneous Articles.*

AMERICAN ASTRONOMY.

APPENDIX IV. of the 'Washington Observations' for 1871 contains a most interesting account of the Founding and Progress of the United-States Naval Observatory by Professor Nourse, in which the earliest steps taken towards the establishment of an observatory, the difficulties encountered during a period of forty years in carrying out the design, the meteorological work under the superintendence of the late Commander Maury from 1844 to 1861, the devotion of the capabilities of the establishment to purely astronomical work during the ten years 1861–1871, and its present equipment and *personnel* are fully set forth. The original germ of the observatory first came into existence in connexion with an occultation of η Tauri observed near the President's house on October 20, 1804, from which Mr. Lambert determined the approximate longitude of the capitol to be 76° 53′ 6″·920 west from Greenwich. It was between five and six years after the occurrence of this occultation that a memorial was presented to the House of Representatives urging the establishment of a *first meridian* at Washington. From the presentation of a report on this memorial to the final establishment of the observatory and its entering on its work in 1844, the reader is presented with a variety of interesting documents relative to the negotiations for this great national object, which may be read with great advantage by all who cultivate astronomy. The following extracts selected from amongst these documents will convey to the reader an idea of the appreciation of the high value of astronomy by the American mind.

"The express object of an observatory is the increase of knowledge by *new discovery*. The physical relations between the firmament of heaven and the globe allotted by the Creator of all to be the abode of man are discoverable only by the organ of the eye. Many of these relations are indispensable to the existence of human life, and perhaps of the earth itself. Who can conceive the idea of a world without a sun, but must connect with it the extinction of light and heat, of all animal life, of all vegetation and production, leaving the lifeless clod of matter to return to the primitive state of chaos, or to be consumed by elemental fire? The influence of the moon, of the planets—our next-door neighbours of the solar system—of the fixed stars scattered over the blue expanse in multitudes exceeding the power of human computation, and at distances of which imagination herself can form no distinct conception; the influence of all these upon the globe which we inhabit and upon the condition of man, its dying and deathless inhabitant, is great and mysterious, and, in search for final causes, to a great degree inscrutable to his finite and limited faculties. The extent to which they are discoverable is, and must remain unknown; but to the vigilance of a sleepless eye, to the toil of a tireless hand, and to the meditations of a thinking, combining, and analyzing mind secrets are successively revealed, not only of the deepest import to the welfare of man in his earthly career, but which seem to lift him from the earth to the threshold of his eternal abode, to lead him blindfold up to the Council Chamber of Omnipotence, and there stripping the bandage from his eyes, bid him look undazzled at the throne of God."

"It is to the successive *discoveries* of persevering astronomical observation through a period of fifty centuries that we are indebted for a fixed and permanent standard for the measurement of time; and by the same science has man acquired, so far as he possesses it, a standard for the measurement of space. A standard for the measurement of the dimensions and distances of the fixed stars from ourselves is yet to be found; and if ever found, it will be through the means of astronomical observation. The influence of all these discoveries upon the condition of man is, no doubt, infinitely diversified in relative importance; but all, even the minutest, contribute to the increase and diffusion of knowledge. There is no richer field of science opened to the exploration of man in search of knowledge than astronomical observation; nor is there, in the the opinion of this Committee, any duty more impressively incumbent upon all human governments than that of furnishing means and facilities and rewards to those who devote the labours of their lives to the indefatigable industry, the unceasing vigilance, and the bright intelligence indispensable to success in these pursuits."

From the time Commander Maury entered on his duties to the present, the course of the "Naval Observatory" has been one of onward progress. The meteorological work was great; but a greater astronomical work was proposed, that of assigning to every star in the heavens that could be seen with the observatory instruments its colour, position, and magnitude. This noble work was begun and carried on until 1849, when, the corps of computers proving altogether insufficient to keep pace with the observers, it was discontinued. Nevertheless much valuable work has been accomplished since, as a reference to the volumes and reports will show.

We cannot close this notice of the progress of astronomical science in the United States, at one observatory only, without referring to the contemplated extension of optical power in the erection of a refracting telescope of good definition and of 26 inches clear aperture, now in course of construction by Messrs. Alvan Clark and Sons. This instrument is to be completed within four years of the date of contract (June 30, 1871), or thereabouts. Doubtless in the hands of such zealous observers as constitute the staff of the observatory it will do good service to astronomical science, and contribute in no small degree to uphold the position now accorded to the United-States Naval Observatory by the spontaneous verdict of the scientific men of the Old World.

ON THE SUDDEN COOLING OF MELTED GLASS, AND PARTICULARLY ON "RUPERT'S DROPS." BY V. DE LUYNES.

The bursting produced in "Rupert's drops" the moment the thin end is broken off has been hitherto attributed to the state of forced dilatation of the interior. It is supposed that the external layer, suddenly solidified by cooling, while the inner portions are still hot and much expanded, compels the latter, to which it remains adherent, to retain a volume greater than that to which they would be reduced if the whole drop had been cooled slowly; hence a state of unstable equilibrium, which is only maintained by the resistance of the outer layers; so that when this resistance is destroyed by breaking off the thin end or another portion of the drop, the state of equilibrium ceases and bursting takes place.

The experiments whose results I have the honour to present to the Academy seem to prove, on the contrary, that the effects in question are chiefly due to the peculiar condition of the exterior layers, and that the interior play no part, or only a secondary part, in the phenomenon.

The mechanical actions by means of which the drops are ordinarily broken, necessarily produce vibrations in the glass, the effect of which it is impossible to appreciate. That is why, in this investigation, I have preferred to make use of fluorhydric acid, the action of which can be moderated at pleasure, and which permits us to destroy at will, and without any shock, any portion we wish to attack.

On suspending a Rupert's drop by a thread over a platinum vessel containing fluorhydric acid, in such a manner that the extremity of the thin end dips into the liquid, we find that we can always dissolve the whole of the thin end without destroying the drop; but when the acid touches the origin of the neck (that is, the point of divergence of the pear), equilibrium is always broken; the drop then separates into a great number of fragments, and in most instances without explosion.

Reciprocally, the swollen part may be immersed in the acid, the origin of the neck and the whole of the thin end being kept out of the liquid; in this case the drop is completely dissolved without rupture, and the thin end remains intact. If with different drops the experiment is arrested at different stages of the dissolution, it is found that the nucleus which remains presents no longer the properties of the original drop; it no longer breaks up when the thin end is broken off—which shows clearly that the interior mass of the glass does not intervene in the phenomenon.

These two experiments prove at once that the stability of the drop is bound up with the existence of the origin of its neck, since whenever it is preserved no disaggregation of the drop takes place.

Now it is known that chilled glass remains more expanded than if it had been cooled slowly; the exterior layers of the drop, more strongly chilled, are more expanded than the interior layers, which have occupied more time in cooling. We may therefore regard the drop as formed by the superposition of layers of glass unequally chilled and expanded, cemented to one another. The exterior layers, kept by the resistance of the interior ones, can only yield to the force of elasticity which solicits them if, through any cause whatever, they are all at the same time set free to return to their normal state of expansion.

It results, moreover, from the form of the drop, that all these layers, unequally stretched, meet together at the origin of the neck; so that on destroying this the common point of resistance vanishes, and these layers, the actions of elasticity of which are added together, are displaced along the same directions and produce the disaggregation of the system.

If this supposition is correct, one might make a drop burst by cutting it at the large end, in such a manner as to set free at one of their extremities the unequally chilled layers of glass. This is, in fact, what takes place when the large end of the drop is ground off in a lathe, or when we attempt to saw it; the explosion always takes place as soon as a little more than half of the thickness has been attacked.

Another consequence of this hypothesis is, that the vitreous molecules will be displaced in a direction different according to the manner in which rupture takes place. This displacement should be greater for the exterior than for the interior layers, which are less expanded; and it is almost *nil* for the central portions, which have not been chilled at all or but very little. Consequently, on considering the molecules in a plane transverse section of the drop, the eccentric molecules, belonging to the more expanded layers, will be more displaced than those situated nearer to the centre, which will be less and less so as they are further from the surface; so that after the rupture this plane section will have the appearance of a conic surface formed of little needles of glass, arising from the shrinkage on all sides; and the direction of the summits of these sorts of cones will indicate that in which the displacement of the molecules has taken place.

If the thin end be broken off, the shrinking will be towards the bulb, and all the summits of the cones will be directed towards the thin end. If the explosion be caused by sawing the big end of the drop, the cones will have their summits directed towards the big end.

That this is what in fact happens I have ascertained by operating in the following manner. I fit some Rupert's drops in plaster, covering only a little more than half of their thickness. The thin end, which is left protruding, I immerse in fluorhydric acid. The moment the neck is attacked the drop is disaggregated, with or without explosion; and the fragments constitute, by their grouping, a series of conic assemblages encased one within another and having their summits towards the thin end. On sawing the big end, the summits have the opposite direction; and if the drop is sawn in the middle, the two opposite arrangements are observed on the two sides of the incision. Operating while the plaster is fresh, we can easily detach the fragments of the drop and establish all the results I have stated.

These facts demonstrate that in the drop the glass is in no peculiar condition other than that which arises from the unequal expansion resulting from the difference of cooling.

Analogous phenomena are presented by thick glass rods which are chilled naturally by cooling in the air at the moment of their fabrication. When these rods (which have always some curvature) are heated at one end, it sometimes happens that they break along their whole length, the fracture being conic and acicular. I am indebted to the kindness of M. Friedel for a fine specimen of this kind. The tubes obtained by letting melted glass flow in threads of more or less thickness into water possess in a high degree the explosive properties of Rupert's drops. They have almost always the form of cork-screws, on account of the extreme expansion of the upper layers; and dipping the extremity in fluorhydric acid is sometimes sufficient to cause an instantaneous explosion, with the same characters in the fracture. In fine, the lumps of glass which remain at the extremities of the canes by means of which the tubes are drawn have the form of large Rupert's drops, and are of considerable weight. When these are detached from the cane they are in the condition of a drop of which the bulb has been sawn: during their cooling in the air they break up, throwing off splinters with violence; and their fracture is identical with that of the small drops broken at the big end. A fragment of one of these large drops, which I had brought from the works of MM. Appert at La Villette, presented an interesting phenomenon: on slightly squeezing it between my finger and thumb, a considerable disengagement of heat was produced; the temperature rose to about 40° C. This confirms the results obtained by M. Dufour concerning the heat disengaged during the explosion of Rupert's drops.

The existence of layers cooled with unequal suddenness in the thickness of the glass affords an explanation of the brittleness of chilled glass. In fact we may suppose that, on account of the low heat-conductivity of glass, a very thin layer at the surface is, from whatever cause, cooled with sufficient suddenness to be in a very different state of expansion from that of the layers beneath. The least shaking, or the slightest change of temperature, will cause it to break; and the fissure will be propagated in the mass of the glass: exactly the same thing takes place when a crack shows itself on pottery the glaze of which has been ill-compounded.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxvi. pp. 346–349.

ON A METHOD OF MEASURING INDUCED CURRENTS.

BY F. H. BIGELOW.

If a Wheatstone's bridge be formed, in which the secondary coil of the inductorium is the resistance R_1 to be measured, the relation between the resistances will be expressed by the proportion $R_1 : R_2 = R_3 : R_4$, where R_3 and R_4 are in a fixed ratio. By passing a current from an independent battery through the primary coil, the strength of the current in R_1 will be increased or diminished on the breaking or making of the inducing current. Therefore, to preserve equilibrium in the bridge, R_1 must be changed, and we shall have $R_1 \pm C : R_2 \pm x = R_3 : R_4$, in which x is a resistance equivalent to the effect of the induced current. Let $R_1 \pm C$ be found by trial such that the addition or subtraction of the induced current will produce equilibrium in the bridge—that is, will be such as to bring the deflection of the galvanometer to the zero of the scale. We shall then have the strength of the induced currents expressed as resistances. One advantage of this method is this—that the readings are always reduced to the same point, the zero of the scale. This method also has a wider range than that of merely taking the swing of the galvanometer-needle; for currents which would throw the spot of light in a reflecting galvanometer off the scale can be readily kept on the scale in this method by merely altering the ratio of R_3 to R_4 in the Wheatstone's bridge. Since the shunting of induced currents is accompanied with difficulties, this method is especially advantageous. When the bridge was set up so that the smallest variation in the resistance of the branch containing the inductorium gave the greatest variation in the current going through the galvanometer, namely when $\frac{dS_0}{dR_3}=0$, S_0 being the current through the galvanometer, and the resulting value of R_3 being $R_3=\sqrt{\frac{GR_1(2B+R_1)}{G+2R_1}}$, in which G is the resistance of the galvanometer, B that of the circuit exterior to the Wheatstone's bridge, it was found that the induced currents could be measured to one hundred-thousandth of an ohm.

The following Table contains a comparison of the induced currents produced by making and breaking the circuit. The first two columns contain the variation in ohms of the variable resistance of the bridge; the third and fourth columns give the strength of the induced currents on making and breaking, expressed in ohms.

Change in the resistance on breaking.	Change in the resistance on making.	Strength of Induced Current on breaking.	Strength of Induced Current on making.
650	600	·00325	·00300
700	680	·00350	·00340
720	720	·00360	·00360
720	750	·00360	·00375
700	700	·00350	·00350
850	850	·00425	·00425

Care should be taken to send the induced currents to be compared in the same direction by means of a pole-changer. It will be seen from the above Table that the equality of the currents on making and breaking can readily be proved by this method.—Silliman's *American Journal*, May 1873.

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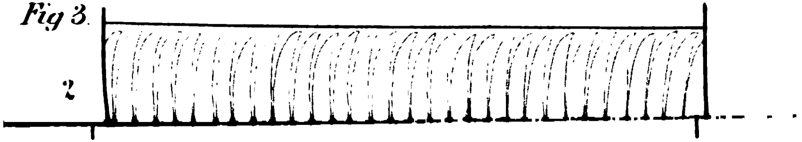

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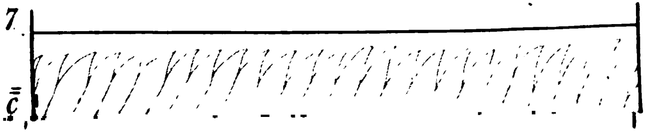

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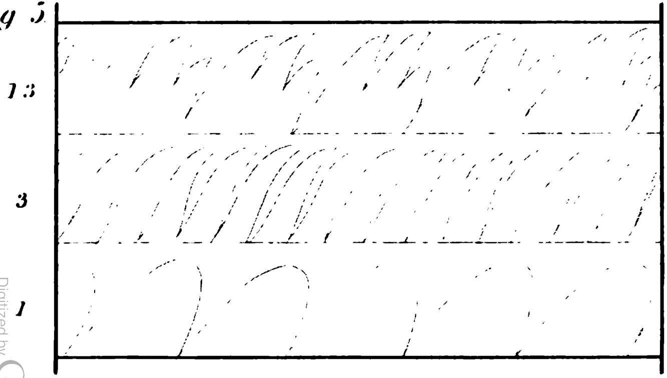

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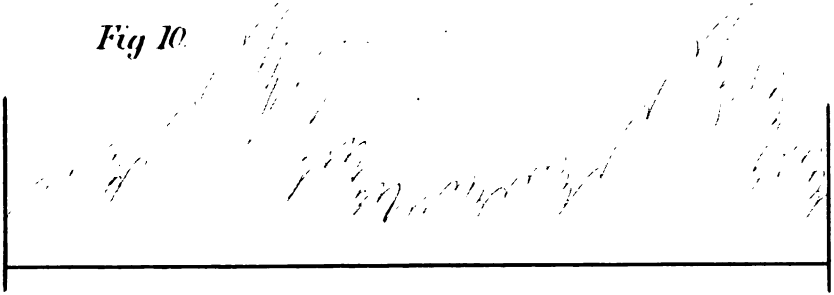

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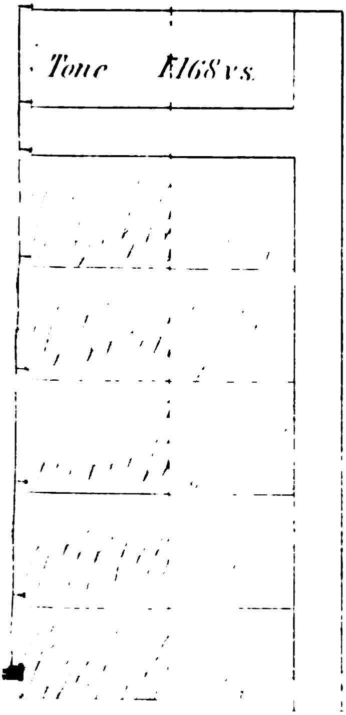

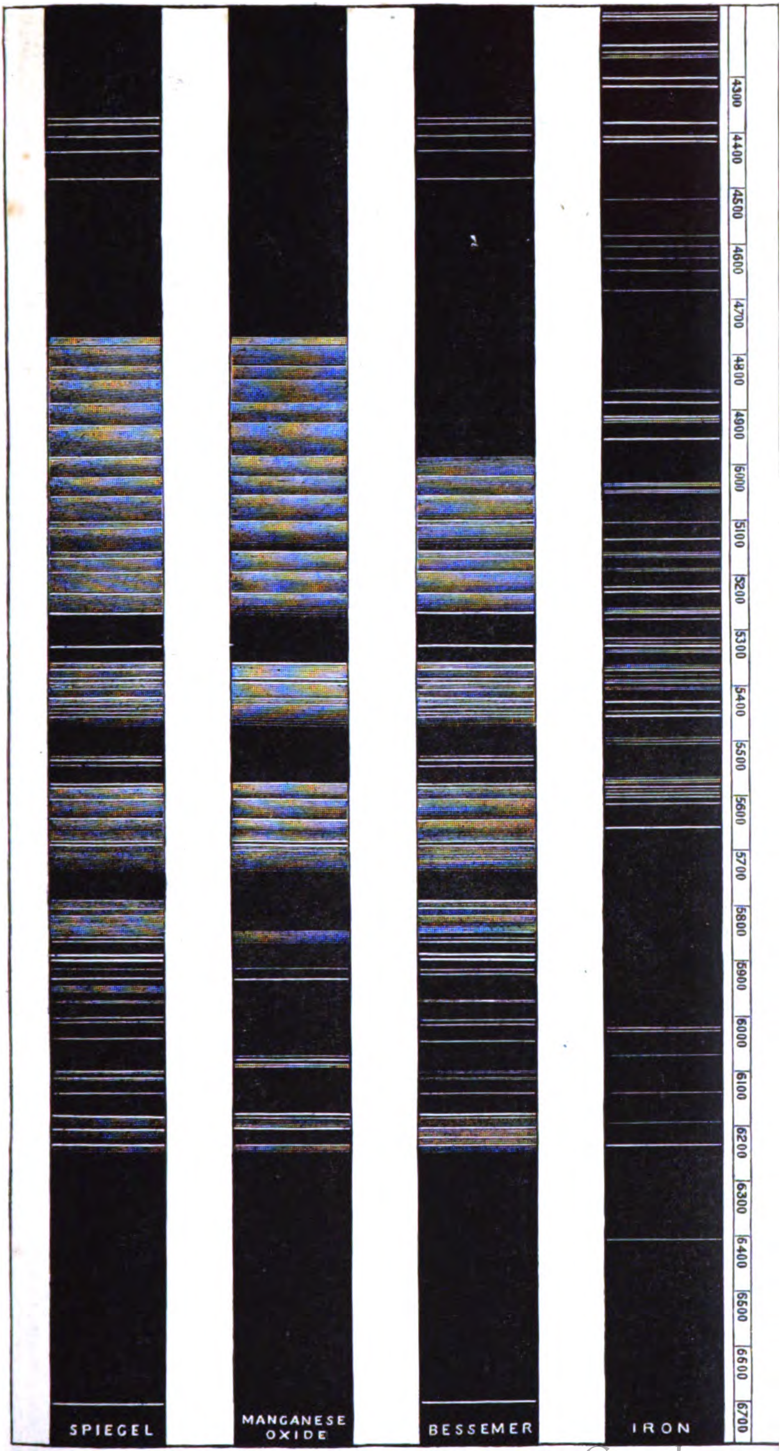

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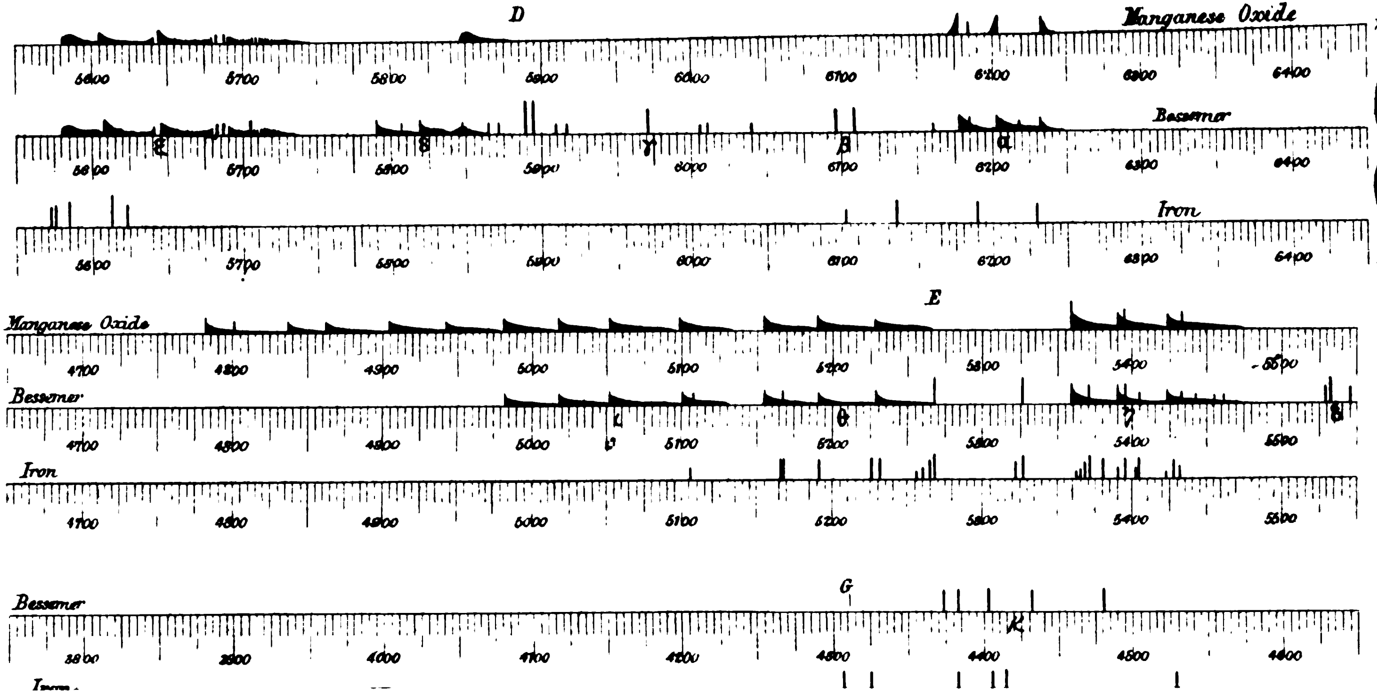
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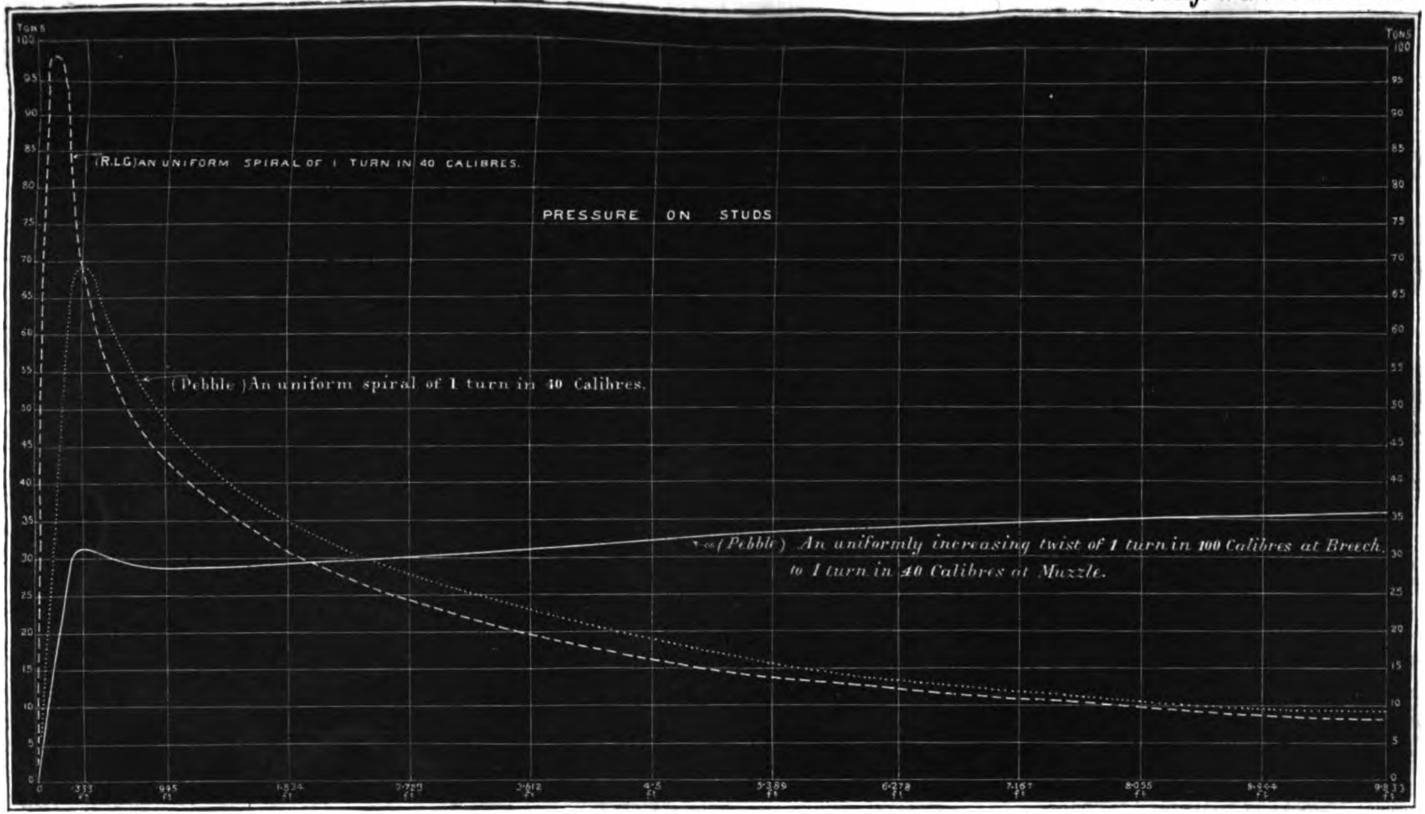

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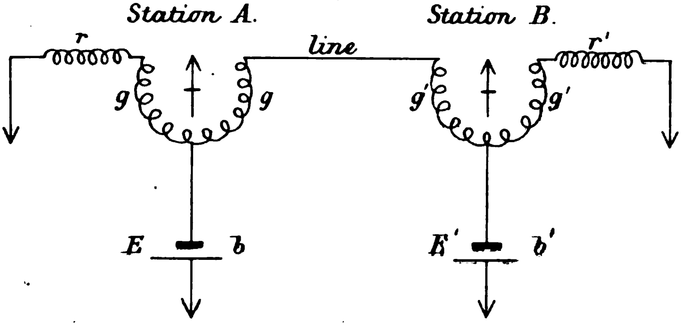

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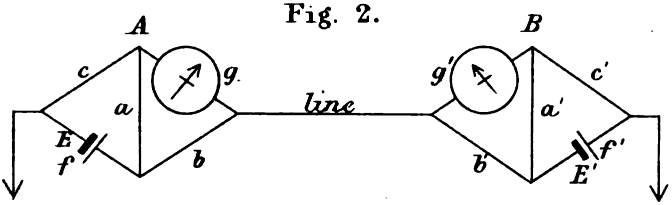

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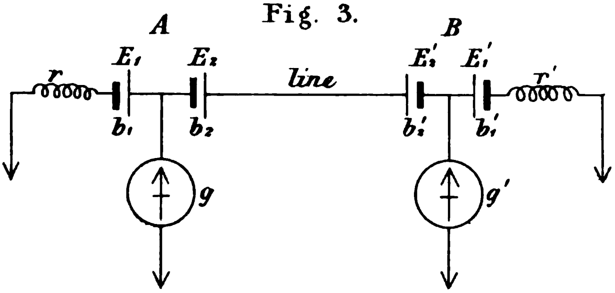

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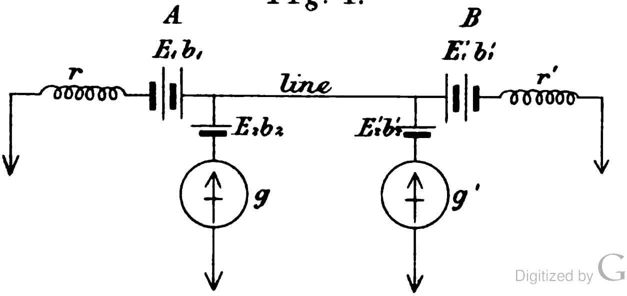

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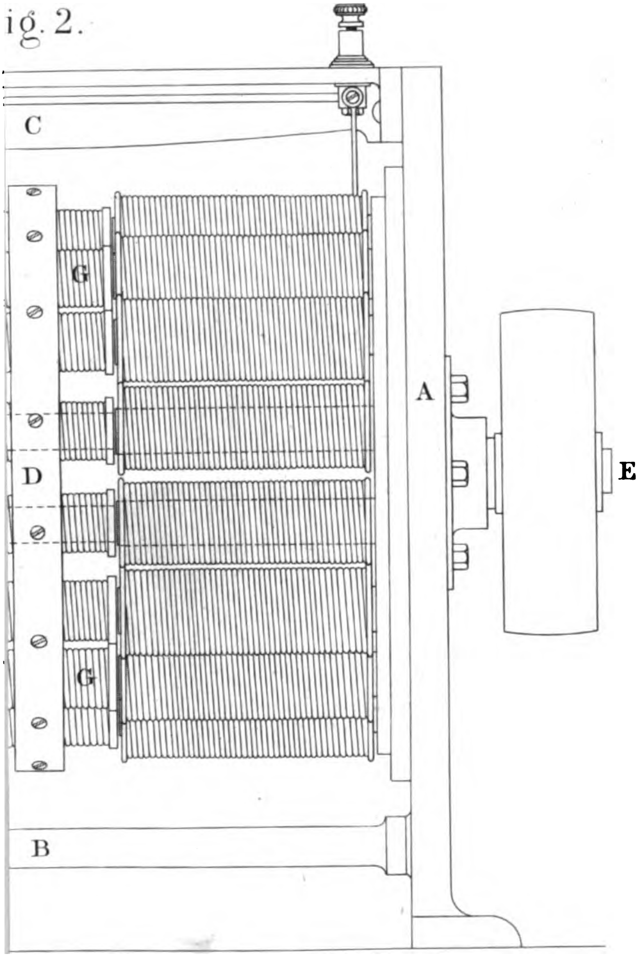

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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THE
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[FOURTH SERIES.]

JULY 1873.

I. *On the Relations between the characteristic Quantities occurring in Central Motions.* By R. CLAUSIUS[*].

IN a memoir published last year[†], I derived for the motion of a material point about a fixed centre of attraction, and for the motion of two material points around each other, some new relations between period of revolution, *vis viva*, ergal, and energy, which have the indispensable condition that the motions take place in closed paths. As, however, this condition is generally fulfilled only with particular kinds of attractive forces, it appeared to me important to extend the same method of treating the subject to the case in which the paths do not form closed curves; and I take leave to communicate the following results of this investigation.

1. Two new equations advanced by me for stationary motions serve as the basis of the inquiry. I will therefore first quote these equations; and as their connexion with equations previously known has recently been mooted in various quarters, it will perhaps be advisable also briefly to discuss this point, in order on the one hand to make evident the connexion, and on the other hand the difference.

Given a freely movable point with the mass m, which at the time t has the coordinates x, y, z, and is acted on by a force the components of which, taken in the directions of the coordinates, are X, Y, Z; then from the differential equation of motion,

---

[*] Translated from a separate copy, communicated by the Author, from the *Nachrichten der Königl. Gesellsch. der Wiss. zu Göttingen* of Dec. 25, 1872.

[†] *Nachr. d. K. Ges. d. Wiss.* May 24, 1871; *Math. Ann. von* Clebsch *und* Neumann, vol. iv. p. 231; Phil. Mag. November 1871.

$$m\frac{d^2x}{dt^2}=\mathrm{X},$$

can very readily be derived the following equation:—

$$\frac{m}{2}\left(\frac{dx}{dt}\right)^2=-\frac{1}{2}\mathrm{X}x+\frac{m}{4}\frac{d^2(x^2)}{dt^2}. \quad . \quad . \quad . \quad . \quad (1)$$

This equation I have applied to the case in which the motion is stationary—that is, where the coordinates and velocities do not continually change in the same direction, but only vary within certain limits*. In this case the mean value, referred to one period of the motion or to a very long time, of the differential coefficient of the second order $\frac{d^2(x^2)}{dt^2}=0$. Hence, if we indicate the mean values of the two other quantities occurring in the equation by putting a horizontal stroke above their symbols, the equation becomes

$$\frac{m}{2}\overline{\left(\frac{dx}{dt}\right)^2}=-\frac{1}{2}\overline{\mathrm{X}x}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equations (1) and (2) hold, of course, in the same form, also for both the other coordinates. If, further, instead of a single freely movable point, we have a whole system of such points, the same equations hold for each point of this system. We can therefore forthwith extend them so that they shall refer to all three coordinates and the whole system of points. If, namely, we employ the letter r for the radius vector of a point, and the letter v for its velocity, which letters can have different indices for the different points, the two equations will be transformed into:—

$$\Sigma\frac{m}{2}v^2=-\frac{1}{2}\Sigma(\mathrm{X}x+\mathrm{Y}y+\mathrm{Z}z)+\frac{1}{4}\frac{d^2(\Sigma mr^2)}{dt^2}, \quad . \quad (1a)$$

$$\Sigma\frac{m}{2}\overline{v^2}=-\frac{1}{2}\overline{\Sigma(\mathrm{X}x+\mathrm{Y}y+\mathrm{Z}z)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

Equation (2), in its amplified form (2*a*), is the *first* of my above-mentioned two equations. The quantity found on the right-hand side of (2*a*) I have named the *virial* of the system, and expressed the sense of the equation in the following proposition:—*The mean* vis viva *of the system is equal to its virial.* But at the same time I remarked that the proposition is true not merely for the whole system of points, and for the three coordinates together, but also for each point and each coordinate singly. Naming the quantity $\frac{m}{2}\left(\frac{dx}{dt}\right)^2$ the *vis viva* of the point relative to the x-direction, and in like manner the quantity $-\frac{1}{2}\overline{Xx}$ its *virial* with respect to the x-direction, and considering that the x- may be any direction we choose, we can express the proposition in the following terms:—*For every point the mean* vis viva *relative to any direction is equal to the* virial *relative to the same direction.*

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* *Sitzungsberichte der Niederrhein. Gesellschaft für Natur- und Heilkunde* 1870, p. 114; Pogg. *Ann.* vol. cxli. p. 124; Phil. Mag. S. 4. vol. xl. p. 126.

Now I have been reminded that an equation advanced by Jacobi and another by Lipschitz stand in connexion with the considerations I have instituted.

Jacobi's equation is found in Crelle's *Journal*, vol. xvii. p. 121, and in Jacobi's *Vorlesungen über Dynamik*, p. 22, where it is given as equation (2). It is there assumed that the forces acting in the system have a force-function, and, still more specially, that this force-function U is a homogeneous function of the kth dimension; and the equation relating to it is:—

$$\frac{d^2(\Sigma m_i r_i^2)}{dt^2}=(2k+4)U+4h,$$

in which h is the additive constant belonging to the force-function. It is evident that this equation can only be compared with equation (1 a), and not with (2a); and even from (1a) it is distinguished, on the one hand, by a very different form (as it does not include the *vis viva*), and on the other very essentially by holding only for a very limited class of forces, while (1a) is valid for all forces.

Lipschitz's equation is much more general (contained in his memoir "über einen algebraischen Typus der Bedingungen eines bewegten Massensystems"*). He likewise has under consideration a system of material points in motion, $P_1, P_2, \ldots P_n$, which, however, need not be free, but may be subject to conditions which are expressed by equations of a form there more closely defined. He assumes for each point a certain defined position, the coordinates of which are denoted for the point P_α by $a_\alpha, b_\alpha, c_\alpha$, while P_α itself at the time t has the coordinates $x_\alpha, y_\alpha, z_\alpha$. Further, besides the force-function U and the *vis viva* T of the entire system, he introduces a quantity G, which is determined by the following equation:—

$$\Sigma_\alpha m_\alpha[(x_\alpha-a_\alpha)^2+(y_\alpha-b_\alpha)^2+(z_\alpha-c_\alpha)^2]=2G;$$

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* *Journ. für reine und angew. Math.* vol. lxvi.

and with the aid of this quantity he forms the following equation:—

$$\frac{d^2G}{dt^2}-2T=\Sigma_\alpha\left[(x_\alpha-a_\alpha)\frac{dU}{dx_\alpha}+(y_\alpha-b_\alpha)\frac{dU}{dy_\alpha}+(z_\alpha-c_\alpha)\frac{dU}{dz_\alpha}\right].$$

This is very similar to equation (1); in some relations it is more general. First, Lipschitz has given it a more general signification, as he advances it not merely for free systems, but also for systems which are subject to certain conditions; and, secondly, for each point a special defined position is assumed, which is subject to further arrangement, while in (1 *a*) the corresponding position marked out for each point is the initial point of the coordinates. In other relations, on the contrary, it is more limited, as in it the forces are presupposed to have a force-function, which is not the case in (1 *a*).

Further than this equation (numbered 6 in his memoir) the agreement of Lipschitz's considerations with mine does not extend, as from this point he gives quite another turn to the investigation. While even equation (1), which is more general than (1 *a*), I have applied to stationary motions, and for these have derived equation (2), Lipschitz proceeds as follows. First he gives to his equation another form, in that he eliminates from it the *vis viva* by means of the relation which subsists between this and the force-function. Then he goes on to further specialize his equation by making definite suppositions concerning the nature of the force-function, particularly this—that it is an algebraic homogeneous function of the elements $x_\alpha-a_\alpha, y_\alpha-b_\alpha, z_\alpha-c_\alpha$. For the cases limited by these presuppositions, he then inquires what condition is necessary and sufficient in order that the motion may be stable. This investigation is, on account of the subject on which it treats, as well as the treatment itself, of high interest; but it is altogether different from my considerations, and accordingly the result which I have derived, and expressed by the theorem of the virial, does not appear in it.

The circumstance that no one before me put forth this theorem, although equation (2) is so easily obtained from the fundamental equations of motion, must, I think, be accounted for by this, that hitherto they had less inducement to turn their attention to stationary motions as such. But since the newer view of the nature of heat has become current we have, in the theory of heat, to do with a stationary motion of the minutest constituents of bodies; and as we knew (and still know) very little about the nature of this motion, it lay near to draw at least the conclusion which could already be drawn from the condition that the motion is stationary; and indeed the theorem thus gained is of peculiar importance for the theory of heat.

2. As for my *second* equation, I will here quote it only in the form which it has for a single movable point, since that form is sufficient for the following considerations, and the extension of it to a system of any number of points would require analyses of too great a length.

This equation stands in connexion with those which express the proposition of least action and Hamilton's amplification of the same; yet in an essential point it is different from them, as will be immediately evident when I put here the two latter equations, so that all three may be seen together.

Let a material point move freely, under the influence of a force, from a given initial point to a given final point. Then, instead of this motion, imagine another motion of the material point taking place between the same two limits in a path infinitesimally changed. If, now, the force acting on the point has a force-function or (as I have proposed to name it) *ergal*, and if it is further assumed that in both motions the ergal is represented by the same function of the space-coordinates, and that the energy (the sum of the ergal and *vis viva*) has the same value in each, then the following equation (known as the expression of the proposition of least action) holds:—

$$\delta\int v^2 dt = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If the time which the point requires for its motion be denoted by i, then this equation can also be written thus:—

$$\delta(\overline{v^2}i) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

Now this equation is valid even when the limits between which the changed motion takes place are not the same as with the original motion, provided they fulfil the condition that the quantity

$$\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z$$

has the same value at the end of the motion as at the beginning. This condition is fulfilled, for example, when both motions are in closed paths and in each motion an entire revolution is considered, so that the point where the motion ends coincides with the point where it begins.

If, on the contrary, this condition relative to the limits were not fulfilled, for (3a) the following equation would have to be substituted,

$$\delta(\overline{v^2}i) = \left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_1 - \left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_0, \quad (3b)$$

in which the indices $_0$ and $_1$ signify the initial and the final value of the bracketed terms. For the sake of simplicity we will, in the following, always consider that condition fulfilled, and accordingly put the difference which stands on the right-hand side of the above equation $=0$, so that the equation will retain the form (3*a*).

The amplification introduced by Hamilton consists in the same value not being attributed to the energy in both motions, a change of energy being considered admissible, while yet the ergal with the changed motion must still be the same function of the space-coordinates as with the original motion. The equation given by Hamilton for this case* is the following, in which E signifies the energy:—

$$m\delta(\overline{v^2}i) = i\delta E. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The earliest form of my equation† is

$$-\overline{(X\delta x + Y\delta y + Z\delta z)} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad . \quad . \quad (5)$$

In this equation the force acting on the point is not limited by any condition. Let us suppose, as before, that the force has an ergal, and designate it by U, fixing at the same time the positive and negative directions of the ergal so that the sum of the *vis viva* and the ergal during the motion is constant, then the equation becomes

$$\overline{\frac{dU}{dx}\delta x + \frac{dU}{dy}\delta y + \frac{dU}{dz}\delta z} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta\log i; \quad . \quad (5a)$$

here, however, it is not assumed, as in Hamilton's equation, that the function (designated by U) of the space-coordinates is invariable, but this function may, on one motion passing into the other, undergo a change independent of the alteration of the coordinates. Let us imagine, for example, that the function contains any quantities whatever independent of the coordinates and therefore constant during the motion, these constants, in order that Hamilton's equation may hold, must have the same values when the motion is changed as they had with the original motion. On the contrary, this is not necessary for the validity of my equation, but with the transition from one motion to the other the constants may change their values. This gives to the sum

$$\frac{dU}{dx}\delta x + \frac{dU}{dy}\delta y + \frac{dU}{dz}\delta z,$$

the mean value of which occurs in my equation (5*a*), a peculiar significance. We cannot replace it by the symbol δU, if by this we understand the complete variation of the ergal. That is to say, the complete variation must contain not merely the difference which is conditioned by the difference of the coordinates, but also that which arises from the change of the form of the function, consequently from the change (for example) of certain constants occurring in the function. If we designate these constants by c, c_1, &c., and their changed values by $c+\delta c$, $c_1+\delta c_1$, &c., and will employ the universal-variation symbol δU, my equation must be written thus:—

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* Thomson and Tait, 'Treatise of Natural Philosophy,' p. 235.

† *Sitzungsberichte der Niederrhein. Gesellsch. für Natur- und Heilkunde*, 1870, p. 174; Phil. Mag. S. 4. vol. xlii. p. 167.

$$\delta\overline{U} - \frac{d\overline{U}}{dc}\delta c - \frac{d\overline{U}}{dc_1}\delta c_1 - \&c. = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad (5\,b)$$

In order to gain a more convenient way of writing the equation, it will perhaps be advisable to introduce a special symbol for that part of the variation which relates only to the alteration of the coordinates, *e. g.* to employ a δ with an index, putting

$$\delta_1 U = \frac{dU}{dx}\delta x + \frac{dU}{dy}\delta y + \frac{dU}{dz}\delta z.$$

My equation then reads,

$$\delta_1\overline{U} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta\log i^*. \quad . \quad . \quad . \quad (5\,c)$$

3. Having made these preliminary remarks, we can now proceed to the discussion of central motions.

When we refer the motion of a point about a fixed centre of attraction to polar coordinates whose middle point coincides with that centre, two different processes present themselves for our consideration—the angular motion of the radius vector, and the motion of the point in the radius vector. The latter, so far as the whole motion generally is stationary, consists in alternate approach toward the centre and removal from it.

When the time expended in an approach and recession is equal to the period of revolution of the radius vector, after each revolution the moving point comes again to the same place, and from here commences a new revolution in the same path, and consequently we have a perpetual motion in a closed path. It is the same when during one revolution any whole number whatever of approaches and recessions is accomplished. If, further, during one approach and recession several revolutions take place, or if, generally, the period of an approach and recession be merely commensurable with the period of a revolution, the point will, certainly not after each revolution, but yet after a certain number of revolutions, come again to the same place and then repeat in like manner the motion just completed, so that a closed path will result from several revolutions. If, on the contrary (which is the most general case), the period of approach and recession be incommensurable with the period of revolution of the radius vector, each successive revolution will take place in another path, and we shall then have no longer to do with a closed path.

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* In my first memoir relative to this subject I have, it is true, replaced the sum on the left-hand side of my equation by the simple symbol $\delta\overline{U}$; but I have there expressly attached to the letter U a different signification from that here given to it—namely, by saying there, "let U denote the ergal *for the original motion*. The change in the form of the ergal which enters with the transition from one motion to the other I then expressed thus—by designating the ergal for the altered motion by $U+\mu V$, in which V denotes a second function of the coordinates, and μ an infinitely small constant.

Now, in order to apply my *first* equation to the central motion of a point, we can first give the virial a simpler form. If the force acting on the point is an attraction or repulsion proceeding from the origin of the coordinates, the intensity of which is represented by the function $F'(r)$, a positive value of the function denoting attraction, and a negative value repulsion, then is

$$-\frac{1}{2}(Xx+Yy+Zz)=\frac{1}{2}rF'(r),$$

and hence equation (2*a*) changes into

$$\frac{m}{2}\overline{v^2}=\frac{1}{2}\overline{rF'(r)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

In this equation the distinction whether the path is closed or not is of no consequence. For its applicability it is merely necessary that the motion be *stationary*, so that the quantities v^2 and $rF'(r)$ shall not continually change in the same direction, but only oscillate within certain limits and hence have definite mean values.

It is not so with my *second* equation; in this occurs the period i of the motion under consideration. If, then, the motion takes place in a closed path and therefore regularly repeats itself in exactly the same manner, we can confine our attention to a single run through this path, and take the time required for it as the quantity i. I have treated of this case in my previous memoirs. But when the motion takes place in a path which is not closed, and therefore does not present any periods repeating themselves in the same manner, the contemplation cannot thus be limited simply to one definite period; and a further investigation is required in order to ascertain whether and in what way my second equation can be applied to such a case.

4. For this purpose we will carry a little further the already above intimated division of the total motion into two—the an-

gular motion of the radius vector, and the to-and-fro motion of the point within the radius vector.

The angular motion of the radius vector we will name *motion of rotation*, and understand by *period of rotation* the time during which the radius vector runs through the entire angular space 2π. The motion of the point within the radius vector we will name *radial motion of vibration*, or, briefly, *motion of vibration*, and choose the name *period of vibration* for the time during which a vibration to and fro takes place. The period of a rotation may be denoted by i, and the period of a vibration by i_1.

The motion of vibration can be treated as quite independent of the motion of rotation, if we introduce the centrifugal as a special force. Let θ denote the angle which the radius vector forms, at the time t, with a fixed right line in the plane of rotation, so that $\frac{d\theta}{dt}$ will denote the angular velocity of the radius vector; then the centrifugal force to which the rotation gives rise will be represented by the product

$$mr\left(\frac{d\theta}{dt}\right)^2.$$

Now, as for the motion of a point about a fixed centre of attraction the proposition is valid that the radius vector describes equal spaces in equal times, we have the equation

$$r^2\frac{d\theta}{dt}=c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

in which c is a constant; and from this we immediately obtain, further,

$$mr\left(\frac{d\theta}{dt}\right)^2=mc^2\frac{1}{r^3}. \quad . \quad . \quad . \quad . \quad . \qquad (7a)$$

The centrifugal force represented by this expression we will now regard as a force of repulsion exerted from the centre, and to be added to the force represented by $F'(r)$, which is actually exerted from the centre. Then we obtain for the motion of vibration the following differential equation,

$$m\frac{d^2r}{dt^2}=-F'(r)+mc^2\frac{1}{r^3}, \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

with the aid of which we can treat the vibration as a motion subsisting by itself. To this motion thus regarded, both my equations are found at once applicable.

Equation (2) gives, if we substitute r for x, and $-F'(r)+mc^2\frac{1}{r^3}$

for X,

$$\frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^2}=\tfrac{1}{2}\,\overline{rF'(r)}-\frac{m}{2}c^2\,\overline{\frac{1}{r^2}}\,.\quad .\quad .\quad .\quad (9)$$

The second equation reads, in the form given under (5*c*) :—

$$\delta_1\overline{U}=\frac{m}{2}\,\delta\overline{v^2}+m\overline{v^2}\,\delta\log i.$$

In order to apply this equation, we have first to form the expression of the ergal U for the vibration-motion. In accordance with equation (8), we may put

$$U=\int\left(F'(r)-mc^2\frac{1}{r^3}\right)dr,$$

from which by integration, writing F(*r*) for the integral of F′(*r*), we get

$$U=F(r)+\frac{m}{2}c^2\frac{1}{r^2}\cdot$$

This is such a function as was mentioned above, containing a quantity *c* which is constant during each motion, but may change its value on passing from one motion to another. This quantity must, when we form the variation δ_1U, be regarded as constant; and thus we obtain

$$\delta_1U=\delta F(r)+\frac{m}{2}c^2\delta\frac{1}{r^2}\cdot$$

By introducing this expression into equation (5*c*), at the same time replacing v^2 by $\left(\frac{dr}{dt}\right)^2$ and using instead of *i* the symbol chosen for the period of a vibration, i_1, we transform that equation into

$$\delta\overline{F(r)}+\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}=\frac{m}{2}\,\delta\overline{\left(\frac{dr}{dt}\right)^2}+m\overline{\left(\frac{dr}{dt}\right)^2}\,\delta\log i_1.\quad .\quad (10)$$

From the two equations (9) and (10) we can eliminate the quantity $\left(\frac{dr}{dt}\right)^2$. We will, however, for the present, only employ in the variation of $\overline{\left(\frac{dr}{dt}\right)^2}$ the expression given in (9). At the same time it is to be noticed that *this* is a complete variation, in which the change of the quantity *c* must also be considered. We thus get

$$\frac{m}{2}\,\delta\overline{\left(\frac{dr}{dt}\right)^2}=\tfrac{1}{2}\,\delta\overline{rF'(r)}-\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}-m\overline{\frac{1}{r^2}}\,c\delta c.\quad .\quad (11)$$

This being put in (10) gives

$$\delta\overline{F(r)}+\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}=\tfrac{1}{2}\delta\overline{rF'(r)}-\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}-m\overline{\frac{1}{r^2}}\,c\delta c+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1,$$

or, differently arranged,

$$\delta\overline{F(r)}-\tfrac{1}{2}\delta\overline{rF'(r)}=-mc^2\delta\overline{\frac{1}{r^2}}-m\overline{\frac{1}{r^2}}\,c\delta c+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1,$$

for which we may write

$$\delta[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}]=-mc\delta\left(c\overline{\frac{1}{r^2}}\right)+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (12)$$

5. We will now turn to the motion of rotation.

In such a motion, in which the movable point need not have the same distance from the centre at the end as at the commencement of a rotation, the period of a rotation of the radius vector also need not be exactly the same for several successive rotations; yet at all events for a greater number of rotations we shall obtain a definite mean value of the period of a rotation. To this we will refer the symbol i. Now, pursuant to equation (7), we can write

$$d\theta=c\frac{1}{r^2}dt;$$

and when we integrate this equation for a whole number n of rotations, consequently between the limits $\theta=0$ and $\theta=n\,.\,2\pi$, there comes

$$n\,.\,2\pi=c\int_0^{ni}\frac{1}{r^2}dt=c\overline{\frac{1}{r^2}}ni,$$

and consequently

$$c\overline{\frac{1}{r^2}}=\frac{2\pi}{i}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Putting this value of $c\overline{\frac{1}{r^2}}$ in equation (12), we obtain

$$\delta[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}=-2\pi mc\delta\frac{1}{i}+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (14)$$

To this equation we can give a more symmetrical form; for the first term on the right-hand side can, in consideration of equation (13), be transformed thus:—

$$-2\pi mc\delta\frac{1}{i}=-mc^2\overline{\frac{1}{r^2}}\,i\delta\frac{1}{i}=mc^2\overline{\frac{1}{r^2}}\delta\log i.$$

Thereby the preceding equation is changed into

$$\delta[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}]=mc^2\overline{\frac{1}{r^2}}\delta\log i+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (15)$$

The half of the factor $mc^2\overline{\frac{1}{r^2}}$ is the mean *vis viva* of the motion of rotation; for the component of velocity originated by the rotation is $r\frac{d\theta}{dt}$, and the part of the *vis viva* corresponding to this component is $\frac{m}{2}r^2\left(\frac{d\theta}{dt}\right)^2$, for which, according to (7), we can even put $\frac{m}{2}c^2\frac{1}{r^2}$. Just so the half of the factor $m\overline{\left(\frac{dr}{dt}\right)^2}$ is the mean *vis viva* of the motion of vibration. Accordingly the equation is symmetric in relation to the motions of rotation and vibration*. Since, according to (9), the sum of the two factors $mc^2\overline{\frac{1}{r^2}}$ and $m\overline{\left(\frac{dr}{dt}\right)^2}$ is equal to $\overline{rF'(r)}$, we can even give to the preceding equation the following forms, more convenient for many applications:—

$$\delta\left[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}\right]=\overline{rF'(r)}\,\delta\log i-m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log\frac{i}{i_1}; \quad (16)$$

$$\delta\left[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}\right]=\overline{rF'(r)}\,\delta\log i_1+mc^2\overline{\frac{1}{r^2}}\delta\log\frac{i}{i_1}. \quad (17)$$

This equation, given in various forms under (14), (15), (16), and (17), is the expression of a new relation, universally valid for motions about a fixed centre of attraction.

When the central motion is of such a kind that a constant ratio subsists between the period of a rotation and the period of a vibration, the fraction $\frac{i}{i_1}$ is invariable, and therefore $\delta\log\frac{i}{i_1}=0$. Thereby the two preceding equations become accordant and are

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* Pursuant to equation (13), we can also form the following equation:—

$$-\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}=\frac{m}{2}\delta\left(c^2\overline{\frac{1}{r^2}}\right)+mc^2\overline{\frac{1}{r^2}}\delta\log i.$$

Now, as the expression $mc^2\frac{1}{r^3}$ represents a force of attraction equal to the centrifugal force, we may regard the quantity $-\frac{m}{2}c^2\frac{1}{r^2}$, arising from the integration of that expression, as the ergal relative to the motion of rotation. Remembering further that $\frac{m}{2}c^2\frac{1}{r^2}$ represents the *vis viva* of the motion of rotation, we see that the preceding equation has precisely the same meaning for the motion of rotation as (10) has for the motion of vibration. The sum of the two equations, together with the consideration of (9), gives the preceding equation (15).

changed into

$$\delta[\overline{F(r)} - \tfrac{1}{2}\overline{rF'(r)}] = \overline{rF'(r)}\,\delta \log i,$$

for which, as $\overline{rF'(r)} = m\overline{v^2}$, we can write

$$\delta\overline{F(r)} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\,\delta \log i.$$

This is the equation which holds for central motions in closed paths, and here appears as a special case of more general equations.

6. Having put the equations in such a manner that they are valid for any conceivable law of force, we will apply them to a special group of such laws, viz. to those according to which the force is *proportional to any power of the distance.* At the same time, however, we will exclude the minus first power, because in the integration it leads to logarithms, and consequently demands separate discussions, which would detract from the compendiousness of the analysis.

Accordingly, k and n denoting two constants, the latter of which is different from -1, we will put

$$F'(r) = kr^n, \quad \ldots\ldots\ldots \quad (18)$$

whence results

$$F(r) = \frac{k}{n+1}\,r^{n+1}. \quad \ldots\ldots \quad (19)$$

These formulæ we have to substitute for $F(r)$ and $F'(r)$ in the above equations. Equation (16) may be selected, which by the substitution is changed into

$$\frac{1-n}{2(n+1)}\,k\delta\overline{r^{n+1}} = k\overline{r^{n+1}}\,\delta \log i - m\,\overline{\left(\frac{dr}{dt}\right)^2}\,\delta \log \frac{i}{i_1}. \quad . \quad (20)$$

For the sake of a more convenient expression, the quantity ρ shall now be introduced, with the signification

$$\rho^{n+1} = \overline{r^{n+1}}. \quad \ldots\ldots\ldots \quad (21)$$

This quantity can be immediately determined when the *energy* (sum of ergal and *vis viva*) is known, consequently when we know, for any position of the movable point, its velocity. That is to say, since the mean *vis viva* is equal to the virial, we have generally, if E denotes the energy,

$$E = \overline{F(r)} + \tfrac{1}{2}\overline{rF'(r)},$$

and for our special law of force:—

$$E = \frac{k}{n+1}\,\overline{r^{n+1}} + \frac{k}{2}\,\overline{r^{n+1}} = k\,\frac{n+3}{2(n+1)}\,\overline{r^{n+1}},$$

and consequently, employing the quantity ρ,

$$E=k\frac{n+3}{2(n+1)}\rho^{n+1},$$

from which it follows that

$$\rho=\left[\frac{2(n+1)}{k(n+3)}E\right]^{\frac{1}{n+1}}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (22)$$

After substituting, with the aid of this quantity ρ, for the mean value of a power, represented by $\overline{r^{n+1}}$, a simple power ρ^{n+1}, we can forthwith carry out the variation of it, and write

$$\delta\overline{r^{n+1}}=(n+1)\rho^{n}\delta\rho.$$

Accordingly, by the introduction of ρ, equation (20) changes into

$$\frac{1-n}{2}k\rho^{n}\delta\rho=k\rho^{n+1}\delta\log i-m\overline{\left(\frac{dr}{dt}\right)^{2}}\delta\log\frac{i}{i_1}. \quad . \quad . \qquad (23)$$

Into this equation we will introduce a second simplifying quantity, p; for as the mean *vis viva* of the rotatory motion and the mean *vis viva* of the radial vibratory motion together make up the total mean *vis viva*, we can represent the two former as fractions of the latter, which fractions shall be denoted by p and $1-p$. Thus

$$\left.\begin{aligned}&\frac{m}{2}c^{2}\overline{\frac{1}{r^{2}}}=p\,\frac{k}{2}\rho^{n+1},\\&\frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^{2}}=(1-p)\,\frac{k}{2}\rho^{n+1}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \qquad (24)$$

The value of p may vary between 0 and 1; and on it depends which out of all the forms possible with a given value of n the path takes. When $p=0$, the point moves in a right line to and from the centre; and when $p=1$, it moves in a circle round the centre. Between these two limits lie all the other possible forms.

By introducing p into equation (23) we obtain first

$$\frac{1-n}{2}k\rho^{n}\delta\rho=k\rho^{n+1}\delta\log i-(1-p)k\rho^{n+1}\delta\log\frac{i}{i_1};$$

and this equation, divided by $k\rho^{n+1}$, gives

$$\frac{1-n}{2}\delta\log\rho=\delta\log i-(1-p)\delta\log\frac{i}{i_1}. \quad . \quad . \quad . \qquad (25)$$

As two of the terms of this equation are complete variations, the third term,

$$(1-p)\delta\log\frac{i}{i_1},$$

must likewise be such, whence it follows that p is a function of $\frac{i}{i_1}$ alone, or vice versâ $\frac{i}{i_1}$ is a function of p alone. Regarding $\frac{i}{i_1}$ as a function of p, we can from this function derive others; and we will introduce one, denoted by J, which shall be determined by the following equation:—

$$\log J = \int (1-p) \frac{d \log \frac{i}{i_1}}{dp} dp. \quad . \quad . \quad . \quad . \quad (26)$$

Then is

$$(1-p)\delta \log \frac{i}{i_1} = \delta \log J;$$

and therefore equation (25) changes into

$$\frac{1-n}{2} \delta \log \rho = \delta \log i - \delta \log J. \quad . \quad . \quad . \quad (27)$$

By transposition and integration of this equation we obtain

$$\log i = \frac{1-n}{2} \log \rho + \log J + \text{const.}$$

It is immaterial which value we attribute to the integration-constant, since, in accordance with (26), we can suppose any additive constant we please to be likewise contained in log J. The value most suitable for what follows is $\log 2\pi\sqrt{\frac{m}{k}}$. If we put this quantity in the place of the constant, and then combine the three logarithms of the right-hand side, we get

$$\log i = \log\left(2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}} J\right);$$

and from this we obtain, for the rotation-period i, the following simple expression:—

$$i = 2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}} J. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

We can deduce a corresponding expression for the oscillation-period i_1. Equation (25), namely, can also be written in the following form:—

$$\frac{1-n}{2} \delta \log \rho = \delta \log i_1 + p\delta \log \frac{i}{i_1}.$$

Introducing into this the function J_1, which is determined by the following equation,

$$\log J_1 = -\int p \frac{d\log\frac{i}{i_1}}{dp}\, dp, \quad . \quad . \quad . \quad . \quad (29)$$

the result is similar to the preceding:—

$$i_1 = 2\pi\sqrt{\frac{m}{k}}\,\rho^{\frac{1-n}{1}} J_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

Between the functions J and J_1, occurring in these two expressions, there subsists, according to equations (26) and (29), the following relation:—

$$p\frac{d\log J}{dp} = (p-1)\frac{d\log J_1}{dp}. \quad . \quad . \quad . \quad . \quad (31)$$

As, with the aid of this equation, one of the two functions can be deduced from the other, we can say that, in the two expressions of i and i_1, only one undetermined function of p occurs.

7. In equations (28) and (30), the periods i and i_1 are expressed by the quantities ρ and p. According to equation (22), ρ is a simple function of the energy E, which remains invariable during the whole motion, and hence can be taken as known. It is otherwise with the quantity p. It is true this has a simple signification (the mean *vis viva* of the motion of rotation as a fraction of the total mean *vis viva*); but its value cannot be stated so simply, because for the calculation of the mean value of a variable quantity the whole course of the motion must be taken into consideration. Hence it is advisable to introduce instead of p another quantity the value of which is obtained immediately from the data usually employed for the determination of the motion.

These are the energy E and the already discussed quantity c, the half of which represents the area described by the radius vector in the unit of time, and which, as well as the energy, remains constant during the whole motion. We will now bring in a quantity q determined by the following equation,

$$q = m^{\frac{1}{2}} k^{\frac{1}{n+1}} c \left[\frac{2(n+1)}{n+3} E\right]^{\frac{n+3}{2(n+1)}}, \quad . \quad . \quad . \quad (32)$$

and which, consequently, can be calculated in a very simple manner from E and c.

In order to find the connexion of this new quantity q with p, we can first, with the aid of equation (22), transform the expression of q into the following:—

$$q = \sqrt{\frac{m}{k}}\,\frac{c}{\rho^{\frac{n+3}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

Further, equations (13) and (24) hold good—

$$c\overline{\frac{1}{r^2}}=\frac{2\pi}{i},$$

$$\frac{m}{2}c^2\overline{\frac{1}{r^2}}=p\frac{k}{2}\rho^{n+1},$$

from which, by elimination of $\frac{1}{r^2}$ results

$$i=2\pi\frac{m}{k}\frac{c}{p\rho^{n+1}}. \qquad (34)$$

If herein for i we put its value from (28), we obtain

$$J=\sqrt{\frac{m}{k}}\frac{c}{p\rho^{\frac{n+3}{2}}}; \qquad (35)$$

and the combination of this with equation (33) gives

$$q=pJ. \qquad (36)$$

Consequently q stands in a very simple relation to p.

Its behaviour is also very similar to that of p. It can likewise vary only between the limits 0 and 1, and takes these boundary values simultaneously with p. When $c=0$, then, according to (32), is q also $=0$; and consequently the *vis viva* of the rotation-motion also, and therefore the quantity p, has the value zero. If, now, c is increasing, p and q increase at the same time. When p has reached the value 1 the path has become circular. For this case we have, in the differential equation

$$m\frac{d^2r}{dt^2}=-kr^n+mc^2\frac{1}{r^3}$$

for the oscillatory motion, to put the differential coefficient $\frac{d^2r}{dt^2}$ equal to zero; and the equation thereby arising can be brought into the following form:—

$$\frac{m}{k}\frac{c^2}{r^{n+3}}=1.$$

Further, where r is constant, we may regard r and ρ as signifying the same thing, and hence the preceding equation can also be written

$$\frac{m}{k}\frac{c^2}{\rho^{n+3}}=1;$$

and this gives, in accordance with (33), for q the value 1.

Since, according to (36), q is represented by the product pJ, in which J is a function of p only, q itself is likewise a function of p alone, and accordingly p can reciprocally be regarded as a function of q only. Thence it follows, further, that J and J_1, which we have hitherto written as functions of p, can just as well be regarded as functions of q; and for this we have only to transform the relation between J and J_1 expressed in (31) so that it shall contain q in the place of p.

Equation (31) can be written in the following form:—

$$\frac{d\log J_1}{dp}=p\frac{d}{dp}\left(\log\frac{J_1}{J}\right). \quad . \quad . \quad . \quad . \qquad (37)$$

Differentiation of the logarithms gives

$$\frac{1}{J_1}\frac{dJ_1}{dp}=p\frac{J}{J_1}\frac{d}{dp}\left(\frac{J_1}{J}\right).$$

Here J_1 can be taken away from both denominators; and, according to (36), q can be substituted for pJ. Further, we can transform the two differential coefficients according to p into such according to q by employing the general equation

$$\frac{dZ}{dp}=\frac{dZ}{dq}\frac{dq}{dp},$$

wherein the differential coefficient $\frac{dq}{dp}$, which occurs on both sides, can be omitted. We thus obtain

$$\frac{dJ_1}{dq}=q\frac{d}{dq}\left(\frac{J_1}{J}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (38)$$

This is the relation sought between J and J_1. It is seen that the new equation, in relation to J_1, $\frac{J_1}{J}$, and q, has the same form as (37) in relation to $\log J_1$, $\log\frac{J_1}{J}$, and p.

8. In the preceding, for central motions in which the attractive force is proportional to any power of the distance, a series of formulæ is given which represent the times corresponding to the motion-periods, and various mean values, as functions of two easily determinable quantities. As these formulæ are scattered among the equations applied to their derivation, it will be advisable, for the sake of a more convenient review of them, to briefly recapitulate them in juxtaposition.

E denoting the energy of the moving point, and c twice the value of the area described by the radius vector during the unit of time, we first form the following two quantities:—

$$\rho=\left[\frac{2(n+1)}{k(n+3)}E\right]^{\frac{1}{n+1}},$$

$$q=m^{\frac{1}{2}}k^{\frac{1}{n+1}}c\left[\frac{2(n+1)}{n+3}E\right]^{-\frac{n+3}{2(n+1)}}=\sqrt{\frac{m}{k}}\frac{c}{\rho^{\frac{n+3}{2}}}.$$

We next introduce two functions of q, denoted by J and J_1, which are connected by the following equation:—

$$\frac{dJ_1}{dq}=q\frac{d}{dq}\left(\frac{J_1}{J}\right).$$

Then for the undermentioned quantities the equations written after them are valid:—

(1) The mean ergal:

$$\frac{k}{n+1}\overline{r^{n+1}}=\frac{k}{n+1}\rho^{n+1}.$$

(2) The mean *vis viva*:

$$\frac{m}{2}\overline{v^2}=\frac{k}{2}\rho^{n+1}.$$

(3) The mean *vis viva* of the rotatory motion (according to equation (24), replacing p therein by $\frac{q}{J}$):

$$\frac{m}{2}c^2\overline{\frac{1}{r^2}}=\frac{k}{2}\rho^{n+1}\frac{q}{J}.$$

(4) The mean *vis viva* of the radial oscillatory motion:

$$\frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^2}=\frac{k}{2}\rho^{\;+1}\left(1-\frac{q}{J}\right).$$

(5) The period of a rotation:

$$i=2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}J.$$

(6) The period of a vibration:

$$i_1=2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}J_1.$$

Besides the above quantities, others can easily be expressed after the above developments—for example, the mean ergal of the rotatory motion and of the vibratory motion; meanwhile the preceding expressions may suffice.

9. In all these expressions, only one undetermined function of q occurs, since the two denoted by J and J_1 can, through the relation subsisting between them, be reduced to one. To determine this function also was not originally included in the plan of my investigation, as I only wished to deduce those consequences which result immediately from my new mechanical equations; but after I had produced the above formulæ it seemed to me advisable, for the sake of completeness, to undertake at

least an approximative determination of that function. I have therefore developed the function J_1 in a series, and calculated some of the terms of that series.

From the above-mentioned differential equation

$$m\frac{d^2r}{dt^2}=-kr^n+mc^2\frac{1}{r^3},$$

we obtain by the first integration:—

$$\frac{m}{2}\left(\frac{dr}{dt}\right)^2=-\frac{k}{n+1}r^{n+1}-\frac{m}{2}c^2\frac{1}{r^2}+\mathrm{E},$$

wherein E, as before, denotes the energy of the motion. This gives, for the determination of the time, the differential equation

$$dt=\frac{dr}{\sqrt{\frac{2}{m}\mathrm{E}-c^2\frac{1}{r^2}-\frac{k}{m}\frac{2}{n+1}r^{n+1}}};$$

or, multiplying both numerator and denominator by r,

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{-c^2+\frac{2}{m}\mathrm{E}r^2-\frac{k}{m}\frac{2}{n+1}r^{n+3}}}\quad .\ \ .\ \ .\ \ (39)$$

Into this we will introduce the constants ρ and q instead of E and c, putting, according to equations (22) and (33),

$$\mathrm{E}=k\frac{n+3}{2(n+1)}\rho^{n+1}\text{ and }c^2=\frac{k}{m}q^2\rho^{n+3}.$$

We then get

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{-\frac{k}{m}q^2\rho^{n+3}+\frac{k}{m}\frac{n+3}{n+1}\rho^{n+1}r^2-\frac{k}{m}\frac{2}{n+1}r^{n+3}}}.$$

If we take away the factor $\frac{k}{m}\rho^{n+3}$ from the root-sign, we obtain

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{\frac{k}{m}}\rho^{\frac{n+3}{2}}\sqrt{-q^2+\frac{n+3}{n+1}\left(\frac{r}{\rho}\right)^2-\frac{2}{n+1}\left(\frac{r}{\rho}\right)^{n+3}}};$$

or, otherwise written,

$$dt=\frac{1}{2}\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\frac{d\left(\frac{r^2}{\rho^2}\right)}{\sqrt{-q^2+\frac{n+3}{n+1}\left(\frac{r}{\rho}\right)^2-\frac{2}{n+1}\left(\frac{r}{\rho}\right)^{n+3}}}.\ \ .\ (40)$$

Herein, for convenience we will put, instead of the fraction $\frac{r}{\rho}$,

$$dt=\tfrac{1}{2}\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}. \quad . \quad . \quad (41)$$

This equation must be integrated in order to obtain the vibration-period. Therein, as limits, two values of x are to be taken for which the expression under the root-sign, and accordingly the radial velocity $\frac{dr}{dt}$, becomes $=0$. These values may be denoted by x_0 and x_1. The integral from one limit to the other gives the time needed by the movable point in order to arrive at the highest from the lowest value; but as we understand by the vibration-period the time employed by the point in attaining from the lowest to the highest value and then again to the lowest, the double of the above-mentioned must be taken. We thus obtain

$$i_1=\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}. \quad (42)$$

Comparing this expression for i_1 with that given in (30), we obtain for the function J_1 the equation

$$J_1=\frac{1}{2\pi}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}. \quad . \quad . \quad (43)$$

10. In order to effect this integration, we will first write the equation thus:—

$$J_1=\frac{1}{2\pi}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{1-q^2-\left(1-\frac{n+3}{n+1}x^2+\frac{2}{n+1}x^{n+3}\right)}}. \quad (44)$$

Into this we will introduce a new variable, z, determined by the following equation:—

$$1-\frac{n+3}{n+1}x^2+\frac{2}{n+1}x^{n+3}=z^2. \quad . \quad . \quad . \quad (45)$$

If we then imagine the quantity x^2 developed in a series according to ascending powers of z, putting

$$x^2=a+a_1z+a_2z^2+a_3z^3+\&c., \quad . \quad . \quad . \quad . \quad (46$$

and so determine the coefficients of this series that they satisfy the preceding equation, we obtain the following values, in which, further, for abbreviation, μ with the signification

$$\mu=\frac{(n+2)(n-1)}{n+3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (47)$$

is introduced:—

$$\left.\begin{aligned}
a&=1,\\
a_1&=\pm\frac{2}{\sqrt{n+3}},\\
a_2&=-\frac{n-1}{n+3}\cdot\frac{1}{3},\\
a_3&=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{1}{2\,.\,3^2},\\
a_4&=-\mu\frac{n+5}{n+3}\cdot\frac{1}{3^3.\,5},\\
a_5&=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{2^3.\,3+\mu}{2^4.\,3^3.\,5},\\
a_6&=-\mu\frac{n+5}{n+3}\cdot\frac{2\,.\,3^2-\mu}{3^5.\,5\,.\,7},\\
a^7&=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{2^4.\,3^3.\,5^2-2^6.\,3^2\mu-139\mu^2}{2^6.\,3^5.\,5^2.\,7}.
\end{aligned}\right\} \quad . \qquad (48)$$

By the employment of the new variable, z, equation (44) is changed into the following, in which the limits of the integration are definitely given:—

$$J_1=\frac{1}{2\pi}\int_{z=-\sqrt{1-q^2}}^{z=\sqrt{1-q^2}}\frac{d(a+a_1z+a_2z^2+a_3z^3+\&c.)}{\sqrt{1-q^2-z^2}} \quad . \quad . \qquad (49)$$

The integral herein signified falls, according to the terms of the series, into an infinite number of integrals, the values of which can easily be given. Every term that contains an even power of z gives as an integral the value zero. For the terms with odd powers the following general equation (in which ν is to be an uneven integer) holds good:—

$$\int_{z=-\sqrt{1-q^2}}^{z=\sqrt{1-q^2}}\frac{d(z^\nu)}{\sqrt{1-q^2-z^2}}=\frac{1\,.\,3\,.\,5\,.\,7\ldots\nu}{1\,.\,2\,.\,4\,.\,6\ldots\nu-1}(1-q^2)^{\frac{\nu-1}{2}}\pi.$$

If we apply this formula to the odd terms of the preceding equation, we obtain first

$$J_1=\frac{1}{2}\left[a_1+\frac{3}{2}a_3(1-q^2)+\frac{3\,.\,5}{2\,.\,4}a_5(1-q^2)^2+\&c.\right]. \quad . \qquad (50)$$

In this equation, for the coefficients a , a_3, &c. we have to put their values derived from (48), taking into account only the upper of the two signs which stand before the roots, as the lower sign would give a negative period. Thereby we obtain

$$\mathrm{J}_1=\frac{1}{\sqrt{n+3}}\Big[1+\frac{\mu}{2^3.3}(1-q^2)+\mu\frac{2^3.3+\mu}{2^8.3^2}(1-q^2)^2$$
$$+\mu\frac{2^4.3^3.5^2-2^6.3^2\mu-139\mu^2}{2^{11}.3^5.5}(1-q^2)^3+\&\text{c.}\Big] \quad . \; . \; (51)$$

Hereby the quantity J_1 as a function of q is so far determined that, for values of q not deviating too much from unity, it can be calculated with tolerable approximateness.

From this series, with the aid of (38), the corresponding series for J can be derived.

11. This derivation and other calculations become somewhat easier when the series is developed according to ascending powers of $1-q$. We will represent the latter difference by a special letter, putting

$$s=1-q. \quad . \; . \; . \; . \; . \; . \; . \; . \quad (52)$$

Then

$$1-q^2=-(1-s)^2=2s-s^2;$$

and the insertion of this value changes the above series into

$$\mathrm{J}_1=\frac{1}{\sqrt{n+3}}\Big[1+\frac{\mu}{2^2.3}s+\frac{\mu^2}{2^6.3^2}s^2$$
$$-\mu\frac{2^4.3^3.5+2^2.3^2.31\mu+139\mu^2}{2^8.3^5.5}s^3+\&\text{c.}\Big]. \quad (53)$$

In order from this to calculate J, we can write equation (38) in the following form:—

$$\frac{d}{ds}\Big(\frac{\mathrm{J}_1}{\mathrm{J}}\Big)=\frac{1}{1-s}\frac{d\mathrm{J}_1}{ds}. \quad . \; . \; . \; . \; . \; . \quad (54)$$

Putting herein for J_1 the preceding expression, we obtain first the differential coefficient of $\frac{\mathrm{J}_1}{\mathrm{J}}$, and then, by integration, $\frac{\mathrm{J}_1}{\mathrm{J}}$ itself. The added integration-constants can be readily determined, because for $s=0$, consequently for the circular motion, the rotation-period i, and accordingly the value of the function J, can be directly determined. It follows, namely, that for this case J is to be put $=1$, whence $\frac{\mathrm{J}_1}{\mathrm{J}}=\frac{1}{\sqrt{n+3}}$. Taking into account this value gives for $\frac{J_1}{J}$ the following series:—

$$\frac{J_1}{J}=\frac{1}{\sqrt{n+3}}\Big[1+\frac{\mu}{2^2.3}s+\mu\frac{2^3.3+\mu}{2^6.3^2}s^2$$
$$+\mu\frac{2^4.3^4.5-2^2.3^3.7\mu-139\mu^2}{2^8.3^5.5}s^3+\&c.\Big]. \quad (55)$$

As the fraction $\frac{J_1}{J}$ is equal to the fraction $\frac{i_1}{i}$, the preceding expression represents the ratio between the vibration-period and the rotation-period.

We might now, in order to determine J, simply divide equation (53) by equation (55); but then we should only obtain J developed as far as the third power, while the term containing the fourth power is obtainable; for if we write equation (54) in the form

$$\frac{d}{ds}\left(\frac{J_1}{J}-J_1\right)=s\frac{d}{ds}\left(\frac{J_1}{J}\right), \quad . \quad . \quad . \quad . \quad . \quad (56)$$

we can from this, on account of the factor s on the right-hand side, determine to the fourth power of s the difference $\frac{J_1}{J}-J_1$ (which does not contain a constant term, but begins at once with the second power of s), although $\frac{J_1}{J}$ is known only to the third power. If we then form the identical equation

$$J=1-\frac{\frac{J_1}{J}-J_1}{\frac{J_1}{J}},$$

and employ, on the right-hand side of it, instead of the difference which forms the numerator of the second term, the expression developed to the fourth power, we get

$$J=1-\frac{\mu}{2^3.3}s^2+\mu\frac{-2^2.3+\mu}{2^4.3^3}s^3$$
$$+\mu\frac{-2^4.3^4.5+2^2.3.7.29\mu+89\mu^2}{2^{10}.3^4.5}s^4+\&c. \quad (57)$$

12. We have therefore obtained for the functions J and J_1 expressions from which their values can be approximately determined, and indeed with an accuracy proportional to the smallness of s, or inversely as the deviation of the path from the circular form. They afford, in many a relation, a ready insight into the behaviour of these functions.

The factor preceding the series in the expression for J_1, viz. $\frac{1}{\sqrt{n+3}}$, which for $n=-3$ is infinitely large, and for values of n below -3 becomes imaginary, shows very clearly that, for attractive forces proportional to a negative power of the distance, the minus third power constitutes the limit to which stationary motions are in general possible.

Further, it is characteristic of both expressions that all the terms which contain s have the common factor μ. From this it follows that, when $\mu=0$, both expressions become independent of the quantity s, equations (53) and (57) changing into

$$J_1=\frac{1}{\sqrt{n+3}} \text{ and } J=1.$$

Now, since according to (47)

$$\mu=\frac{(n+2)(n-1)}{n+3},$$

this simple behaviour occurs in the two cases in which n has the values -2 and 1.

These two values of n are the only ones with which the motions universally (that is, for all values of s) take place in closed paths. That is to say, this can only be the case when the ratio between the vibration-period and the rotation-period is invariable, and hence $\frac{i_1}{i}$, which is identical with $\frac{J_1}{J}$, must be independent of s. Thence it follows further that in the series under (55) the coefficients of all the powers of s, from the first onward, must be $=0$, so that the constant part alone remains—which only happens when $\mu=0$ and consequently n is either $=-2$ or $=1$.

For $\mu=0$, the fraction $\frac{i_1}{i}$ is represented by the formula $\frac{1}{\sqrt{n+3}}$, which, according as $n=-2$ or $n=1$, takes the value 1 or $\frac{1}{2}$. By this is expressed that, in the former case, during a rotation one radial vibration takes place, so that the radius vector has one maximum and one minimum—while in the latter case there are two radial vibrations, so that the radius vector has two maxima and two minima.

All the preceding considerations relate to the motions of a material point about a fixed centre. Considerations altogether similar might be instituted in relation to the motions of two material points about each other, and would lead to corresponding results. In my previous memoirs I have carried out this extension for the case in which the motions take place in closed paths; and with motions in paths not closed the extension can be made in substantially the same manner.

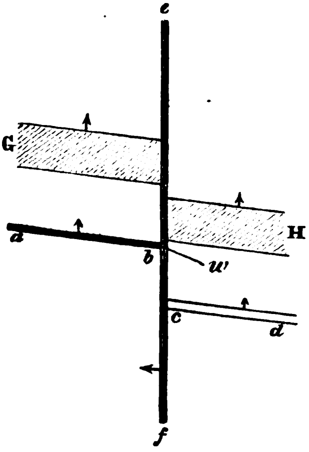
II. *On the Composition and Origin of the Waters of a Salt Spring in Huel Seton Mine, with a Chemical and Microscopical Examination of certain Rocks in its vicinity.* *By* J. ARTHUR PHILLIPS. *M.Inst.C.E., F.G.S., F.C.S., &c.*[*]

[With a Plate.]

HUEL SETON Copper-mine is situated about one mile north-east of the town of Camborne, Cornwall, and is distant from the sea, on the north coast, a little more than three miles. Its position will be readily understood by the aid of the accompanying skeleton map (Plate I.) of the district, traced from that of the Geological Survey.

The workings of Huel Seton are entirely in "killas," or clay-slate; and the saline waters issue at the rate of 50 gallons per minute, and at a temperature of 92° F., from the eastern fore-breast of the 160-fathom level[†]. This has intersected a fault or cross-course, which can be traced in a northerly direction to the sea. The temperature of the level from the end of which the water issues, like that of the water itself, is 92° F.

Fig. 1.



The lode *a b, c d,* fig. 1, which is not well defined, and is frequently a mere narrow fissure in the clay-slate, has been driven on to its point of intersection *b* with the cross-course *e f,* and is believed to have been thrown in a southerly direction from *b* to *c*. A dyke of porphyry, G H, 40 feet in width, courses parallel with the lode at a distance of a few fathoms to the north; but its intersection with the cross-course not having been seen, it is not known whether any throw or dislocation actually takes place at this point, as shown in the sketch; the direction of the dip is in each case indicated by an arrow. The water which issues from the point *w* was collected and brought to the surface in carefully cleaned stoneware jars.

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[*] Communicated by the Author, having been read before the Royal Society, January 30, 1873.

[†] My attention was first directed to the discovery of this spring by Mr. W. J. Henwood, F.R.S., of Penzance, who forwarded me in June last a cutting from a local newspaper headed "A Salt Spring in Huel Seton Mine." This notice states that the water had been sent to Mr. S. T. Rowe, analyst to the Truro Agricultural Association, and that an analysis had been returned "showing it to be highly impregnated with salt, salts of lime, and other chemical matter to the extent of 1072 grains to the imperial gallon." It then goes on to say, "very little iron, no copper, and no

The following results, in grammes per litre and grains per gallon, were obtained by analysis.

Mineral Water from Huel Seton.

Solid matter 14·3658 grammes per litre, or 1005·61 grains per gallon. Sp. gr. =1·0123.

Analytical Results.

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Carbonic acid	·0795	·0786	5·56	5·50
Sulphuric acid	·0178	·0177	1·25	1·24
Silica	·0270	·0280	1·89	1·96
Chlorine	9·1728	9·1662	642·10	641·63
Bromine	trace	trace	trace	trace
Alumina	·3456	·3460	24·19	24·22
Ferric oxide	·0031	·0033	·22	·23
Manganese	trace	trace	trace	trace
Copper	minute trace	minute trace	minute trace	minute trace
Lime	3·4795	3·4963	243·56	244·74
Magnesia	·0721	·0710	5·05	4·97
Alkaline chlorides.	6·4920	6·4626	454·44	452·38
Potassium	·0832	·0835	5·82	5·84
Cæsium*	trace	trace	trace	trace
Sodium	2·2977	2·2885	160·84	160·19
Lithium	·0805	·0794	5·63	5·56
Ammonia	trace	trace	trace	trace
Nitric acid	trace	trace	trace	trace

The foregoing results may be thus tabulated†:—

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gas, beyond carbonic acid, was found by the analyst." The presence of lithium is not mentioned. A few days after the receipt of the cutting above referred to, another copy was received from Mr. R. Hunt, F.R.S., and a third from Mr. J. H. Collins, F.G.S., of Falmouth. I have also much pleasure in acknowledging my obligations to Captain R. Williams, the Manager of the Mines, for the facilities afforded by him for the collection of the water for analysis.

* The amount of cæsium appears to be very small. On adding chloride of platinum to a rather dilute solution of the alkaline chlorides obtained from this water, a slight yellow precipitate was deposited; this, after resolution and the removal of the platinum by sulphuretted hydrogen, afforded by the spectroscope indications of the presence of cæsium.

† As the state of combination in which the various substances present in mineral waters exist cannot be accurately determined, the system of grouping adopted in the Table must to some extent be regarded as arbitrary.

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Calcium carbonate............	·0921	·1011	6·45	7·08
Ferrous carbonate............	·0045	·0047	·31	·33
Manganous carbonate	trace	trace	trace	trace
Calcium sulphate	·0303	·0301	2·12	2·11
Cupric chloride	minute trace	minute trace	minute trace	minute trace
Calcium chloride	6·7697	6·7934	473·88	475·54
Magnesium chloride	·1712	·1686	11·98	11·80
Aluminium chloride	·9003	·9013	63·02	63·09
Potassium chloride	·0919	·0900	6·43	6·30
Cæsium chloride	trace	trace	trace	trace
Sodium chloride	5·8442	5·8210	409·09	407·47
Lithium chloride	·4888	·4820	34·22	33·74
Potassium bromide	trace	trace	trace	trace
Potassium silicate ($K^2 SiO^3$).	·0693	·0719	4·85	5·03
Ammonia	trace	trace	trace	trace
Nitric acid......................	trace	trace	trace	trace
Total found by addition ...	14·4623	14·4641	1012·35	1012·49
Total found directly*	14·3658	......	1005·61	
Free carbonic acid............	·0373	·0323	2·61	2·26

The nature and amount of the various substances in solution in the saline waters having been determined, it was considered desirable to ascertain by analysis the composition of the principal rocks occurring in the neighbourhood of the spring. It was hoped that in this way information might be obtained respecting the sources from which some of the substances taken up by the water had been derived. It was further thought that, by comparing the composition of the rock in the immediate vicinity of the cross-course through which the heated waters issue with that of the clay-slate further removed from its influences, it might be possible to ascertain some of the effects produced by their action. In the case of each of the rocks analyzed, numerous thin sections were cut and subjected to careful microscopical examination.

"*Elvan,*" sp. gr. = 2·64.—The rock constituting the dyke G H, fig. 1, is exceedingly hard and compact, presenting the appearance of a crystalline greyish matrix, enclosing numerous well-defined crystals of flesh-coloured orthoclase; a specimen of this rock afforded by analysis the following results:—

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* The difference between the amount of total solid contents found directly and that obtained by the addition of constituents, is doubtless partly due to the partial decomposition of aluminium and magnesium chlorides at the temperature (160° C.) at which the drying of the residue was effected.

	I.	II.
Water*	1·53	1·56
Silica	70·76	70·85
Alumina	16·78	16·86
Ferrous oxide	2·44	2·50
Manganous oxide	1·24	1·18
Sulphur	trace	trace
Lime	1·23	1·21
Magnesia	1·55	1·52
Potassa	3·08	3·20
Soda	·85	·92
	99·46	99·80

Under the microscope this rock is seen to consist of a felsitic base containing crystals of orthoclase. The base is composed of highly altered felspar, pseudomorphs, apparently after hornblende, and numerous sphærulites exhibiting a radial structure. These sphærulitic bodies are not terminated by sharp outlines, but become gradually blended with the surrounding base; their radial structure, however, is perfectly defined, particularly when examined by polarized light. There is a green flocculent substance disseminated throughout the base; and many small patches of quartz are observed, which, by polarized light, appear to break up into granules. There are also prisms of a green colour, which are apparently altered hornblende; in a few cases one portion of a crystal is seen to be composed of some hornblendic mineral, while the remainder has been replaced by felspar. In some of the sections examined minute spots and microscopic crystals of iron pyrites were observed.

This rock would be generally described as a porphyry; but Mr. S. Allport, who at my request had the kindness to cut and examine a section of it, considers its composition would be better indicated by the name Porphyritic Felsite. It has, however, undergone a considerable amount of alteration; and although the larger crystals of orthoclase are for the most part unchanged, the smaller ones are almost without exception pseudomorphs.

Granite from Carn Brea, sp. gr. = 2·64.—The Carn Brea Hill, about a mile south-east of Huel Seton, is composed of a rather coarse-grained granite, in which the proportion of quartz is generally large; the felspar sometimes occurs in crystals of considerable size. It contains two varieties of mica, the one black (lepidomelane?), and the other white or slightly tinged with pink; the latter appears to be a variety of lepidolite.

The chemical composition of this granite was found to be as follows:—

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* Of which ·19 was lost in the water-bath.

	I.	II.
Water*	1·20	1·26
Silica	74·62	74·76
Alumina	16·29	16·13
Ferrous oxide	1·18	1·15
Ferric oxide	trace	trace
Manganous oxide	·55	·62
Lime	·30	·26
Magnesia	·50	·46
Potassa	3·54	3·74
Soda	1·24	1·13
Lithia	·11	·09
	99·53	99·60

A microscopical examination of this rock does not afford much information not to be obtained by a close inspection of hand-specimens. A large proportion of the felspar is seen to be monoclinic; but triclinic felspar is also present in considerable quantity. The outlines of the crystals are not in all cases sharply defined, and they are sometimes rendered slightly cloudy by some flocculent chloritic mineral. Two varieties of mica are distinctly visible, and occur in both quartz and felspar, together with a little tourmaline. The quartz is much fissured, the sides of the cracks being apparently coated by ferric oxide; it contains numerous fluid-cavities enclosing bubbles, which in some of the smaller ones are observed to be in constant motion.

Clay-slate &c.—In those portions of the mine situated at a distance from the cross-course, the "killas" is a very hard clay-slate of a grey colour. Cleavage is in the majority of cases to some extent obliterated; no trace of crystalline structure can be detected; and the rock, in addition to being traversed by numerous strings of white quartz, is thickly studded with minute spots and crystals of iron pyrites. Sp. gr. =2·73.

In the immediate vicinity of the fault, which appears to furnish a channel for the passage of the saline waters, the slate has become much altered, is of a dark green colour, consists to a great extent of minute acicular crystals, with their longer axes parallel to the planes of cleavage, and in many cases closely resembles some varieties of serpentine. Crystals of chlorite are found in the joints of cleavage. It also exhibits many of those highly polished surfaces so frequently met with in serpentinous rocks. Sp. gr. =2·69.

By analysis these rocks were respectively found to have the following compositions:—

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* Of which ·34 was lost in the water-bath.

	Unaltered clay-slate.		Rock in vicinity of cross-course.	
	I.	II.	I.	II.
Water*	3·13 *a*	3·10	6·97 *b*	6·84
Silica	67·78	67·87	45·85	45·97
Alumina	9·60	9·52	16·90	16·78
Ferrous oxide	5·09	4·96	10·59	10·72
Ferric oxide	trace	trace	trace	trace
Manganous oxide	1·27	1·14	1·16	1·12
Lime	2·62	2·54	2·68	2·68
Magnesia	3·45	3·37	6·54	6·87
Potassa	2·45	2·30	·92	·78
Lithia	......	......	trace	trace
Soda	4·46	4·32	5·64	5·53
Sulphur	·36 } = ·68	·32 } = ·60	·78 } = 1·46	·80 } = 1·50
Iron	·32 } FeS^2	·28 } FeS^2	·68 } FeS^2	·70 } FeS^2
Sulphuric acid	......	......	·76	·74
Chlorine	......	......	trace	trace
	100·56	99·72	99·47	99·53

Thin sections of the clay-slate situated at a distance from the cross-course are seen under the microscope to consist of bands of transparent granular quartz alternating with layers of similar quartz, through which a transparent, dichroic, pinkish-brown mineral is thickly disseminated in the form of imperfectly defined elongated crystals. The outline of these, of which the dimensions of the largest are $\frac{3}{1000} \times \frac{1}{1000}$ of an inch, is so irregular, and they are so crowded and interlaced, that it is impossible to determine their crystalline form; they are, however, believed to be hornblende.

When examined with an inch objective, the mass, in addition to numerous crystalline bodies, appears to be full of a brown dust, arranged in approximately parallel bands, which much diminish the transparency of those portions of the sections in which it most plentifully occurs. By the aid of a power magnifying 510 diameters, this dust is resolved into crystalline tufts of a dark colour and hornblendic appearance. Dendritic patches and crystals of iron pyrites are disseminated throughout the rock; and some of the veins by which it is traversed enclose well-defined crystals of both chlorite and hornblende.

The altered rock in the immediate vicinity of the cross-course is seen to be composed of closely matted prisms of brown hornblende enclosing pale green crystals of actinolite. It also contains a few crystals of quartz, and iron pyrites in the form of minute cubes.

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* *a* lost ·94 per cent. in the water-bath, and *b* 1·55 per cent.

Source whence the mineral waters of Huel Seton are probably derived.—Before attempting to account for the presence of the mineral constituents found in these waters, it will be necessary to consider the bearing and importance of the following facts:—

(*a*) The average elevation of the surface of the mine is about 300 ft. above the sea; and the 160-fathom level, being 960 feet below the adit or drainage-tunnel and 1080 feet from the surface, is consequently much beneath low-water mark.

(*b*) The cross-course, shown in the accompanying geological map, may, as has been already stated, be traced for a distance of three miles to the coast, and apparently forms the channel through which the saline waters effect an entrance into the workings.

(*c*) The water contains a very large proportion of chloride of sodium.

(*d*) Similar springs of hot saline water were met with, below the level of the sea, in the neighbouring mines of North Roskear and North Crofty, both situated on the same cross-course. These waters have not been analyzed.

(*e*) A hot spring yielding waters possessing the same general characteristics as those from Huel Seton, formerly issued at the Huel Clifford Mines in the 230-fathom level, or at a depth of 1320 feet below the sea. It will be seen, by referring to the Map, that in this case a well-defined cross-course can be continuously traced in a north-westerly direction from the immediate vicinity of the spring to the sea at Tobban Cove*.

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* These mines are now abandoned and consequently flooded. The waters issued at a temperature of 125° F., and at the rate of 150 gallons per minute. The late Dr. W. A. Miller, who analyzed them in 1864, obtained the following results:—

Sp. gr. = 1·007. The saline constituents were found by evaporation to amount to 646·1 grains per imperial gallon, consisting of:—

Chloride of lithium	26·05
Chloride of potassium with a little chloride of cæsium	14·84
Chloride of sodium	363·61
Chloride of magnesium	8·86
Chloride of calcium	216·17
Sulphate of calcium	12·27
Silica	3·65
Oxides of iron, alumina, and manganese.	minute quantity
	645·45

("Chemical Examination of a Hot Spring in Huel Clifford, Cornwall, by Professor W. A. Miller, M.D. &c.," Report of the Thirty-fourth Meeting of the British Association &c., held at Bath, September 1864.)

My friend, Mr. W. J. Henwood, has called my attention to the fact that so long ago as 1827 waters rich in chloride of sodium had been found in some of the Cornish mines by Mr. R. W. Fox. This gentleman says[*], "In some instances I have detected common salt, particularly in the water from the bottom of the United Mines, the Consolidated Mines[†], Huel Unity, and Poldice. Out of the 92 grains of residuum from the latter[‡], 24 grains proved to be the muriate of soda, 52 grains the muriates of lime and magnesia, and the remainder muriatic acid with iron and sulphate of lime. The water from another part of the same mine contained 5½ grains of common salt. All these mines are in 'killas' or primitive slate, and several miles from the sea. It may be inferred from such facts as these that the sea-water must in some places penetrate into the fissures of the earth."

Previously to discussing whether the water in question is or is not the result of infiltrations from the sea, it will be instructive to compare, by calculation, its composition with that of sea-water so diluted with distilled water as to contain the same amount of fixed constituents as that found in the Huel Seton spring[§]. Analyses of the waters of the German, Atlantic, and Pacific oceans agree so closely with one another as to justify the conclusion that the waters of the entire ocean have an essentially similar composition; but for the purpose of this comparison, an analysis of the waters of the Irish Sea by Messrs. T. E. Thorpe and E. H. Morton has been selected[‖]. This analysis has been chosen, both from the circumstance of its recent date, and also because, from the geographical position of the locality whence the water was obtained, it may be regarded as fairly representing the waters of the Atlantic Ocean which wash this portion of the Cornish coast.

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[*] Transactions of the Royal Geological Society of Cornwall, vol. iii. (1827) p. 324.

[†] Lately part of the Clifford Consolidated Mines.

[‡] From the evaporation of a pint of water.

[§] The error consequent on the assumption of the dilution being made with distilled water is not great. Four deep-mine waters recently analyzed gave an average of ·42 gramme of solid matter per litre, or less than 3 per cent. of the solid contents of the water in question.

[‖] Ann. Chem. Pharm. clviii. pp. 122–131.

	Water from the Irish Sea so diluted as to contain the same amount of solid matter per litre as the water from Huel Seton. Grammes per litre (calculated).	Mineral water from Huel Seton. Grammes per litre found (mean).
Chlorine	7·9070	9·1695
Bromine	·0260	trace
Sulphuric acid	·9172	·0178
Silica	....	·0275
Alumina	....	·3458
Ferric oxide	·0019	·0029
Manganese	....	trace
Copper	....	minute trace
Lime	·2441	3·4829
Magnesia	·8627	·0715
Alkaline chlorides	11·5393	6·4773
Potassium	·1665	·0834
Cæsium	....	trace
Lithium	....	·0800
Sodium	4·4157	2·2933
Ammonia	trace	trace
Carbonic acid	·0096	·0790
Nitric acid	·0006	trace

If the Huel Seton spring be regarded as only yielding modified and diluted sea-water, it will be found, on comparing the two columns of the foregoing Table, that the percentage of chlorine in the second has been considerably augmented, that the sulphuric acid has almost entirely disappeared, silica and alumina have been taken up, and the amount of calcium has been greatly increased, whilst magnesium has, on the contrary, been abstracted. The amount of alkaline chlorides has thereby been much reduced; but a considerable quantity of lithium has entered into solution.

It would at first appear that the presence of a much larger amount of chlorine in the waters of the saline spring than should be found in dilute sea-water holding an equal quantity of solid constituents in solution affords an argument against the probability of the Huel Seton waters having had such an origin. If, however, neglecting the smaller differences, the chlorides of calcium and aluminium in the spring-water be, on the one hand, replaced by an equal weight of chloride of sodium, and, on the other, the excess of sulphuric acid in the dilute sea-water be replaced by chlorine, making a correction for the resulting slight difference in the total weight, the amount of chlorine in the two columns will be found very nearly the same. This objection is consequently disposed of.

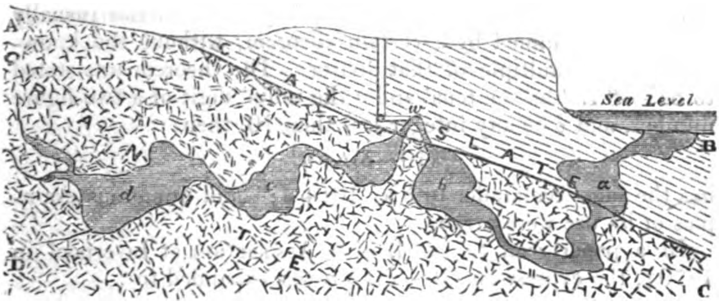
The analyses of the different rocks in the vicinity of the spring have shown that the granite only could have furnished the lithium, of which the waters have taken up a notable amount.

A consideration of the various phenomena connected with the occurrence of this and other apparently similar springs which have at different times been discovered in the district, would seem to lead to the inference that they all have some more or less direct communication with the sea, and that they are either the result of infiltration of sea-water through faults, or are true and independent sources, which, before being tapped below the sea-level, had found their way to the ocean through the faults or channels before alluded to.

It would be impossible to ascertain the precise conditions under which springs of this description have been produced; but the accompanying ideal sketch may perhaps assist in rendering intelligible what would appear, in the present state of our knowledge, a not improbable explanation of their origin.

The plane A B C D (fig. 2), being that of the cross-course, is seen to extend through both granite and clay-slate to the sea. From the close contact of its surfaces, the presence of clay, and from other causes, this fault is supposed not to be uniformly permeable by water, which can only follow the circuitous passage, *a b c d*. In this way it penetrates to depths where reactions take place, which, although not entirely in accordance with the results of daily experience in our laboratories, can, after the investigations of M. Daubrée, M. de Sénarmont, and others, be readily understood.

Fig. 2.



By the action of sea-water on *silicates of calcium*, *silicates of sodium* and *chloride of calcium* may be produced. The *sulphate of sodium* of the sea-water will be decomposed by the *chloride of calcium*, with the production of *sulphate of calcium* and *chloride of sodium*. The decomposition of clayey matter by common salt may produce *chloride of aluminium* and *silicates of sodium*, while the *magnesium* of the chloride of magnesium may be replaced by *calcium*; lastly, a portion of the *potassium* in the sea-water appears to have been replaced by the *lithium* of the granite.

The alteration which has taken place in slates in the vicinity of the cross-course has evidently been attended with the loss of a large percentage of its silica. In fact this appears to have been the chief chemical change effected, since, if we calculate what would be the composition of the clay-slate after the reduction of its percentage of silica to the amount contained in the altered rock, the calculated results will be found to agree very closely with the analysis of the latter. One per cent. of *lime*, however, has been exchanged for *alumina*, and about the same amount of *magnesia* has been substituted for an equivalent quantity of *potassa* and *soda*.

III. *On Negative and Fractional Unitates.*

By W. H. Walenn[*].

THE definition of a unitate, as originally given in the year 1868[†], is:—The remainder to any given divisor (usually a digit), determined by a certain theorem, without the knowledge of any multiple of that divisor. The theorem was stated to be:—"If t be the tens' and u the units' digit of a two-figure number, and δ be any integer less than 10, then

$$(10-\delta)t+u$$

has the same remainder to δ as $10t+u$;" and numerous examples of its use were given.

In a paper read to the British Association at Liverpool in 1870[‡], the expression $(10-\delta)t+u$ was expanded to

$$(10-\delta)^{n-1}a+(10-\delta)^{n-2}b+(10-\delta)^{n-3}c+\ldots+(10-\delta)^2s$$
$$+(10-\delta)t+u,$$

n being the number of digits in the given number—so as to adapt it to other than two-figure numbers; and further examples of its use were given.

The examples showed that this theorem is useful, especially when $\delta=9$, in checking calculations and verifying Tables; when other values of δ are used, it is applicable to quickly ascertaining the remainders to divisors.

In his address to the Mathematical Section at the British

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[*] Communicated by the Author.

[†] Phil. Mag. S. 4. vol. xxxvi. p. 346.

[‡] Report of the British Association for the Advancement of Science for 1870, Transactions of the Sections, p. 16.

Association Meeting of 1869 at Exeter, Professor J. J. Sylvester laid much stress upon the employment of inductive philosophy in mathematics. He said that he was aware that many who had not gone deeply into the principles of mathematical science believed that inductive philosophy, or the method of evolving new truths by induction, was reserved for the experimental sciences, and that the methods of investigation in mathematical science might all be classified as deductive. He went on to say that this opinion is not a correct one, and that many valuable results are obtained in mathematical science by induction, or reasoning from particulars to generals, which could not otherwise be obtained so easily. Although making a distinction between mathematical induction and the induction used in natural philosophy, De Morgan, in his article in the 'Penny Cyclopædia' on this subject, states that an instance of mathematical induction occurs in every equation of differences and in every recurring series. Taking the definition of induction as given by Dr. Whateley, namely, "a kind of argument which infers respecting a whole class what has been ascertained respecting one or more individuals of that class," it will be evident to any experimenter in chemical or physical science who is also acquainted with the use of induction in mathematical science, that mathematical induction is of a higher and more perfect kind than the induction used in the physical sciences, especially when it assumes the form of successive induction as De Morgan calls it, and as it is employed in recurring series.

It is this high class of reasoning, which is involved in the construction of series that recur according to a given law, that makes the use of recurring series so valuable in unitation. Nevertheless, in addition to the policy of investigating every general result by the use of a more general application of inductive philosophy, namely by the calculation or examination of particular examples (and so, having by one train of reasoning obtained a general result, proving or illustrating the use of that general result by particular applications), it is especially necessary to study examples of every possible type in unitation. It is moreover necessary to have other means than the principles of recurring series, and of verification by examples, to ascertain the truth of certain results, in consequence of the number of anomalies that present themselves for explanation.

These anomalies occur not only at the ordinary *singular* values that the theory of numbers affords, such as 0 and 1, but the base of the system of unitation—take 9 for instance—together with the submultiples of that base, namely six and three, also furnish singular or anomalous values.

In respect of operations, as might be expected, inverse operations, such as subtraction and division, when regarded as operations upon unitates, involve mathematical investigation and a rigid comparison of results. These results used for comparison, may either consist of the separate determination of unitates by totally distinct processes, or of individual examples obtained by the application of unitation to checking calculations that are known to be correct.

Applying the principles and modes of thought sketched out above to the examination of the meaning of a negative unitate, the following results are arrived at. It is important to bear in mind certain properties of unitates. 1st. By definition, the unitate of a given number to a given base, say 9, is the remainder obtained by dividing the given number by the base, 9; it must therefore always be a plus or positive quantity, equal to or less than 9. 2nd. Nought (or 0) never occurs in unitation; when there is no remainder the unitate of the number is 9. The three cases into which subtraction of unitates resolves itself are:—(I.) When the unitate of the subtrahend, or number to be subtracted, is less than the unitate of the minuend or quantity to be diminished. In this case the subtraction of the former unitate from the latter leads to a plus or positive unitate, which is the unitate of the remainder. (II.) When the unitate of the subtrahend is equal to that of the minuend. The subtraction leads to 0 as a unitate; this may at once be written 9. (III.) When the unitate of the subtrahend is greater than that of the minuend. Regarded algebraically, this subtraction leads to a negative unitate, and, to obtain its real value, it must be subtracted from 9. Thus when, in unitation, the expression $6-8$ occurs, it may be written -2, but should be at once reduced to a positive quantity by adding 9 to it, thus $9-2=7$, 7 is the unitate required; if it be desired to give the -2 a name, it may be called the co-unitate of 7. For arithmeticians only, it may in practice be the best plan to add 9 to the unitate of the minuend in the last two cases before performing the subtraction of the unitate of the subtrahend therefrom.

The performance of the operation of division upon unitates is derived from the performance of the same operation upon ordinary numbers; but, unlike that operation upon ordinary numbers of which it is the analogue, it always has a remainder, or may be presumed to have one. This is a consequence of no unitate being equal to 0; for instance, in $\frac{25}{5}=5$, the unitate of $\frac{25}{5}=\frac{7}{5}$; the unitate of the quotient 5 may be written $5+\frac{9}{5}$, which is (for the purpose of checking the division of 25 by 5) equal to $\frac{25+9}{5}$, or $\frac{34}{5}$, or $\frac{7}{5}$. Therefore unitates of all quotients may be obtained by preserving them in the fractional form which they originally have—neither neglecting the remainder, nor reducing any fraction which may thence arise to lower terms, but preserving the exact unitate of the divisor to the very end of the process, either in reducing the unitate of the fraction which expresses the division to be checked, or in reducing the unitate of the quotient (or answer), including the unitate of the remainder. If there be no remainder, the unitated remainder must be a fraction which has 9 for its numerator. Division of unitates, therefore, renders the investigation of fractional unitates necessary.

Since all divisions can be written under the form $a \cdot \frac{1}{b}$, and since $\frac{1}{b}=b^{-1}$, it follows that the question of fractional unitates is identical with that of the unitates of the (-1)th power of numbers, and also with that of the unitates of reciprocals decimally expressed. In the annexed Table of unitates of the powers of numbers, it is observable that the unitates of the same power of different numbers (the vertical columns) repeat themselves after every nine consecutive numbers; also that (with some exceptions presently to be noticed) the unitates of different powers of the same number (the horizontal columns)

Uni. a^{-1}.	Uni. a^{0}.	Uni. a^{1}.	Uni. a^{2}.	Uni. a^{3}.	Uni. a^{4}.	Uni. a^{5}.	Uni. a^{6}.	Uni. a^{7}.	Uni. a^{8}.	Uni. a^{9}.	Uni. a^{10}.
1	1	1	1	1	1	1	1	1	1	1	1
5	1	2	4	8	7	5	1	2	4	8	7
$\frac{1}{3}$	1	3	9	9	9	9	9	9	9	9	9
7	1	4	7	1	4	7	1	4	7	1	4
2	1	5	7	8	4	2	1	5	7	8	4
$\frac{1}{6}$	1	6	9	9	9	9	9	9	9	9	9
4	1	7	4	1	7	4	1	7	4	1	7
8	1	8	1	8	1	8	1	8	1	8	1
$\frac{1}{9}$	1	9	9	9	9	9	9	9	9	9	9
1	1	1	1	1	1	1	1	1	1	1	1

repeat themselves after every six consecutive powers. In this way the column for reciprocals, headed $\overset{\text{Uni.}}{a^{-1}}$ is obtained, so far as the numbers 1, 5, 7, 2, 4, 8 are concerned; for these are also the unitates of the corresponding decimals to the reciprocals, the unitate of 7^{-1} or of $\frac{1}{7}$ being obtained from the unitate of the decimal value of $\frac{1}{16}=\cdot0625$. To test the correctness of these unitates, the principle that every number is the reciprocal of its own reciprocal may be applied; accordingly the following equations are true in unitates, bearing in mind that, in the case of unitates, the signs +, −, ×, ÷, and = have not the same signification as in ordinary arithmetic. The words "the unitate of" are understood to be placed before every numerical value set down.

$$1=\frac{1}{1}=1.$$

$$2=\frac{1}{5}=\frac{9+1}{5}=2.$$

$$3=\frac{1}{\frac{1}{3}}=1\times3=3.$$

$$4=\frac{1}{7}=\frac{(3\times9)+1}{7}=\frac{28}{7}=4.$$

$$5=\frac{1}{2}=\frac{9+1}{2}=\frac{10}{2}=5.$$

$$6=\frac{1}{\frac{1}{6}}=1\times6=6.$$

$$7=\frac{1}{4}=\frac{(3\times9)+1}{4}=\frac{28}{4}=7.$$

$$8=\frac{1}{8}=\frac{(7\times9)+1}{8}=\frac{64}{8}=8.$$

$$9=\frac{1}{\frac{1}{9}}=1\times9=9.$$

Another property of reciprocals that will test these results is, that the product of any number multiplied by its reciprocal is always equal to unity. This is found to be true in carrying out the column headed $\overset{\text{Uni.}}{a^{-1}}$ in the above square Table according to this principle; and it is clear that neither 3, 6, nor 9 multiplied by any other whole number can give a product whose unitate is 1.

The operation of multiplying the unitates by their reciprocals is

$1\times1=1.$

$2\times5=10=1.$

$3\times\frac{1}{3}=\frac{3}{3}=1.$

$4\times7=28=10=1.$

$5\times2=10=1.$

$6\times\frac{1}{6}=\frac{6}{6}=1.$

$7\times4=28=10=1.$

$8\times8=64=10=1.$

$9\times\frac{1}{9}=\frac{9}{9}=1.$

It is to be remarked that the discontinuity of the series in the case of the unitates of the powers of 3, 6, and 9 respectively, which is evident from the second and higher powers not following the same law of recurrence as the first and powers below the first follow, is an indication of departure from a general rule, which is borne out by the Table of reciprocals calculated by means that are totally independent of series.

From the values that have been thus obtained and verified for the unitates of the reciprocals of 1, 2, 4, 5, 7, 8, it follows that, in any division sum which has these unitates in the divisor, the corresponding unitates of their reciprocals may be written as multipliers to find the unitate of the quotient. For instance, in $\frac{25}{5}=5$, as above given, the following is the readiest process of unitation—the unitate of $\frac{25}{5}=7\times 2=14=5$, and the unitate of $5=5$. This substitution of multiplication for division cannot be made in the case of the unitates of the reciprocals of 3, 6, or 9; therefore in the cases in which 3, 6, or 9 is in the denominator, and in these cases only, it is necessary to preserve the fractional form of the unitate during the whole of the process of unitation.

Thus, considered *in extenso*, subtraction gives birth to co-unitates or negative unitates, and inverse powers, or the operation of division, to fractional unitates, or unitates that are irreducible to the whole number form.

74 Brecknock Road, N.,
June 1873.

IV. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* *By* JAMES D. DANA*.

PART I.

PREPARATORY to a discussion of some questions connected with the earth's contraction, I here present a statement of the views which I have entertained with regard to the prominent results of this agency. They first appeared in 1846 and 1847, in volumes ii., iii., and iv. of the Second Series of the American Journal of Science, and were somewhat extended in 1856 in vol. xxii.† Full credit is given to earlier writers in connexion with the articles referred to. The views are as follows‡:—

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* From the American Journal of Science and Arts, vol. v. June 1873. Communicated by the Author, with additional notes.

† Vol. ii. p. 385, iii. pp. 94, 176, 380, iv. p. 88, xxii. pp. 305, 335.

‡ I may add in this place that a sight of Mädler's chart of the moon in 1846, six years after my visit to the crater of Kilauea, in the Wilkes Exploring Expedition, prompted to the first of the articles on the subject (that on the Volcanoes of the Moon, vol. ii. pp. 335, 1846), in which the origin of continents and oceanic basins is considered. The most important

1. The defining of the continental and oceanic areas began with the commencement of the earth's solidification at surface, as proved by the system of progress afterward.

2. The continental areas are the areas of least contraction, and the oceanic basins those of greatest, the former having earliest had a solid crust. After the continental part was thus stiffened and rendered comparatively unyielding, the oceanic part went on cooling, solidifying, and contracting throughout; consequently it became depressed, with the sides of the depression somewhat abrupt. The formation of the oceanic basins and continental areas was thus due to "unequal radial contraction"*.

3. The principal mountain-chains are portions of the earth's crust which have been pushed up, and often crumpled or plicated, by the lateral pressure resulting from the earth's contraction.

4. (*a*) Owing to the lateral pressure† from contraction over

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of Prevost's papers on the origin of mountains had been published six years before, but I knew nothing of his views until after my paper was ready for publication, as I remark in a paragraph near its close.

* The principle thus expressed by Professor LeConte in volume iv. of the same journal (1872) does not differ essentially from my old view, except that it is connected with the idea of a solid globe. Professor LeConte, on p. 466 of his article, attributes to me the opinion that the "sinking of sea-bottoms, determined by interior contraction, is the [source of the] force by which continents are elevated." But I have never referred the origin of continents to such a cause, or to any other than that stated above.

Moreover the elevation of mountains on the borders of continents I have attributed, not to "sinking sea-bottoms" merely, but to lateral pressure produced by contraction over continental as well as oceanic areas, that on the oceanic being made much the greatest, as stated beyond. My language is frequently ambiguous on this last point, because I speak of the oceanic as the "subsiding" areas. But the term is used relatively. In volume iii. on p. 179 (1847) I observe that mountain elevations occur "near the limit between the great contracting and the non-contracting (comparatively non-contracting) areas;" and in various places I describe the contraction as general. In my 'Manual of Geology,' on page 732, I remark that the elevating "force acted most strongly from the oceanic direction," which was the idea throughout. I do not deny, however, that I have supposed too large a part of the lateral force to have come from the special contraction and consequent subsidence of the oceanic part of the globe.

Professor N. S. Shaler, in 1866 (Proc. Boston N. H. Soc. vol. x. p. 237, vol. xi. p. 8, and Geol. Mag. vol. v. p. 511), presented, *as original*, the idea that "mountain-chains are only folds of the outer portion of the crust caused by the contraction of the lower regions of the outer shell," and that "the subsidence of ocean-floors would, by producing fractions and dislocations along shore-lines, tend to originate mountain-chains along sea-borders and approximately parallel to them," which is essentially the view that LeConte attributes to me. These ideas are coupled with others respecting limitations of the action of contraction due to denudation and deposition, in which I have no share.

† In my papers in 1847 I used the terms lateral pressure, lateral force,

both the continental and oceanic areas, and to the fact that the latter are the regions of greatest contraction and subsidence, and that their sides pushed, like the ends of an arch, against the borders of the continents, therefore, along these borders, within 300 to 1000 miles of the coast, a continent experienced its profoundest oscillations of level, had accumulated its thickest deposits of rocks, underwent the most numerous uplifts, fractures, and plications, had raised its highest and longest mountain-chains, and became the scene of the most extensive metamorphic operations, and the most abundant outflows of liquid rock.

And (*b*) since the most numerous and closest plications, the greatest ranges of volcanoes, the largest regions of igneous eruption and metamorphic action exist on the *oceanic slope* of the border mountain-chains instead of the continental, therefore the lateral pressure acted most effectively in a direction *from* the ocean.

(*c*) Since these border features are vastly grander along that border of a continent which faces the largest ocean, therefore the lateral pressure against the sides of a continent was most effective on the border of the largest oceanic basin, and for the two (the Pacific and Atlantic) was approximately proportioned to the extent of the basins,—this being due to the fact that the oceanic were the subsiding areas (that is, those which contracted most), and that the larger area became the most depressed.

5. The oscillations of level that have taken place over the interior of North America, through the geological ages, have in some degree conformed in direction of axis to those of the border regions, all being parts fundamentally of two systems of movements—one dominantly in a direction northwestward or from the Atlantic, the other northeastward or from the Pacific.

6. Owing to the approximate uniformity of direction in the lateral thrust under these two systems through the successive ages (a consequence of the isolated position of the continent between two oceanic basins, transverse to one another in axial direction), mountains *of different ages* on the same border, or part of a border, have approximately the *same trend*, and those *of the same age* on the opposite border (Pacific and Atlantic) have in general *a different and nearly transverse trend.* Hence "one dial plate for the mountains of the world, such as Elie de Beaumont deduced mainly from European geology, will not

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tension, horizontal force, force acting tangentially, as synonyms. "Lateral pressure" was the term oftenest employed, and it was explained by reference to a Prince Rupert's drop. (See Amer. Journ. Sci. 2nd ser. vol. iii. p. 96 &c.) The action appealed to was not in any way different from the "tangential thrust" of Mallet.

mark time for America" (Amer. Journ. Sci., 2nd ser. vol. iii. p. 398, 1847; vol. xxii. p. 346, 1856).

7. The features of the North-American continent were to a great extent defined in præ-Silurian time, the course of the Azoic, from the Great Lakes to Labrador, being that of the Appalachians, and various ridges in the Rocky Mountains foreshadowings of this great chain, and so on in many lines over the continental surface; and thus its adult characteristics were as plainly manifested in its beginnings as are those of a vertebrate in a half-developed embryo.

8. Metamorphism of regions of strata has taken place only during periods of disturbance, or when plication and faults were in progress—all metamorphic regions being regions of disturbed and generally of plicated rocks.

The heat required for alteration came up from the earth's liquid interior. (This part of the view requires modification, while the other part, I believe, remains good.)

9. The volcanoes of the continental areas are mostly confined to the sea-borders, or the oceanic slope of the border mountain-chains—not because of the vicinity of salt water, but because these were the regions of greatest disturbance and fractures through lateral pressure. Volcanoes are indexes of danger, never "safety-valves."

10. Earthquakes were a result of sudden fracturings and dislocations proceeding from lateral pressure. In vol. iii. p. 181 (1847) occurs the remark :—"We see that the lateral pressure exerted would be likely to dislocate;" and in the next line, "such fissurings, whether internal or external, would cause shakings of the earth (*earthquakes*) of great violence, and in all periods of the earth's history, and it might be over a hemisphere at once."

Another important subject (that of the systems in the trends of feature-lines over the globe) is discussed in the articles referred to; but I pass it by for the present.

I propose to bring the above principles under consideration with reference to making such changes as may now be necessary.

I take up, first, the question as to whether oscillations of level (that is, subsidences and elevations) have been made by the lateral pressure resulting from contraction, as is assumed in my writings on the subject and those of most other authors; and how was the lateral thrust from the direction of the oceanic areas made to differ in its results from that from the opposite direction? After which I shall pass to the subjects of metamorphism, igneous eruptions, volcanoes, the earth's interior, and the origin of oceanic basins.

1. *Have subsidences been produced by lateral pressure?*

The theory of Professor James Hall, that the great subsidences of the globe have been made by the gravity of accumulating sediments, has been shown elsewhere* to be wholly at variance with physical law.

Another theory is presented by Professor LeConte in his recent paper in the last volume of the American Journal of Science, to which the reader is referred. Admitting, with Professor Hall, that the mean thickness of the accumulations in the Appalachian region of Pennsylvania is 40,000 feet, and therefore that this is the measure of the gradual subsidence that attended their deposition, he shows that the temperature in the bottom deposits would have been, supposing the usual rate of increase downward (1° F. for 58 feet of descent) 800° F., and at 10,000 feet 230° F.; and he argues that hence there would have resulted below, first, "lithification and therefore increasing density, and therefore *contraction and subsidence pari passu* with the deposit;" next, or at a greater depth, "aqueo-igneous softening" or "melting," the temperature of 800° F. being "certainly sufficient to produce this result as well as metamorphism, and during this process the subsidence would probably continue;" and, in addition, the underlying strata on which the sediments were deposited would have participated in the "aqueo-igneous fusion" and thus have added to the result†.

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* Amer. Journ. Sci. 2nd ser. vol. xlii. p. 210 (1866); 3rd ser. vol. v. p. 347 (1873); LeConte, ib. 3rd ser. vol. iv. p. 461 (1872).

† The principal points in Professor Hall's theory of mountains, published in 1859 (Amer. Journ. Sci. 3rd ser. vol. v. p. 347), are:—

1. Coast regions the courses of marine currents, and hence of deposited sediments.
2. The accumulation of sediments by their gravity gradually sink the crust, and thus a great thickness is attained; the rocks become solidified and sometimes crystallized below.
3. The continents afterward somehow raised—not the mountain-regions separately.
4. Shaping of the mountains out of other sediments by denudation.
5. Metamorphism due to "motion," "fermentation," and a little heat; the heat coming up from below (the isogeothermal planes rising) in consequence of the increasing accumulations at surface.

In Professor LeConte's theory (Amer. Journ. Sci. 3rd ser. vol. iv. pp. 345, 460 (1872):—

1. The same as in Professor Hall's.
2. As explained in the text above.
3. After an aqueo-igneous softening of the beds below, the lateral thrust from the earth's contraction pressed together the region of sedimentary accumulation, plicating and crushing the beds.
4. The elevation of mountains due solely to crushing and plication.
5. Metamorphism consequent on the heating derived by the rise of the isogeothermal planes.

No other cause of the gradual subsidence than that here cited is appealed to.

Now *the whole of this contraction took place,* if any occurred, *in the underlying Archæan rocks* (Azoic, or Laurentian and Huronian); for in obtaining by measurement this thickness (40,000 feet) the *contracted* rocks were measured.

The 40,000 feet of subsidence required was therefore wholly independent of contraction in the stratified sediments. But these underlying Archæan rocks were probably crystallized before the Palæozoic era began; for in New York and New Jersey they are in this condition, and they underlie the Silurian rocks unconformably; and the New-Jersey Archæan or Highland region is but a northern part of that of Pennsylvania and Virginia. They would consequently have expanded with the heat instead of contracting. Even if not crystallized, they would have been well compacted under the enormous weight of 40,000 feet of strata; and no experiments on rocks that I have met with authorize the assumption that the ordinary law of expansion from heat would have been set aside.

For further argument on this point I refer to the subsidence in the Connecticut valley during the era of the Connecticut river sandstone (supposed to be Triassico-Jurassic). The thickness of rock produced in the era was probably about 4000 feet; and this is the extent, therefore, of the registered subsidence. The sandstone strata, as is apparent in many places, rest on the upturned metamorphic rocks (gneiss, mica-schist, &c.) of Palæozoic or earlier age. As shown in the preceding paragraph, the contraction, under Professor LeConte's principle, must have been confined to the underlying rocks; and since these are crystalline metamorphic schists, and the depth of sandstone was not sufficient to raise much the temperature within them (the rocks are in general little compacted and often feebly solidified), the heat ascending from below as accumulation went on above would have produced expansion instead of contraction.

Without further reference to facts, it is, I think, clear that the subsidence required could not be obtained by the method appealed to by Professor LeConte. Whatever cause, in either of the above cases, occasioned the subsidence, it must have been one that could do its work in spite of opposition on the part of the heat in the rocks themselves or those below.

Another cause of local subsidence is local cooling beneath, accompanying the increasing accumulation of sediments. But this idea is too obviously absurd to require remark.

In the present state of science, then, no adequate cause of subsidence has been suggested apart from the old one of lateral pressure in the contracting material of the globe.

2. *Have elevations been produced directly by lateral pressure?*

The theory of Professor Hall denies that mountains are a result of *local* elevations, or of any elevation apart from a general continental. This hypothesis I have elsewhere discussed*.

Professor LeConte makes the elevation of mountains real; but after explaining that the crushing effects of lateral thrust would necessarily cause a lengthening upward of the compressed strata (as in the compression of slate rocks attending the production of slaty cleavage), and thereby produce a large amount of actual elevation, arrives at the view that there is no permanent elevation beyond what results from crushing. With crushing, in this action, plication is associated; but it should have a larger place than his words seem to give it (in all plication the rocks over a region being pressed into a narrower space, which could be done only by adding to the height), as it has performed tenfold more work of this kind than crushing.

But are plication and crushing the only methods of producing, under lateral pressure, the actual elevations of mountain-regions? Is there not real elevation besides?

In the later part of the Posttertiary or Quaternary era the region about Montreal was raised nearly 500 feet, as shown by the existence of sea-beaches at that height; and similar evidence proves that the region about Lake Champlain was raised at the same time at least 300 feet, and the coast of Maine 150 to 200 feet. Hence the region raised was large. No crushing or plication of the upper rocks occurred; and none in the under rocks could well have taken place without exhibitions at the surface; and this cause, therefore, cannot account for the elevation. The elevated sea-border deposits of the region are in general horizontal. This example is to the point as much as if a mountain had been made by the elevation.

But we have another example on a mountain-scale, and one of many. Fossiliferous beds over the higher regions of the Rocky Mountains are unquestioned evidence that a large part of this chain has been raised 8000 to 10,000 feet above the ocean-level since the Cretaceous era†. The Cretaceous rocks, to which these fossiliferous beds belong, were upturned in the course of the slowly progressing elevation; and so also were part of the Tertiary beds; for the elevation went forward through the larger part, or all, of the Tertiary era. But the local crushing or plication of these beds cannot account for the elevation; and no other crushing among the surface-rocks of the mountains can be referred to this era. There may have been a crushing and crumpling of the nether rocks of the mountain. But it must also be admitted that there might have been, under tangential pressure, a bending of the strata without crushing, especially if there is beneath the earth's rind along the continental borders a region or layer of "aqueo-igneous fusion," such as Professor LeConte recognizes.

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* Amer. Journ. Sci. 2nd ser. vol. xlii. pp. 205, 252 (1866), and 3rd ser. vol. v. p. 347 (1873).

† The height of the Cretaceous (stratum No. 2 of the Upper Missouri Cretaceous) at Aspen, in Wyoming, is full 8000 feet above tide-level (Meek). Beds occur also in South Park, Colorado, the height of which is 8500 feet; and, according to Hayden, in the region of the Wind-River Mountains the beds have a height of 10,000 to 11,000 feet above the sea.

In the course of the geological history of the North-American continent, there were many oscillations of level in the land. Portions that were raised above the sea-level in one era, in another subsided again and sank beneath it; and Professor LeConte, in the course of his discussion, admits the existence of an elevated region along the Atlantic border which afterward disappeared. Had the elevation in the case of such oscillations been dependent on plication and crushing beneath, so complete a disappearance afterward would have been very improbable.

Such facts as the above appear to prove that elevatory movements have often been, like those of subsidence, among the direct results of lateral pressure. The facts are so well known and the demonstration so generally accepted as complete, that I have suspected that there is here an unintentional omission or oversight in Professor LeConte's paper.

3. *Kinds and Structure of Mountains.*

While mountains and mountain-chains all over the world, and low lands also, have undergone uplifts in the course of their long history that are not explained on the idea that all mountain-elevating is simply what may come from plication or crushing, the *component parts* of mountain-chains, or those simple mountains or mountain-ranges that are *the product of one process of making,* may have received, *at the time of their original making,* no elevation beyond that resulting from plication.

This leads us to a grand distinction in orography hitherto neglected, which is fundamental and of the highest interest in dynamical geology—a distinction between:—

1. A simple or *individual* mountain-mass or range, which is the result of *one process of making,* like an individual in any process of evolution, and which may be distinguished as a *monogenetic* range, being *one in genesis*; and

2. A composite or *polygenetic* range or chain made up of two or more monogenetic ranges combined.

The Appalachian chain (the mountain-region along the Atlantic border of North America) is a polygenetic chain; it consists, like the Rocky and other mountain-chains, of several monogenetic ranges, the more important of which are:—(1) The Highland range (including the Blue Ridge or parts of it, and the Adirondacks also, if these belong to the same process of making) præ-Silurian in formation. (2) The Green-Mountain range, in western New England and eastern New York, completed essentially after the Lower Silurian era or during its closing period. (3) The Alleghany range, extending from southern New York southwestward to Alabama, and completed immediately after the Carboniferous age.

The making of the Alleghany range was carried forward at first through a long-continued subsidence—a *geosynclinal** (not a *true* synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments which occupied the whole of Palæozoic time; and it was completed finally in great breakings, faultings, and foldings or plications of the strata along with other results of disturbance. The folds are in several parallel lines, and rise in succession along the chain, one and another dying out after a course each of 10 to 150 miles; and some of them, if the position of the parts which remain after long denudation be taken as evidence, must have had, it has been stated, an altitude of many thousand feet; and there were also faultings of 8000 to 10,000 feet, or, according to Lesley, of 20,000 feet†. This is one example of a *monogenetic* range.

The Green Mountains are another example, in which the history was of the same kind:—first, a slow subsidence or geosynclinal, carried forward in this case during the Lower Silurian era or the larger part of it, and, accompanying it, the deposition of sediments to a thickness equal to the depth of the subsidence; finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, &c. between the sides of the trough.

In the Alleghany range the effects of heat were mostly confined to solidification, the reddening of such sandstones and shaly sandstones as contained a little iron in some form‡, the coking of the mineral coal, and probably, on the western outskirts where the movements were small, the distillation of mineral oil, through the heating of shales or limestones containing carbohydrogen material, and its condensation in cavities among overlying strata, with also some metamorphism to the eastward; while in the making of the Green Mountains there was metamorphism over the eastern, middle, and southern portions, and imperfect metamorphism over most of the western side to almost none in some western parts.

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* From the Greek γῆ *earth*, and *synclinal*, it being a bend in the earth's crust.

† See an admirable paper on these mountains by Professors W. B. and H. D. Rogers in the Trans. Assoc. Amer. Geol. and Nat. 1840-42. J. P. Lesley gives other facts in his 'Manual of Coal and its Topography,' and in many memoirs in the 'Proceedings of the American Philosophical Society.' A brief account is contained in the author's 'Manual of Geology.'

‡ Oxide of iron produced by a wet process at a temperature even as low as 212° F. is the red oxide, Fe^2O^3, or at least has a red powder. (Am. Journ. Sci. 2nd ser. vol. xliv. p. 292.)

Another example is offered by the Triassico-Jurassic region of the Connecticut valley. The process included the same stages in kind as in the preceding cases. It began in a geosynclinal of probably 4000 feet, this much being registered by the thickness of the deposits; but it *stopped short of metamorphism*, the sandstones being only reddened and partially solidified—and *short of plication or crushing*, the strata being only tilted in a monoclinal manner 15° to 25°; it ended in numerous great longitudinal fractures as a final catastrophe from the subsidence, out of which issued the trap (Dolerite) that now makes Mount Holyoke, Mount Tom, and many other ridges along a range of 100 miles*.

These examples exhibit the characteristics of a large class of mountain-masses or ranges. A geosynclinal accompanied by sedimentary depositions, and ending in a catastrophe of plications and solidification, are the essential steps, while metamorphism and igneous ejections are incidental results. The process is one that produces final stability in the mass and its annexation generally to the more stable part of the continent, though not stable against future oscillations of level *of wider range*, nor against denudation.

It is apparent that in such a process of formation elevation by direct uplift of the underlying crust has no necessary place. The attending plications may make elevations on a vast scale; and so also may the shoves upward along the lines of fracture; and crushing may sometimes add to the effect; but elevation from an upward movement of the downward-bent crust is only an incidental concomitant, if it occurs at all.

We perceive thus where the truth lies in Professor LeConte's important principle. It should have in view alone *monogenetic* mountains, and these only *at the time of their making*. It will then read (plication and shovings along fractures being made more prominent than crushing) :—

Plication, shoving along fractures, and crushing are the true sources of the elevation that takes place *during the making* of geosynclinal monogenetic mountains.

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* This history is precisely that which I have given in my 'Manual of Geology,' though without recognizing the parallelism in stages with the history of the Alleghanies.

And the statement of Professor Hall may be made right if we recognize the same distinction, and also reverse the order and causal relation of the two events, accumulation and subsidence; and so make it read:—

Regions of monogenetic mountains were, previously and preparatory to the making of the mountains, areas each of a slowly progressing geosynclinal, and *consequently* of thick accumulations of sediments.

The prominence and importance in orography of the mountain individualities described above as originating through a geosynclinal make it desirable that they should have a distinctive name; and I therefore propose to call a mountain-range of this kind a *synclinorium*, from *synclinal* and the Greek ὄρος, *mountain*.

This brings us to another important distinction in orographic geology, that of a second kind of monogenetic mountain. The *synclinoria* were *made through a progressing geosynclinal.* Those of the second kind, here referred to, were *produced by a progressing geanticlinal.* They are simply the upward bendings in the oscillations of the earth's crust, the geanticlinal waves, and hardly require a special name. Yet, if one is desired, the term *anticlinorium*, the correlate of *synclinorium*, would be appropriate. Many of them have disappeared in the course of the oscillations; and yet some may have been for a time, perhaps millions of years, respectable mountains. The "Cincinnati uplift," extending southwestward from southern Ohio (about Cincinnati) into Tennessee, and referred by Newberry and others to the close of the Lower Silurian, was made at the same time, or nearly, with the Green Mountains; but while the latter range is a synclinorium, the former is a geanticlinal or an anticlinorium, and it is one of the few (probably few) permanent monogenetic elevations of this kind over the earth's surface. There may possibly have been crumpling or crushing in the deep-seated rocks below, which determined its permanence. As far as the Palæozoic rocks constituting it go, it is a *simple* synclinal; but it is really a synclinal of the earth's crust, and hence wholly distinct from ordinary synclinals, or those subordinate among the plications in a synclinorium, like the synclinals of the Alleghanies.

The geosynclinal ranges or synclinoria have experienced in almost all cases since their completion true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements, indeed a range of crust that comes strictly under the designation of a polygenetic mass. Thus the Connecticut-valley sandstone beds, which must have been but little raised by the slight upturning they underwent at the epoch of their disturbance (since there was then neither plication nor crushing), are now 700 feet higher above the sea-level in Massachusetts than near New Haven, Connecticut; and this is owing, not to denudation, but to a subsequent elevation in which much of New England participated—a true geanticlinal uplift. So it has been the world over. The great uplift of the Rocky-Mountain region of more than 8000 feet, which began after the Cretaceous, had nothing to do, as I have said, with crushing or plication, although there was disturbance of the beds in certain local Cretaceous and Tertiary areas; it appears to have been a true geanticlinal elevation of the Rocky-Mountain mass, itself mainly, if not wholly, a combination of synclinoria.

Geosynclinals and geanticlinals of low angle, like those of the present day, graduate insensibly into horizontal surfaces. The later oscillations in the world's history have taken in a vastly wider range of crust than those of early time. We cannot point to any geosynclinal in progress that is probably on the way to become the site of a new synclinorium. This comes from the fact already stated, that the completion of a synclinorium has generally consisted in the solidification as well as plication of the rocks, and the addition of the whole mountain-region to the more stable portion of the earth's crust; and the further fact that this process has been often repeated in past time, until the crust has been so stiffened above, as well as below, that only feeble flexures of vast span are possible, even if the lateral pressure from contraction had not also declined in force.

4. *How was the lateral thrust from the direction of the ocean made to differ in its action or results from that from the opposite direction?*

The fact of a difference in the effects of the lateral thrust from the opposite directions, the oceanic and continental, is beyond question. The evidence may here be repeated.

The greatest of elevations, as well as subsidences, and also of plications and igneous eruptions, have taken place on the continental borders or in their vicinity; they thus show that there is something peculiar along such regions. Again, the border mountains in North America are parallel to the axes of the adjoining oceans, and thereby at right angles, instead of parallel, to one another. Again, the folds in the Appalachians are not symmetrical folds, but, instead, have one slope much steeper than the other, proving inequality in the action of lateral pressure from the continental and oceanic directions. Further, the larger ranges of uplifts and effects of heat occur on the *oceanic* slope of the principal border mountain-chain instead of the continental slope, favouring the view that this lateral thrust was more effective in the direction from the ocean against the continents than in the opposite. Finally, there is the fact that the disturbances or effects of lateral thrust have been *very much the greatest* on the border of the *largest* oceans.

But has this greater effectiveness of lateral thrust from the direction of the ocean been due to a proportionally greater contraction and subsidence of the oceanic crust than the continental? —the sinking causing the oceanic arch to press against the sides of the basin. I formerly made this the chief means of mountain-lifting; and now, while not giving it so great prominence, I believe it to be a true cause. It is certain that the depressing of the ocean's bed, like the raising of the continental areas, has been in progress through the ages. The great principal rise of the continent and continental mountains took place after the Cretaceous period or during the Tertiary, and some of it even in the Quaternary; and this is almost positive demonstration that the bottoms of the oceans were tending downward contemporaneously. It is not possible in the nature of contraction that it should have been all accomplished in these basins at the beginning of their existence—a point I shall further illustrate when discussing the nature of the earth's interior. Moreover the mobile waters that occupy the oceanic depressions would have given important aid in the cooling of the underlying crust. It is to be noted also that the distance between the axis of the Appalachians in North America and the opposite (African) side of the Atlantic is 4000 miles, and that between the axis of the Rocky Mountains and the opposite (Australian) coast of the Pacific is over 7000 miles; while between the axis of the Appalachians in Virginia and that of the Rocky Mountains in the same latitude the distance is hardly 1500 miles. Hence the contraction was absolutely greatest over the oceanic areas independently of any result from special causes; and if the generated pressure was not expended in uplifts over the oceanic areas themselves, it would be in uplifts on its borders.

In addition to the above advantage which the oceanic areas have had in the making of border oscillations, the lower position of the oceanic crust, and the abruptness with which the sides fall off, give it an opportunity to push beneath the sides of the continents; and this would determine the production of such mountains and just such other effects of pressure on the continental borders as actually exist, even if contraction were equable over the globe—that is, were alike in rate over the oceanic and continental areas. It puts the oscillations over the continents inevitably under the direction of the adjoining oceanic crust. The angle of slope of the deep-water sides of the oceanic basin is generally above five degrees*.

This conclusion is further sustained by the known universality of oscillations over the oceanic basin. The central Pacific area of coral islands ("registers of subsidence") stretches from the eastern Paumotus to the western Carolines, 90 degrees in longitude, and it indicates that the comparatively recent coral-island subsidence involved a region stretching over more than one fourth the circumference of the globe. The fact teaches that the movements of the globe, which have been in progress through all time in obedience to the irresistible energy generated by contraction, have been world-wide, and so world-developing, even down to the last era of geological history.

The above considerations sustain me in the opinion expressed in 1856 (Amer. Journ. Sci. 2nd ser. vol. xxii. p. 335), that the relation in size between the mountains and the bordering oceans is not merely "formal," as pronounced by my friend Professor LeConte, but has a *dynamical* significance.

In view of the considerations here presented, I believe there is no occasion to reject the fourth proposition, 4 (*a*) on page 42, but only to modify it as follows:—

4. (*a*) Owing to the general contraction of the globe, the greater size of the oceanic than the continental areas, and the greater subsidence from continued contraction over the former than over the latter, and also to the fact that the oceanic crust had the advantage of *leverage*, or, more strictly, of obliquely upward thrust against the borders of the continents because of its lower position, *therefore* along these borders, within 300 to 1000 miles of the coast, &c.

[To be continued.]

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* The angle of slope on the sides of the oceanic basin has not yet been properly investigated. The margin of the basin on the Atlantic border is now in about 100 fathoms water (600 feet). According to soundings by the Coast Survey, as I am informed by Mr. A. Lidenkohl of the Coast-Survey Office, through J. E. Hilgard, Esq., Assistant-in-Charge, the slope between 100 and 200 fathoms off Cape Hatteras is 2° 31′, off New York entrance 2° 02′, off George's Shoal 1° 35′. But for the region beyond 200 fathoms the data are not sufficient for any certain conclusion. Mr. Lidenkohl observes:—"If the soundings by Lieutenant Murray off Cape Look-out can be trusted, the slope between the 100- and 2000-fathom line must be over 7 degrees. Berryman's soundings off St. George's Bank indicate a slope of about 3½ degrees. From this it may be inferred that the slope rather increases than decreases beyond the 200-fathom line."

V. *On Thermodiffusion of Gases.*

By W. Feddersen, *of Leipzig*[*].

THEORETICAL considerations have opportunely been applied by Carl Neumann[†] to the proposition that if a limited portion of a gas enclosed in a tube of unlimited length (or one that returns into itself) be different in density from the rest of the gas, an artificially produced difference of temperature at the two ends of this portion must occasion a continuous motion throughout the endless cylinder of gas in a determined direction—and, indeed, from the cooler to the warmer end through the limited portion in question, if the gas in this be in the state of condensation.

By experiment I have answered, in a simple manner, to the requirements of the theory. Already in the beginning of this year (1872) I found that experiment confirmed that theory, in that without exception the expected direction of the motion could be recognized. My experiments, however, did not permit the deduction of definite laws—partly because they were in reality only qualitative, in order to prove the fact generally, and partly on account, as it appears, of manifold disturbing influences which were not further ascertained. I therefore thought I must defer their publication until I should have an opportunity of furnishing more exact data. Meanwhile the publication of the results of an investigation by M. Louis Dufour[‡] has induced me already, although I have only a few, properly speaking, qualitative results, to publish what I have hitherto discovered, because my observations are the converse of Dufour's and in harmony with them.

My experiments were performed in this manner:—A substance in the form of powder was stuffed tight into a glass tube so as to form therein an immovable plug. This tube was fixed in a horizontal position; and the two ends which projected on each side of the plug were each connected air-tight, by means of caoutchouc, with another horizontal glass tube, which was stopped by a drop of liquid at any place in its interior. In this manner, every displacement of the air cylinder contained in the middle portion of the tubes must displace the two drops of liquid at its extremities in the same direction. One end of the plug was now exposed to a constant source of heat, the other being left cold or artificially cooled. Then, without exception, there appeared a slow displacement of the air column in a direction through the plug, from the cold to the heated end, sometimes quicker, sometimes slower, sometimes stronger on one side and sometimes on the other.

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[*] Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlviii. pp. 302–311.

[†] *Berichte der Königl. Sächsischen Gesellschaft der Wissenschaften*, Feb. 15, 1872.

[‡] *Archives de Genève*, Sept. 1872, pp. 10, 11.

Spongy platinum was the first substance with which I experimented, in the expectation that, in consequence of its great absorptive capacity, especially for oxygen, with it rather than any other substance an action would be perceptible. Into a tube of about 3½ millims. diameter, a length of about 60 millims. of spongy platinum was pressed, which had been heated to redness two days previously. When I inserted a drop of mercury in the accessory tube, applied preliminarily to the heated side only, there was indeed a displacement backwards of the drop, but to a considerable degree with strong heating only. In presence of the minuteness of the pressure-force set free, the drop of mercury was evidently too heavy. I substituted for it sulphuric hydrate; and immediately, with the difference of temperatures 10° (that of the room) and 100°, the expected movement began in a striking manner. As an example, out of various experiments I will only mention that, in the eight hours period of observation, the drop once moved 195 millims. away from the heated end. Within this entire interval, however, the speed of the motion varied. In another instance, the platinum being fresh from incandescence, a brisker movement took place. Nevertheless the displacement of the drop was always slow; so that changes in the temperature of the apartment (of course not heated) may have exerted a sensible influence.

I therefore took a shorter tube about 12½ millims. in diameter, inserted therein a plug of recently incandescent spongy platinum of 31 millims. length, and placed at each end a long tube of glass 3½ millims. in diameter, stopped by a drop of sulphuric acid. One end of the plug was exposed to a temperature of about 200°*, while the other end was left to cool freely down to the temperature (8°) of the unwarmed room. On account of the great heat-conducting capacity of the platinum, this end was also not inconsiderably heated. The following were the ascertained displacements in the accessory tubes, all in the direction from the cool, through the plug, to the hotter side:—

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* Compare what is said further on, under "*Gypsum.*"

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	
12 0 to 12 10	225	225	tube wanting	millims.
12 10 „ 12 15	94	188	95	190
11 15 „ 12 25	tube wanting	……	241	241
12 25 „ 12 30	105	210	95	190
12 30 „ 12 35	110	220	tube wanting	

Accessory tubes removed, and, after an hour and a half, replaced.

2 0 to 2 5	73	146	49	98
2 5 „ 2 20	205	137	153	102
2 20 „ 2 30	151	151	112	112
2 30 „ 2 35	76	152	58	116

After the last observation the bent end of the tube on the cold side was immersed in sulphuric acid. The displacements ceased as soon as the acid had risen about 6 millims. in the tube, in which position it remained until, at the end of an hour and a half, I dismounted the apparatus. The capillary rising of the acid in the tube did not amount to one millimetre; so that the difference of pressure produced by the difference of temperature on the two sides of the plug corresponded to a column of sulphuric hydrate of at least 5 millims. height.

Spongy palladium.—In most of my experiments with this I introduced it into an atmosphere of oxygen; but the results obtained were the least regular of any. I had contrived a special arrangement of straight and T-tubes to bring oxygen into the plug-tube as well as into the accessory pieces. In the experiments a movement always showed itself in the expected direction, but with extraordinary variability. The singular irregularities appeared to be provoked chiefly by the variability of the absorption by the palladium with oscillations of temperature; but probably the aqueous vapour, which always formed when the atmospheric air was expelled from the tube by oxygen, was a further cause of the irregularities. Nevertheless the direction of the phenomenon was by all this not reversed.

As an example of the movement the following numbers of one observation may serve: the tubes had from 3½ to 4 millims. internal diameter, and on both sides were stopped by a drop of oil.

Time.	Cold side.	Hot side.
h m h m	millims.	millims.
9 5 to 9 25	130	. . .
9 25 „ 9 35	155	190
9 45 „ 9 50	45	Aperture touching oil surface raises slight bubbles.

In this example, therefore, between 9^{h} 25^{m} and 9^{h} 35^{m} the oil drop on the hot side removed 190 millims. from the heated part; while that on the cold side moved 155 millims. nearer to the cold end of the palladium.

Gypsum.—In a tube of 12½ millims. internal diameter was a plug of gypsum 70 millims. in length, which, after its pouring-in, had remained some time and become air-dry. The glass tube was encased in sheet copper over one end of the plug; and by a feebly burning spirit-lamp placed beneath, that part was heated to about 200°[*], while it was protected from draughts as much as possible by pasteboard walls. The other end of the plug was exposed to the temperature of the room (8°), and manifested, when touched, a very slight warming. On stopping the accessory tubes, 3½ millims. in diameter, with mercury, no movement made its appearance even after fifteen hours. When afterwards sulphuric hydrate was taken for the liquid stop, I obtained the following displacements in the direction from the cold to the hot side:—

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
10 15 to 10 45	tube wanting.	......	60	20·0
10 45 „ 11 30	„ „	......	87	19·3
11 30 „ 12 15	52	11·5	45	10·0
12 15 „ 1 15	52	8·7	32	5·3
1 30 „ 2 0	20	6·7	23	7·7

The aqueous vapour developed from the gypsum, at first in large quantity, and at last only sparingly, did not disturb the direction of the phenomenon.

Charcoal.—Instead of gypsum the tube had, under otherwise like conditions, a plug, 90 millims. in length, of coarsely powdered and recently heated fir charcoal. After the setting-up the drop of sulphuric acid did not at first move at all on the heated side, while on the cold side it moved briskly toward the charcoal.

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[*] The temperature was measured, as the bulb of a thermometer rested on the copper sheath; above, it was somewhat protected from radiation by a loose-lying piece of pasteboard. As is seen, the determination is very closely approximative. The temperature, however, was preserved pretty constant.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
P.M. 2 50 to 3 40	250	50	20	4·0
3 40 „ 4 20	tube wanting.	……	125	31·2
4 25 „ 4 45	200	100	65	32·5
5 0 „ 5 30	200	67·7	30	10·0
5 30 „ 6 10	130	32·5	50	12·5
6 30 „ 7 0	220	73·3	60	20·0
7 0 „ 7 15	73	48·7	23	15·3

During the night the lamp went out, and cooling took place.

A.M. 6 50 to 7 50	285	47·5	tube wanting.	
8 0 „ 9 9	265	44·2	immovable.	
9 0 „ 12 0	tube wanting.	……	415	23·1
12 0 „ 1 0	225	37·5	10	1·7*
1 0 „ 1 30	40	13·3	immovable.	

We see here that in every instance the motion observed in the drop followed the direction through the plug from the cold to the hot end. From the fact that the drop on the cold side always moved quicker than that on the hot side, it must be concluded that, besides the phenomenon of gas-displacement, there was also a special absorption with the charcoal, greater than that which appeared in like manner to take place with the spongy platinum.

Silicic acid.—The arrangement of the experiment was the same as with the two last-named substances. The length of the plug (of silicic acid in powder fresh from incandescence) in the tube of 12½ millims. diameter amounted to 110 millims. After the action of the difference of the temperatures (about 200° and 8°) had lasted continuously some 3½ hours, the following observations were made:—

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
P.M. 5 10 to 6 0	65	13·0	60	12·0
6 10 „ 6 30	90	45·0	75	37·5
6 30 „ 7 10	125	31·3	140	35·0
7 10 „ 7 30	56	28·0	59	29·5
7 30 „ 8 0	64	21·3	75	25·0

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* From 12 o'clock the *end* of the tube on the hot side was closed by a drop; the difference of pressure produced by the process was not sufficient to overcome the capillary attraction at that place.

The accessory tubes were taken away at night; yet the difference of temperatures was maintained.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
A.M. 6 40 to 7 10	103	34·3	116	38·7
7 10 „ 7 40	75	25·0	80	26·7
7 40 „ 8 10	74	24·7	82	27·3
8 10 „ 8 40	50	16·7	59	19·7
8 40 „ 9 10	tube wanting.	......	83	27·7
9 10 „ 9 40	„ „	......	76	25·3

Of absorption, such as took place with charcoal and spongy platinum, there is here no trace perceptible.

Calcined magnesia.—The experiment was made with freshly heated magnesia under the same conditions as the last-mentioned substances. In the tube of about 12½ millims. diameter the *length of the plug* was at first 100 millims.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minntes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
5 30 to 5 40	145	145	tube wanting.	
5 40 „ 5 50	140	140	„ „	
5 50 „ 6 0	132	132	95	95
6 0 „ 6 15	214	143	135	90
6 15 „ 6 30	tube wanting.	......	134	89
6 30 „ 6 35	„ „	......	43	86
6 35 „ 6 50	205	137	154	103
6 50 „ 7 0	142	142	tube wanting.	

For the following experiments a part of the magnesia was extracted from the tube, in consequence of which the *length of the plug* was reduced to 40 millims.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
7 30 to 7 35	142	284	114	228*
7 35 „ 7 40	140	280	106	212

---

* In support of the quantity of gas supposed to have passed through the cross section of the plug in the unit of time, let it be remarked that a cylinder of about 13 millims. length in the accessory tubes represents a volume equal to that of a cylinder of 1 millim. length in the diaphragm-tube.

After this, the bent end of the tube on the cold side was immersed in sulphuric hydrate. The sulphuric acid rose quickly to 53 millims. height, while the drop received a displacement corresponding to the dimensions of the closed spaces. The position remained unchanged during the first hour; but after the lapse of the second hour the acid had risen to 62 millims., and after another 1½ hour to about 80 millims., where the bend of the tube commenced; and so any further diminution of pressure in the tube could not be observed. Sustained blowing with the mouth into the open end on the warm side very soon reduced (as was to be expected) the column to zero.

The numbers observed with magnesia, again, point to an absorption of the gas by the substance in the form of powder.

From the preceding experiments with the most heterogeneous substances it appears to follow that it is a universal property of porous bodies, when in the form of diaphragms, to draw gases through them in the direction from the cold to the hot side. We have thus a phenomenon of diffusion which, contrary to ordinary diffusion, occurs even when the same gas under the same pressure is found on both sides of the diaphragm. This is a singular, hitherto unknown phenomenon; and we are therefore justified in giving it the name of

Thermodiffusion.

Whether this kind of diffusion depends also on the nature of the gas, so that with mixed gases a selective diffusion might take place (that is, a mechanical separation of the gases), analogous to that which occurs in the diffusion of liquids, I have not yet been able to discover; it appears at all events probable when Dufour's experiments are taken into consideration. Further, it may be an argument in favour of this view, that I often observed a progressive diminution in the velocity of the motion in the enclosed space, starting from the attachment of the tubes, such as must necessarily occur if, on the cooler side, one of the constituents of atmospheric air diffuses through to the warmer end more quickly than the other; for the closed space on the former side would be sooner exhausted of the more easily diffusing gas than of the other.

Dufour, *l. c.*, states that when gases are caused to diffuse, there is a *rise of temperature* on the side where the more quickly diffusing gas enters the porous diaphragm, and a *fall of temperature* on the opposite side; so that in his experiments diffusion produces a change of temperature; in mine a change of temperature causes diffusion, and the latter in a direction such that the artificially produced difference of temperatures, if we apply the laws discovered by Dufour, is diminished by the process of diffusion itself. Accordingly we find between thermodiffusion and Dufour's discovery a reciprocity analogous to that between heat and electricity in the ordinary thermo-current and Peltier's phenomenon.

If the forces set free by thermodiffusion appear mostly to be trifling, yet it cannot be forthwith maintained that this phenomenon plays only a quite subordinate part in the economy of nature; for the conditions of its occurrence may at least be widely spread.

Davos, 26th December, 1872.

VI. *Determination of Degrees of Heat in Absolute Measure.*

By L. Lorenz, *of Copenhagen*[*].

ONE of the most important means of modern times, for elucidating the connexion between various forces independently of all physical hypotheses, is the determination of the magnitudes dependent on these forces by the same absolute units. While the system of absolute measures has been carried out in the sciences of magnetism and of electricity, the *degree of heat* has only been determined arbitrarily; and thus, so to speak, the thread has been broken which connects heat with the other physical forces. The object of the present investigation is to establish a definition of the absolute degree of heat in a purely empirical manner, and by introducing it into science to illustrate more clearly the relation in which heat and electricity stand to each other.

The absolute units introduced by Gauss and Weber, which we shall use in the sequel, are the *millimetre* as *unit of length*, the *second* as *unit of time*, and *the milligramme* as *unit of mass*. By means of these units the *electromagnetic* unit of *current-intensity* is defined as being the intensity of that current which, traversing the unit of surface, acts upon a magnetic pole as an infinitely small magnet whose moment is unity. Weber has further defined the unit of the *quantity of electricity* as being that quantity of positive electricity which in the unit of time moves, in the direction of positive electricity, in a current whose intensity is unity, it being presumed that the same quantity of negative electricity is simultaneously transferred in the opposite direction. In what follows we shall, in common with many other authors, define the unit of electricity as being the *sum* of the quantities of positive and negative electricity which, moving in opposite directions through a circuit, produce the unit intensity of current.

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[*] Translated from Poggendorff's *Annalen*, No. 11, 1872.

The absolute *unit of heat* is defined as being the quantity of heat equivalent to the absolute unit of work. If, now, the *degree of heat* be defined as the increase of temperature which produces an absolute unit of heat in being imparted to the unit of mass of water, the degree of heat is arbitrarily determined, since it depends on the physical nature of the substance chosen, namely *water*. If instead of a certain quantity of water we select a certain number of *atoms of an element*, then, in accordance with Dulong and Petit's law, the heating which a given quantity of heat produces in these atoms is independent of the nature of the substance, and it only remains to settle more precisely the number of atoms to be chosen.

The law in question does not hold quite accurately with reference to the *solid* elements; yet the deviations are explained by the fact that heat is not only used for heating, but also for performing internal work. On the contrary, the law certainly holds for all those *gases* in reference to which it can be assumed that none of the heat is consumed in internal work. The loss of heat in external work can be avoided by heating the air under *constant* volume.

According to Regnault, we have for *constant pressure* the specific heats

Nitrogen.	Oxygen.	Hydrogen.
0·24380	0·21751	3·40900

Hence, to heat under constant pressure 14 mgrms. nitrogen, 16 mgrms. oxygen, 1 mgrm. hydrogen, there are required

3·41320 3·48016 3·40900

relative units of heat (1 mgrm. water through 1° C.). These three numbers, of which especially the first and last are very near each other, show, in accordance with Dulong and Petit's law, that the same quantity of heat is required to raise the same volume at the same pressure, and therefore also, as we assume, the same number of atoms of the gases in question, through one degree.

Under *constant volume* the specific heat of these gases is in the ratio 1·40 : 1 less (1·41 on the old, and 1·3945 from Regnault's determinations of the velocity of sound in air); if of the above three numbers we take the mean value of the two which most closely agree with each other (for nitrogen and hydrogen)—that is,

3·4111,

we obtain 2·436 thermal units (1 mgrm. of water through 1° C.) as the quantity of heat required for heating under constant volume as many atoms of a permanent gas as are contained in 1 mgrm. of hydrogen.

The relative unit of heat used here may be readily expressed in absolute units of work; defined in this measure we will express it by A. The unit of heat is equivalent to a work of 433 milligrammetres; and as the weight of a milligramme is 9806 absolute units (that is, the acceleration of gravity expressed in millimetres), we have

$$A = 425 \times 10^7 \text{ absolute units.}$$

But to heat 1 mgrm. hydrogen at constant volume through 1° C.,

$$2{\cdot}436 A = 1035 \times 10^7 \text{ absolute units are necessary.}$$

Just as a definite quantity of heat is necessary to raise the same number of atoms of various elements through one degree, so, according to Faraday's *electrolytical law*, equal quantities of electricity are required to separate equivalent quantities from an electrolyte. But as equivalent quantities do not always correspond to the same number of atoms, it is necessary to choose a definite type for electrolysis.

As such a type I prefer the electrolysis of bodies constructed on the formula RCl (Br, S)—partly because an equal number of atoms of the element are separated at each electrode, and partly because we have here the greatest number of atoms which can be separated from an electrolyte by the same quantity of electricity. All deviations from the type taken must then be regarded as arising from secondary actions of the chemical forces. Thus, while we regard the electrolysis of strong hydrochloric acid as normal, the decomposition of water must be regarded as a deviation, which may perhaps be explained by assuming that two atoms of oxygen unite to form a double atom.

In the unit of time an electrical current of unit intensity liberates in a voltameter $\frac{1}{960}$ mgrm. hydrogen*. The same current will liberate from strong hydrochloric acid the same weight of hydrogen and as many atoms of chlorine—that is, will separate at both electrodes as many atoms as are contained in $\frac{1}{480}$ mgrm. hydrogen. To raise the same number of atoms through 1° C. under constant volume, we require, in accordance with what has been said above,

$$\frac{2{\cdot}436}{480} A = 0{\cdot}005075 A = 216 \times 10^5 \text{ absolute units.}$$

We may now define a *degree of heat* in absolute measure as that *increase of temperature* which *the unit of work produces*, in being completely and exclusively changed into heat, *in the same number of atoms of the element which the unit of electricity liberates from an electrolyte under normal circumstances.*

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* Conf. Wiedemann, *Die Lehre vom Galvanismus*, second part, p. 917.

From what has been said, this increase of temperature is

$$\frac{1}{216\times 10^5} \text{ of a Centigrade degree;}$$

and in accordance with the definition given,

$$1 \text{ degree Centigrade } = 0\cdot005075\,\mathrm{A} = 216\times 10^5 \text{ absolute units.}$$

Besides the connexion between heat and electricity which is expressed by Dulong and Petit's law, and by Faraday's electrolytical law, and which we have used in establishing a definition of the absolute degree of heat, there is another connexion, which found its first expression in Wiedemann and Franz's law, that the conductivity of the various metals for heat and for electricity are in the same ratio. Subsequent investigations have shown, however, that this ratio changes with the temperature, and therefore that the law in its original form cannot be completely valid, but requires a modification.

The influence of heat upon *electrical* conductivity has been investigated by several physicists, as Lenz, Becquerel, Arendtsen, and especially by Matthiessen and von Bose*. These physicists investigated the conductivity of ten different *pure* metals, namely silver, copper, gold, zinc, cadmium, tin, lead, arsenic, antimony, and bismuth. From their investigations the remarkable result was obtained, that the diminution of electrical conductivity for an increase from 0° to 100° C. is the same for all these metals, namely 29·307 per cent. The *resistance* increases, therefore, for the same increase of temperature by 41·46 per cent.—that is, in a somewhat greater ratio than the increase of temperature (36·6 per cent.) if we calculate the temperature from the absolute zero (−273° C.). Matthiessen and Vogt† found subsequently that among the pure metals iron forms an exception, inasmuch as its conductivity may decrease by more than 38 per cent.

But few experiments have been made as to the influence of temperature on conductivity for *heat*. It must, however, be remarked that all the older experiments on thermal conduction agree with the former assumption, that the conductivity is independent of the temperature. Ångström‡ found with reference to two bars of copper, which, however, were probably not quite pure, that between 0° and 100° there was a decrease of conducting-power of from 15 to 21 per cent., and for iron of 28·7 per cent.; while Forbes§ found for bar iron a decrease of 15·7 and 23·3 per cent.

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* Phil. Mag. S. 4. vol. xxiv. p. 405.

† Phil. Mag. S. 4. vol. xxvi. p. 542.

‡ *Œfvers. af K. Vetensk. Förhandl.* 1862. Pogg. *Ann.* vol. cxviii. p. 423.

§ Trans. Edinb. Royal Soc. 1862–64.

If we now observe that electrical conductivity for the various pure metals is nearly inversely proportional to the temperature calculated from absolute zero, while their thermal conductivity approaches more to constancy, and further that in the case of iron the deviations for both kinds of conductivity are in the same direction, then we may say that the preceding facts establish, with as great closeness as can be expected, the law that *the conducting-power of a pure metal for heat and for electricity is proportional to the temperature* calculated from absolute zero.

This ratio must obviously more or less vary in several cases. If, for instance, the metal is not homogeneous, or contains admixtures of foreign metals, and, generally speaking, in cases in which, owing to unequal heating in the interior, *thermo-electric currents* may be formed, then the thermal conducting-power will probably be increased, or at all events the relation between the two kinds of conductivity will be altered. This is doubtless the case when heat can be transmitted as *radiant heat* in the interior of bodies. In this kind of transmission we must seek the reason why thermal conductivity for all transparent and translucent media, and generally for all non-metallic bodies, is apparently far greater than that which would correspond to their electrical conductivity. Lastly, if the body is liquid, the ratio must alter, owing to the *mobility of the parts.* If, for instance, a column of liquid is heated from below, this mobility will, of course, increase the observed conductivity. If it is heated from above, currents in the interior cannot be entirely avoided; for every part of the liquid in the same horizontal section cannot have quite the same temperature; the colder parts will then sink, and the warmer ones rise toward the source of heat; and the conducting-power must therefore be *diminished* in consequence of the *motion* of the parts.

We must hold, then, that the law, if it is at all valid, is probably only absolutely so for *pure homogeneous* and *solid metals.* In fact even an unequal heating will make the metals heterogeneous and will give rise to thermo-electrical currents.

From the foregoing observations I shall now attempt to deduce the ratio between the conducting-power of metals for heat and for electricity in absolute units. We shall obtain from this the remarkable result, *that for a pure homogeneous solid metal this ratio is equal to the temperature calculated from the absolute zero expressed in the above-defined absolute units.*

To determine thermal conductivity in absolute measure, we must know how great is the quantity of heat which traverses each unit of surface of a plate of given thickness and with a given difference of temperature at the two sides. The older experiments on this subject have, for very intelligible reasons, led to results which are not concordant and which are far too low; hence we can only use the experiments of Ångström, Forbes, and Neumann, which agree pretty closely with each other, although these observers made their experiments independently of each other and in very different manners. We will for the present use the units chosen by Ångström, the centimetre, the minute, the Centigrade scale, and as thermal unit 1 grm. of water 1° C.

Ångström* found for the thermal conductivity of

Copper	58·94 at 0° C.
Copper	51·63 „
Iron	11·927 „
Mercury	1·061 „

The last determination was made with a mercury column enclosed in a glass tube, which was heated from the top.

Forbes found, in the above-mentioned experiments, for

Iron	12·36 at 0° C.
Iron	12·42 „
Iron	9·21 „

Neumann† determined the thermal conductivity of five different metal bars, and at the same time made a comparative determination of their relative electrical conductivity. While this was taken at 100 for silver, that of copper was assumed to be equal to 73·3. These results were, with the above units:—

	Thermal conductivity.	Electrical conductivity.	q.
Copper . . .	66·48	73·3	0·907
Brass . . .	18·12	17·9	1·012
Zinc	18·43	21·1	0·873
German silver .	6·566	6·45	1·018
Iron	9·824	10·20	0·963

The ratio between the thermal and the electrical conductivity (which is designated by q) is greatest for brass and German silver. This is not accidental, but probably a consequence of the fact that they are not pure metals. In like manner we obtain, in agreement with what has been said above, a discordant and obviously too low value for mercury; we find for this metal, from Ångström's experiments, $q=0·617$, taking as the electrical conductivity of mercury 1·72.

The mean value of the quotients q for copper, zinc, and iron is 0·914 according to Neumann's experiments. Whether, in accordance with other experiments, this number should be made greater or less is difficult to decide; but as the thermal conductivity in Neumann's experiments was not reduced to 0°, q must be taken as a little less for this reason.

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* Pogg. *Ann.* vol. cxviii. p. 423, and vol. cxxiii. p. 628.

† *Ann. de Chim. et de Phys.* 1862, vol. lxvi. p. 183.

Hence, then, we may take

$$q=0\cdot 90 \text{ at } 0^\circ \text{ C.}$$

as the result which may be deduced from the present experiments with the greatest degree of probability.

The value of q thus determined is, in Ångström's units, the thermal conductivity of a metal whose electrical conductivity is 1, that of silver being taken as equal to 100. Through each square millimetre of a surface with the thermal conductivity q, whose thickness is 1 millim., there pass in each second

$$q\frac{1}{100}\times 10\times\frac{1}{60}=\frac{q}{600}$$

relative units of heat (1 grm. water 1° C.), with a difference in temperature of 1° C. at the two sides. As the thermal unit used here is equal to 1000 A, and as we have found that 1° C. expressed in absolute units is equal to 0·005075 A, the absolute thermal conductivity corresponding to q, which we will call κ, is defined by

$$\kappa_1=\frac{q}{600}\times\frac{1000\text{ A}}{0\cdot 005075\text{ A}}=328\cdot 4\,q.$$

From this we see that the factor by which the thermal conductivity is reduced from Ångström's units to absolute measure is independent of A.

With the value we have assumed above for q,

$$\kappa_1=296.$$

If we designate the corresponding absolute electrical conductivity by k_1, we should have, in accordance with the above law,

$$\frac{\kappa_1}{k_1}=\text{T},$$

where T is the temperature calculated from the absolute zero. For the freezing-point of water T is equal to 273 × 1° C., and, if a degree Centigrade is expressed in absolute units,

$$\text{T}=1\cdot 385\text{ A}=589\times 10^7,$$

from which

$$\frac{1}{\kappa}=0\cdot 00468\text{ A}=1\cdot 99\times 10^7.$$

If, now, we wish to calculate the absolute resistance of a Siemens's unit (a column of mercury a metre in length and a square millimetre in section at 0° C.), we must know the ratio between the specific conductivities of silver and mercury. This ratio alters materially with the physical condition of silver; even if, as is most usual, the silver be used in the form of hard-drawn wire, no great accuracy is attainable in the determination of the ratio. In Wiedemann's *Lehre von Galvanismus* (Part I. page 181) we find the numbers 1·739 (E. Becquerel), 1·7 (Lamy), 1·63 (Matthiessen) for the conductivity of mercury, that of silver being taken as 100. Matthiessen subsequently* gave the number 1·65; and Siemens's determinations† gave 1·72 and 1·78. These numbers hold for 0° C.

We may thus take the conductivity of mercury at 0° C. as approximately equal to 1·72; so that, in accordance with the above calculation, the resistance in absolute measure would be

$$\frac{1}{1\cdot72k}=0\cdot00272\ A=1\cdot16\times10^{7}.$$

Hence Siemens's unit of resistance denoted by S is, in absolute measure,

$$S=2\cdot72\ A=1\cdot16\times10^{10}.$$

We will compare this result with the direct absolute measurements of Siemens's unit of resistance, which have been made partly with the aid of induced currents, and partly by measuring the quantity of heat developed in a conductor by a constant current.

By the first method Weber‡ found

$$S=1\cdot0257\times10^{10},$$

while the Committee of the British Association§ found as a mean

$$S=0\cdot964\times10^{10}.$$

A small correction introduced by Matthiessen, by which both values are reduced 0·3 per cent., is in the present case without significance.

These determinations do not, therefore, materially deviate from the values of S calculated from the thermal conductivity; they are both, however, somewhat lower. The reason for this difference might be sought in the less accurate determination of the thermal conductivity as compared with the electrical conductivity; I think, however, that the reason for this discrepancy is to be sought elsewhere.

The *great* difference, considering the accuracy with which the experiments were made, between Weber's results and those of the British Association Committee, a difference which actually amounts to 8 per cent., points to errors which cannot be ascribed to accidental faults of observation, but must rather be ascribed to an imperfect theory. It is here to be observed that the experiments were made with *induction-currents of varying intensity*; yet we may at present undoubtedly state, both from theoretical and experimental investigations, that we know only the elements of the theory of variable induction-currents, and that the results can only be regarded as a first approximation. Hence, notwithstanding the great care with which these measurements have been made, we must not ascribe too great a weight to them.

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* Phil. Mag. S. 4. vol. xxii. p. 195.

† Phil. Mag. S. 4. vol. xxi. p. 25.

‡ *Abh. d. k. Gesellsch. d. Wissensch. Göttingen*, 1862.

§ British Association Report, 1863, Jenkin.

The determination of the electrical resistance by means of the *disengagement of heat* which a *constant* current produces in a conductor is theoretically far simpler and more certain than the induction method *in the manner in which this has hitherto been applied*. Fortunately we possess a large series of experiments carefully executed and calculated by Quintus Icilius*. This physicist determined the heat which a given intensity of current disengages in a second in various copper and platinum wires, the electrical resistance of which was determined by means of a standard measured by Weber in absolute units. If V denotes the number of relative units of heat (1 mgrm. water 1° C.) which are developed in a second in a Siemens's unit of resistance by the current-intensity S, we obtain by these experiments a determination of the constant α in the equation

$$V=\alpha s^2\times 1{\cdot}0257\times 10^{10},$$

if with Quintus Icilius we use Weber's determination of electrical resistance; while with the former signification of A as the absolute equivalent of work for the relative unit of heat (1 mgrm. water 1° C.), and of s as the absolute value of Siemens's unit of resistance, we have

$$AV=s^2S.$$

From the two equations we have

$$S=\alpha A\times 1{\cdot}0257\times 10^{10}.$$

In the experiments in question three several liquids were used in the calorimeter, namely water, alcohol, and oil of turpentine. The former liquid had the advantage over the two others, that it gave the quantity of heat directly in the units chosen; on the other hand, a small error cannot be avoided, owing to the greater conductivity of water for electricity, by which the disengagement of heat, and therewith also the constant α, must be found rather too small. Owing to its volatility, the experiments with alcohol showed such small agreement among each other that they must be left out of consideration.

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* Pogg. *Ann.* vol. ci. p. 69.

As the average of twenty-eight experiments with *water,* we obtain

$$\alpha = 2{\cdot}543 \times 10^{-10};$$

and of ten experiments with oil of turpentine,

$$\alpha = 2{\cdot}652 \times 10^{-10}.$$

There is no greater difference between these two values of α than might have been expected from the greater conductivity of water; hence the latter number must be taken as that which follows with the greatest probability from Quintus Icilius's experiments. With this value of α we obtain

$$S = 2{\cdot}720A = 1{\cdot}16 \times 10^{10};$$

that is, exactly the same value for Siemens's unit of resistance in absolute measure which we have deduced above from the thermal conductivity of the metals. That it is *exactly* the same value must, of course, be regarded as accidental.

In another way also we obtain a confirmation of the accuracy of the law here propounded, since we shall find that in this law there is the closest agreement between the laws for the propagation of energy in metals, no matter whether this transmission is effected by the motion of heat or of electricity.

By *energy* we understand any magnitude which can be measured by units of *work.* We will consider only the propagation of heat and of electricity so far as it is effected in both cases by *conduction;* so that we may waive any considerations as to the propagation of heat in the interior of a body by radiation and by thermoelectric currents, just as in the case of electricity we neglect the propagation by induction and by thermoelectrical currents.

If by Q we denote the energy present in a body in the unit of volume, the increase $\frac{dQ}{dt}dt$, which Q experiences by *thermal conduction* in the form of heat in the element of time dt, is expressed by

$$\frac{dQ}{dt} = \frac{d}{dx}\kappa\frac{dT}{dx} + \frac{d}{dy}\kappa\frac{dT}{dy} + \frac{d}{dz}\kappa\frac{dT}{dz}, \quad . \quad . \quad (1)$$

where T is the temperature and κ the thermal conductivity, which may in general be regarded as a function of the temperature.

If we substitute in this, in accordance with the law propounded above,

$$\kappa = kT,$$

where k is the electrical conductivity, we obtain

$$2\frac{dQ}{dt}=\frac{d}{dx}k\frac{dT^2}{dx}+\frac{d}{dy}k\frac{dT^2}{dy}+\frac{d}{dz}k\frac{dT^2}{dz}, \quad . \quad . \quad (2)$$

in which equation all the magnitudes may be considered to be expressed in absolute units.

As the increase of energy here only occurs in the form of heat, it stands in a known relation to the increase of temperature, dependent on the specific gravity and the specific heat; and the equation gives therefore completely the law for the propagation of heat by conduction.

If in any point x, y, z of a body the components of the intensity of the current are u, v, w, and if k is the electrical conductivity, the quantity of heat received by the element of volume $dx\,dy\,dz$ in the element of time dt is, according to Joule's law,

$$\frac{u^2+v^2+w^2}{k}\,dx\,dy\,dz\,dt.$$

If this element of volume contains at the same time the quantity of electricity $\epsilon\,dx\,dy\,dz$, and if the electrical tension (potential) there is P, the element acquires at the same time the energy

$$P\frac{d\epsilon}{dt}\,dx\,dy\,dz\,dt$$

in the form of electricity. If, then, as before, Q denotes the energy present in the unit of volume, the increase arising from the motion of electricity, and which occurs both as heat and as electricity, is

$$\frac{dQ}{dt}=P\frac{d\epsilon}{dt}+\frac{u^2+v^2+w^2}{k}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In like manner, if we disregard the electricity which results from induction, from Ohm's law we have

$$u=-k\frac{dP}{dx}, \quad v=-k\frac{dP}{dy}, \quad w=-k\frac{dP}{dz}, \quad . \quad . \quad (4)$$

in addition to which we have Kirchhoff's equation

$$\frac{d\epsilon}{dt}=-\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Thus

$$\frac{dQ}{dt}=-P\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)=-\left(u\frac{dP}{dx}+v\frac{dP}{dy}+w\frac{dP}{dz}\right),$$

from which there follows

$$\frac{dQ}{dt}=-\left(\frac{duP}{dx}+\frac{dvP}{dy}+\frac{dwP}{dz}\right). \quad . \quad . \quad . \quad (6)$$

If, owing to the transference of energy, electromotor forces are formed in the interior (thermoelectricity), for no element in which they arise does either Joule's or Ohm's law hold. Yet even in this case the latter equation seems to retain its validity; for it agrees with the observation that a constant current which passes through one section of a conductor from a less to a greater tension produces an absorption of heat which is proportional to the intensity of the current and to the increase of tension.

Yet in this case we disregard any possible thermoelectric currents in the interior of the body, and we obtain from the last equation, by means of equations (4),

$$2\frac{dQ}{dt}=\frac{d}{dx}k\frac{dP^2}{dx}+\frac{d}{dy}k\frac{dP^2}{dy}+\frac{d}{dz}k\frac{dP^2}{dz}. \quad . \quad . \quad (7)$$

Comparing this equation with equation (2), we see that the laws for the propagation of electricity by electrical conduction and by thermal conduction have quite the same form; the positive or negative electrical tension and the absolute temperature calculated from absolute zero will correspond to each other, and, if we choose the absolute measure for the degree Centigrade which has here been proposed, may be measured with the same units. In accordance with these equations a body will receive in each element of its volume the same increase in energy, whether it be unelectrical and have in various places a different absolute temperature T, or whether it be uniformly warmed and have an electrical tension $\pm$P whose numerical value in each point is equal to T. It is, however, here presupposed that *k* has in both cases invariably the same value, which is only approximately true. In the next moment the ratio will be materially altered, since the increase of energy in the electrical body occurs in the form of heat, not of electrical tension.

Hence the law for the propagation of electricity cannot be determined by equation (7), which can only serve to define the increase of energy, whereas, as we have seen, the laws for the propagation of heat are defined by equation (2). If electricity continuously moves without change through a body (and this is the only case we can here deal with, as we do not take induced currents into consideration), the quantity of electricity is in each moment the same, and the equation then becomes

$$0=\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}.$$

This equation, combined with equations (4), gives

$$\frac{d}{dx}k\frac{dP}{dx}+\frac{d}{dy}k\frac{dP}{dy}+\frac{d}{dz}k\frac{dP}{dz}=0. \quad . \quad . \quad . \quad (8)$$

The electrical tension will therefore have to be [illegible] from this equation in combination with the given limiting conditions.

The motion of electricity is here looked on as permanent; that this may completely be the case, the heat developed (which in this case represents the total increase in energy) must be conducted away. In order now to develop further the analogy between electrical tension and temperature, we will *imagine* the corresponding suppositions applied to the motion of heat in a body; that these suppositions cannot in fact be fulfilled may in this case be disregarded.

We consider a body in which the motion of heat is kept permanent, and in which the entire energy which heat can produce in the form of work in passing from a higher to a lower temperature is conducted away from every point of the body.

If, now, the quantity of heat W at the absolute temperature T is added to a body in every second, and simultaneously and continuously therewith the quantity of heat W_1 at a lower temperature T_1 passes away by conduction, the condition remains unchanged if the entire difference $W-W_1$ is converted into work; and, in accordance with the mechanical theory of heat, the entire amount of work which the quantity of heat W can produce in the change of temperature T to T_1 is obtained if we have

$$\frac{W}{T}=\frac{W_1}{T_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

If we put for the three coincident surfaces of an infinitely small rectangular parallelepiped

$$\frac{W}{T}=\xi\, dy\, dz+\eta\, dx\, dz+\zeta\, dx\, dy,$$

we have for the other three surfaces

$$\frac{W_1}{T_1}=\left(\xi+\frac{d\xi}{dx}dx\right)dy\, dz+\left(\eta+\frac{d\eta}{dy}\right)dx\, dz+\left(\zeta+\frac{d\zeta}{dz}\right)dx\, dy;$$

and equation (9) will then give

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}=0. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In this case $T\xi\, dy\, dz$ is the quantity of heat transmitted through the surface $dy\, dz$ in the unit of time; but this is also defined by $-\kappa\frac{dT}{dx}dy\, dz$ if κ is the thermal conductivity; then

$$\xi=-\frac{\kappa}{T}\times\frac{dT}{dx}=-k\frac{dT}{dx};$$

and in the same manner

$$\eta=-k\frac{dT}{dy},\quad \zeta=-k\frac{dT}{dz}.$$

In combination with equation (10) these equations give

$$\frac{dk\frac{dT}{dx}}{dx}+\frac{dk\frac{dT}{dy}}{dy}+\frac{dk\frac{dT}{dz}}{dz}=0. \quad . \quad . \quad . \quad (11)$$

We see, therefore, that in the motion of heat which we have here imagined, in which the suppositions are quite analogous to the conditions which actually obtain for the permanent motion of electricity, the temperature would be determinable by the same differential equation as the electrical tension.

By the permanent motion of electricity through a body heat results, which ultimately acquires a permanent motion when it is continuously conducted away in the same manner. The increase of energy which owes its origin *both* to the motion of electricity *and* to that of heat is now *nil* in each element of the body; and from the equations (2) and (7) we shall obtain

$$\frac{d}{dx}\,k\,\frac{d(P^2+T^2)}{dx}+\frac{d}{dy}\,k\,\frac{d(P^2+T^2)}{dy}+\frac{d}{dz}\,k\,\frac{d(P^2+T^2)}{dz}=0. \quad (12)$$

By this equation, in combination with equation (8), both the electrical tension P and the absolute temperature T are to be determined when both the motion of electricity and that of heat are become permanent.

If, for instance, we pass electricity through a body, keeping in a small part σ_0 of its surface a constant electrical tension P_0, and in another part σ_1 of its surface the tension P_1, and if at the same time we maintain both surfaces at the same constant temperature T_0, while the other part of its surface is kept surrounded by perfectly bad conductors of heat and electricity, a permanent motion of electricity and of heat will ultimately result, and the electricity will develop the same heat as is conducted away by the surfaces σ_0 and σ_1.

If we put

$$P^2+T^2+AP+B=\phi, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

in which A and B are two arbitrary constants, we shall obtain by the equations (12) and (8),

$$\frac{d}{dx}\,k\,\frac{d\phi}{dx}+\frac{d}{dy}\,k\,\frac{d\phi}{dy}+\frac{d}{dz}\,k\,\frac{d\phi}{dz}=0. \quad . \quad . \quad (14)$$

Both constants A and B will then be so determined that for both surfaces σ_0 and σ_1 $\phi=0$, by putting

$$P_0^2+T_0^2+AP_0+B=0,$$

$$P_1^2+T_0^2+AP_1+B=0.$$

With the values of A and B which result from these, namely

$$A=-(P_0+P_1) \text{ and } B=P_0P_1-T_0^2, \quad . \quad . \quad . \quad (15)$$

we shall have for all points of the body

$$\phi = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

since here both the differential equation (14) and the limiting conditions in the surfaces σ_0 and σ_1 are satisfied, while the limits for the rest of the surface of the body, where k is *nil*, are satisfied by any given value of ϕ, and therefore as well as by that here taken. From equations (13), (15), and (16) we have

$$T^2 - T_0^2 = (P_0 - P)(P - P_1). \quad . \quad . \quad . \quad . \quad (17)$$

If, then, a constant electrical current be passed for some time through a conductor of any shape which is surrounded by bad conductors, and if the temperature at the two surfaces be kept constant and equal, it will be possible by equation (17) to calculate the temperature at each point of the conductor from the two differences of electrical tension at the given point and the conducting surfaces. The result found will, conversely, serve for an experimental determination of the Centigrade degree in absolute units.

The increase of temperature which depends on the electrical current is $T - T_0$. Now

$$T^2 - T_0^2 > (T - T_0)^2;$$

and in accordance with the last equation,

$$(T - T_0)^2 < (P_0 - P)(P - P_1).$$

As the right-hand side acquires its greatest value for $2P = P_0 + P_1$, if P_0 is taken as greater than P_1, we have also

$$T - T_0 < \frac{P_0 - P_1}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

From this we see that the greatest increase of temperature which can result in any point of the circuit is always numerically smaller than half the difference of the electrical tensions in the two conductor-surfaces. It would be *equal* to just half this difference if the conductor-surfaces could be cooled to the absolute zero—that is, for $T_0 = 0$. Thus the differences of electrical tension, and the greatest increase of temperature which can be obtained by them, stand in the closest connexion with each other.

Hence it is not without interest to calculate the difference of electrical tension in the poles of a voltaic element for instance, the electromotive force of the element, in Centigrade degrees. Thus the electromotive force of a Daniell's element is about 12×10^{10} absolute units; half the difference of tension of the elements, or, as it is commonly expressed, the positive tension of the element (the negative taken at the same amount), is therefore 6×10^{10}

absolute units, or (since 1° C. is equal to 216×10^5) 2780° C. This would thus be the greatest increase of temperature which the element could produce with a constant current in a circuit if the element itself were kept at a constant temperature. This increase of temperature would in fact only take place if the element or its poles could be cooled down to absolute zero; if, on the other hand, we assume for this the temperature of 20° C. above the freezing-point of water, we shall obtain from equation (17) 2502° C. as the greatest increase of temperature.

In a thermoelectric element of copper-pyrites and copper investigated by Bunsen*, the electromotive force was about equal to one tenth of a Daniell's element; the positive tension therefore was about equal to 278° C. when one junction was heated to the melting-point of tin, and the other to about 60° C. above the freezing-point of water. In fact the greatest increase of temperature, if it were calculated as above, would be about 111° C.

Yet it cannot be concluded from this that, for instance, the last-named element could not produce a spark (and consequently a far greater heating) through the interruption of the circuit; I am convinced, on the contrary, that this is possible. To show with what extraordinary facility the electrical spark can result from a break in the circuit, I will adduce the following experiment. An electrical current, whose intensity in absolute measure was 20, was passed through a copper wire 1 millim. in thickness. This was connected with a sharp knife-edge, with which another part of the wire was scraped with rapid strokes. It was then seen that in complete darkness there was still a bright space between the steel knife-edge and the copper wire when the distance between the two points of the copper wire which were placed by the knife-edge in conducting communication was only 400 millims. This facility with which a spark results from the interruption of the current, however, shows only that *induction* here plays an important part.

VII. *Notices respecting New Books.*

Light Science for Leisure Hours. Second Series. *By* Richard A. Proctor, *B.A.* London: Longmans and Co.

MR. PROCTOR tells us in his titlepage, quoting from Tennyson, "the truths of science [are] waiting to be caught;" and numerous at the present day are the fishers for these truths. In the work before us the author performs the office of urging scientific facts combined with his own and others' opinions into the nets prepared and spread for their reception, these nets being the minds of his readers. In the short notice which we propose to give of this second series, it will be our object to draw the attention of the reader to the mental process which he ought to perform while perusing the familiar essays presented to him: he must gather of every kind of opinion; indeed he cannot fail to do so; but it is incumbent on him to store up that which is good and to cast the bad away.

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* Pogg. *Ann.* vol. cxxiii. p. 505.

These thoughts have been suggested by the controversial spirit which generally pervades the essays. So important is it to present the truths which science seeks in a familiar form to the uninitiated, that, after carefully perusing the volume, we pondered for some interval of time over the title itself, "Light Science" for "Leisure Hours," and were almost inclined to come to the conclusion that it partook of the nature of a misnomer. Turning to the essays entitled "The ever-widening World of Stars" and "Movements in the Star-depths," we could not find in either the characteristic of *lightness*. With so masterly a hand have they been written that every step the author takes to establish the point he aims at (that of the unity of the sidereal universe) is of the most sterling character. To regard either of them as *light* is doing injustice to the sublime science of which they form a part; and we feel certain that if our leisure hours are to be occupied by that which is "light" (we cannot help adding the concluding part of the sentence, "and trifling"), then Mr. Proctor's Essays are not the pabulum for minds that seek for light reading in their leisure hours; for there is not one essay in the volume but requires thought, and deep thought too, ere the reader can gather up the good and cast the bad away. Perhaps Mr. Proctor adopted the title from a desire to catch the nets that they might catch the truths.

In the author's sketch of the life and works of Mrs. Somerville, we find him giving expression to his conviction that, as respects the main purpose of her great work, 'The Mechanism of the Heavens,' Mrs. Somerville failed entirely; and as explanatory of so remarkable a conclusion, one, be it remembered, opposed to the opinion of the late Sir John Herschel, which is quoted by Mr. Proctor, the author alleges that in his opinion success was altogether impossible, and also that "the thorough training, the scholarly discipline which can alone give to the mind the power of advancing beyond the point up to which it had followed the guidance of others, had unfortunately been denied to her." Otherwise, and under happier auspices, our author intimates that Mrs. Somerville might have done original work. Mr. Proctor, we apprehend, has had the advantage of this thorough training, and has experienced this scholarly discipline, and therefore is on the road for executing much original work; but has he ever heard of men or women being beyond the age in which they lived and worked? Have *all* who occupy niches in the great temple of Science had their names enrolled in the records of universities? or have the pioneers in the perpetual siege which has been for ages and is still carried on with the view of acquiring a knowledge of the great forces operating in the Natural World, *always* been men who have first distinguished themselves by their proficiency in their academical studies? Where are the self-taught men who have even left behind them in the race others who have had the advantages of which Mr. Proctor speaks? We cannot agree with the author even in the slightest manner depreciating the work of so gifted a woman as Mary Somerville; and it might have been that, had she undergone the discipline of which he speaks, she might have found herself hampered, even as when she endeavoured to please Mr. Murray she considered that she departed from clearness and simplicity. We fully agree with her own estimate, that the truths of science are in themselves not so formidable as most persons imagine; and, as regards the popularizing of science, so dear to Mr. Proctor, we much question whether the course adopted and greatly patronized in the present day is one calculated to extend the basis of the pyramid of knowledge. Is it not rather aiming at increasing its height? and do we not find in our ordinary intercourse with mankind that the knowledge is in the books and but seldom in the minds of men?

There are many passages in the Essays to which we should like particularly to direct the attention of the reader, both for their excellence and also for the controversial aspects which they bear; but our space reminds us that to the reader himself we must leave them, simply advising him to use his own judgment, and to read the work not as one which he would take up to while away a leisure hour, but one requiring patient and steady thought, and especially to weigh the author's opinions, which in some cases we think he will find far from being "light." The Essays are calculated to advance science, not so much by the author's endeavour to present it in an attractive form, as by leading the thoughtful reader to test every opinion presented in them.

Selections from the Portfolios of the Editor of the Lunar Map and Catalogue. London: Taylor and Francis.

This Selection, in which it is intended to place on permanent record observations of the physical aspects of lunar objects, contains papers by the Rev. T. W. Webb, Edmund Neison, Henry Pratt, and the Editor, which treat more or less on changes on the moon's surface, a knowledge of which is increasing in interest. These are followed by illustrated notices of recent observations, and comparisons of these with older observations. We hope this attempt to bring observers of the moon into communication with each other will meet with the support it deserves. From the great heat which the moon's surface attains under its long day it is likely that change is continually occurring upon it.

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xlv. p. 391.]

March 13, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"Visible Direction: being an Elementary Contribution to the Study of Monocular and Binocular Vision." By James Jago, M.D. Oxon., A.B. Cantab., F.R.S.

It is a well-known fact that when the eye has been displaced in its socket, as, for instance, by the tip of the finger applied to the eyeball through the eyelid, all objects seen by it deviate from their true directions; and the author's mode of proceeding in this paper is to inquire whether visual deviations that may be observed in arbitrary, but methodically devised, displacements of the eyeball in its socket follow any law, and then to consider how far the results thus derived are conformable with other monocular and binocular experiences, and how far they may be available in the explanation of certain phenomena that have been deemed anomalous in physiological optics.

Having pointed out means by which the ball may be easily displaced in any direction, he draws attention to the fact that, when by such means the apparent directions of objects seen by the eye are made to deviate from their true directions through fully 30°, the orbital muscles so fully retain their command over the movements of the eyeball, that that point in the visual field which was painted on the point of direct sight in the centre of the foramen centrale retinæ still continues to be there painted. He shows this to happen whatever be the direction in which the eyeball is displaced in its orbit.

This fact being a fundamental one in the inquiry he has in hand, he puts it to nicer tests still.

He adjusts the two eyes, when equally displaced so as to cause objects to deviate greatly from their true directions, to look awhile at the top of a high object in the open air, and having obtained a strong *spectrum* of this object in the retinæ, he, with the released eyes, looks at an appropriate mark on a grey wall, and finds that the spectrum really has its margin across the point of direct sight; and he tries other experiments in corroboration.

Also by agitating a pin-hole in a card across the eye when looking at such a high object, he brings the retina into view, and sees that the point of direct sight is visibly within the foramen centrale retinæ, as made visible by the shadow of the wall that bounds the foramen. He indicates other means of proving the same fact.

He gathers from a series of experiments that the mastery of the orbital muscles over such movements of the eyeball as are requisite for pointing the optic axis to its objective point, is practically unimpaired by such shiftings of the eyeball in its socket as have been described.

He then proceeds to show that the regulating duties of the orbital muscles, when the eyeball is displaced in its orbit, are not only fulfilled as to the rotation of the optic axis about a central point, but as to the rotation of the eyeball about this axis. To make experiments to this end, we must have another subjectively visible retinal spot besides the foramen centrale; and this we negatively have in the punctum cæcum, or at the base of the optic nerve.

A diagram is devised by which we may manage that one point of it shall be seen by the direct sight of both eyes, whilst another point is found to fall in the middle of the blind spot of one eye; and the diagram is examined by this eye when this has been pushed from its orbital place upwards, downwards, inwards, and outwards, and in various oblique directions, besides when more or less twisted on its axis; and thus it is demonstrated that what happened with the point of direct sight in the retina happens equally surely for every other retinal point—that under all these displacements the orbital muscles do not forfeit their control of the eyeball, but so regulate its movements that the different points of the field of vision remain constantly painted on the same retinal points.

From these and other methodically continued experiments, he draws the general inference, that if the centre of the foramen centrale retinæ be forced at any instant from its position by any sort of manipulation, and then made to describe a circle round its first position of ease whilst the optic axis has never ceased to remain parallel to its first direction (that is, has generated a cylinder in revolving), the axis of the *seeming* field of vision will have so revolved as to have generated a cone, whose apex is posterior to the retina in the first or undisturbed direction of the optic axis. The like might have been said of any other normal to the retina, the axis of the base of the optic nerve, for instance, were it accessible to light, whilst a twisting retinal movement about a fixed axis twists the *seeming* field of vision.

If the optic axis revolve so as to generate a cone whose apex is in front of the eye, the axis of the *seeming* field may, according to circumstances, generate a cylinder, or a more acute cone enclosing the other.

Conversely, the parallax of the visual field being noted, we can assign the retinal displacements that have produced them.

Should undue contraction of any orbital muscle, or discordant contractions of the orbital muscles, engender visual parallaxes, we may as safely judge from these parallaxes of the retinal displacements that must have been induced, as if they had been due to manipulation of the eyeball.

In these summarized conclusions we have the means of solving highly important problems in physiological optics.

It is found that sensation, or the function of responding to objective light, is exclusively resident in the retinal elements of the bacillar layer, but that the visual functions of the retina extend no further; for it has been evinced in manifold experiments that when the axes of all the pencils of objective light which concur in imaging a picture upon the retina are normals to its surface, any point in the picture may be *perceived* as lying in successive directions, forming very variable angles with and round about its normal. The retina cannot inform us of the visible direction of any point painted on its surface.

This being so, there is no alternative but to seek for a solution of the mystery in the structure out of which the retina proceeds, for the property in question is plainly inherent in the visual nervous apparatus.

The author recalls that he had long ago pointed out that, though the optic nerve in its orbital course and its fibres in their retinal course are obnoxious to mechanical pressure, no visual sensation can be immediately produced by such pressure on nerve-trunks or branches. Sensation can only be produced by pressure through the sclerotic by affecting the rods and cones of the bacillar layer, and then only when the flexure of the retina crowds together the *internal* (as to the eye) ends of the bacillar elements. He cites his former words:—"When we turn in the dark the eyeballs sharply, or even mildly, a couple of white circular rings, brighter at one margin than the other, enclosing a paler area with a central dark spot, flash forth, the diameter extending an angle of several degrees. . . . . . The phenomenon is plainly the result of flexure of the retina where the nerve runs into it, as the eye is pulled round in its socket until it drags upon the nerve; and it is to be noted that it is *again* where the inner retinal elements are squeezed laterally that the phenomenon is disclosed." The absence of the tough and dense sclerotic where the nerve penetrates it, as well as of the choroid, indicates how readily the nerve must yield to the slightest traction.

In these previously recorded facts the author feels assured that he had, unwittingly, provided himself with a key to the secret of visible direction.

For it has been shown by diversified experiments that whenever there is a parallax in visible direction it is accompanied by a displacement of the base of the optic nerve in the same direction—that is to say, by *traction* upon the nerve-stem, tending to carry its distal extremity that way. The "white circular rings, brighter at one margin than the other," have been instanced as proclaiming that such traction cannot occur without flexure between the nerve-stem and the eye-apple, which displays itself at the junction of the optic disk with the surrounding retinal expansion. In other words, under the concordant action of the orbital muscles, all the movements of the globe are so equably coordinated that the nerve-stem is never subjected to unwonted traction, and consequently always emerges through the ocular tissues to open out into the retina as a normal to their surfaces, in which case no visual parallax appears. But no sooner is there lateral traction than the axis of the emergent nerve-stem, or of the optic disk, deviates from the said normality; and were that disk impressible by objective light, its central point would deviate in the same direction, and an equal deviation in visible direction would be associated with every other point in the visual field.

Hence we are fairly landed upon the conclusion that visible direction, which has already been tracked backwards to the optic nerve, is a function of its terminal direction, being identical with it at the centre of the optic disk, both in the equable use of the eye and in the unequable.

Finally, it is clear that if the eyeball be twisted round the axis of the optic disk the terminal portion of the nerve will be twisted in the same direction; and thus the opposite twisting of the visible field in certain experiments related are explicable by the same hypothesis—an hypothesis that accounts for all the phenomena of visible direction, whether regular or irregular.

Whenever the inverted retinal image, by means of nervous arrangement, is reinverted, an erect image is seemingly projected, if not from, by means of the base of the optic nerve.

The principles here arrived at, when applied to binocular vision, lead to the observation of phenomena that have not been before put on record.

Wheatstone, in his classic paper in the Philosophical Transactions, wherein he announces his discovery of the stereoscope and expounds its theory, only speaks of stereoscopic vision from two perspectives, an appropriate one for each eye, when (no instrument being used) the optic axes meet each other beyond them, or have previously intersected, so that each eye sees the other's perspective; that is, in all experiments by him and other subsequent writers on the subject the optic axes have always been supposed to intersect or to lie in one plane.

But as it has been demonstrated that the axes of visible direction need not be coincident with the optic axes, it ought to follow that we may continue to see bodies in relief from a pair of stereoscopic perspectives, though these are not placed transversely to each other.

Two perspectives of a pyramid are drawn, such as, when placed laterally apart as is usual in stereoscopic slides, and looked at by concourse of the optic axes beyond them, yield a hollow pyramid, and when looked at by a previous decussation of these axes yield a solid pyramid. But these perspectives are placed so that the one which was at the left has the one that was at the right immediately underneath it, with about half an inch of plain paper between them.

Then it comes to pass that by properly displacing the right eyeball upwards, by means of the tip of the finger placed underneath it, we can put the under perspective immediately upon the upper one seen with the other eye, and thus realize the hollow pyramid; or by placing the finger upon the top of the left eye, we can depress the upper perspective to cover the under one, and thus realize the solid pyramid. The first pyramid depends from the plane of the paper, the second stands upon it.

By means of a finger under one eye and another upon the other, we can obtain either hollow or solid pyramids anywhere between the two perspectives; or by a finger on both eyes, or under both, we can obtain the pyramid and its "converse" below or above both perspective outlines.

In all these cases the optic axes do not intercept each other, but the axes of visible direction (functional of the final directions of the optic nerves) do meet on the paper; that is, the first pair of axes are not, and the second pair are, in such a realization as is herein planned, in one plane.

In all these cases the perspectives fall on similar parts of the two retinæ, as in the modes originally mentioned by Wheatstone.

The author goes on to consider in what way the sensorium refers the sensations it receives notice of from the optic nerves into space, so as to fix the place, size, and form of an object.

The theory of vision in retinal normals being proved to be untenable, it is admitted that there is some such association of the two retinæ as to have fairly suggested the theory of "identical" or "covering" points; but this relation he believes to be subordinate to a law by which the sensorium *projects* or *emits* its perceptions into space, as it were in two imaginary cones of *sight-rays*, which, though not issuing from the ends of the optic nerves, have apices whose positions are functional of the directions of these ends for the instant in question—and that it is by the intersection of the sight-rays in these cones, limited by the law of similar retinal parts, that the places, sizes, and forms of objects are determined. Hence, if we conceive that a pair of stereoscopic perspectives, one being imaged on one retina and one on the other, exist as sight-affections in miniature in the substance of the optic nerves, the size of the resultant solid form will be greater the greater is the distance from the nerves at which the axes of visible direction intersect, or the optic axes when they are coincident respectively with them.

The paper concludes by exemplifying in sundry ways the modes in which the conclusions in it may be applied in investigating seemingly anomalous phenomena in physiological optics.

IX. *Intelligence and Miscellaneous Articles.*

INDUCED CURRENTS AND DERIVED CIRCUITS.

BY JOHN TROWBRIDGE.

THE expression for the intensity of an induced current, deduced by Neumann and Sir William Thomson, is as follows:—$i=\frac{1}{k}\frac{dU}{dt}$, in which k is a coefficient depending upon the resistance of the complete wire in the secondary circuit, and U is a certain "force-function" which depends solely upon the form and position of the wire at any instant, and on the magnetism of the influencing body. The expression, in general language, is as follows:—

"When a current is induced in a closed wire by a magnet in relative motion, the intensity of the current produced is proportional to the actual rate of variation of the 'force-function' by the differential coefficients of which the mutual action between the magnet and the wire would be represented if the intensity of the current in the wire were unity."

This investigation was undertaken to ascertain if the laws of derived circuits apply to the currents of induction, which are represented by equations of which the above is a type. A reflecting galvanometer of large resistance was included in the secondary circuit, and connected by copper wires of very small resistance with the coil in which the secondary currents were produced: the resistance of these wires was infinitesimal in comparison with the resistance of the galvanometer. The galvanometer was then shunted. The first two columns of the following Table show that, with an inappreciable resistance outside of the galvanometer coils, the shunts made no difference in the deflection of the galvanometer-needle when the shunts were not less than three ohms. Below this the current divided. The resistance of the galvanometer was 5880 ohms; and the last numbers in the second and third columns show that an equal impulse was transmitted through both the shunt and the galvanometer; for no reason can be assigned why it should take one course in preference to the other. Two galvanometers, therefore, of the same resistance, one forming the shunt to the other, will give the same deflection, which is equal to that given by the undivided circuit.

Exterior Resistances, in ohms.	Shunts, in ohms.	Deflections.	Exterior Resistances, in ohms.	Deflections.
0	3	210	10	210
0	4	210	20	210
0	5	210	30	210
0	6	210	40	210
0	5880	210	100	190

Resistances were then introduced into the circuit exterior to the galvanometer-coils, and a shunt of 588 ohms was used.

The fifth column shows that no effect was produced by the shunt until the exterior resistance was appreciable in comparison with that of the galvanometer.

The following Table exhibits the effect of resistances which were appreciable in comparison with the galvanometer-resistance. The same shunt of 588 ohms was used. The second column is calculated on the assumption that $i=\frac{dU}{dt}$ (where k^{1} is a coefficient) is equivalent to $i_1=\frac{E}{R}$, and that the laws of Kirchhoff hold. The third column is obtained from the experimental data. The fourth and fifth columns are also calculated on the assumption that $i=\frac{E}{R}$

=tangent of the deflection. Columns second and third are expressed in arbitrary scale-divisions.

Exterior Resistances, in ohms.	Calculated value of *i*.	Experimental value of *i*.	Ratio of Intensities.	Ratio of Tangents.
1500	1242	1375		
2000	1033	1055	1·06	1·03
2600	954	990	1·06	1·04
3000	767	825	1·06	1·04
3500	673	770	1·11	1·05
4000	613	660	1·05	1·03
4500	558	649	1·05	1·07
5000	514	550	1·04	1·03

It will be seen by comparison that, with large resistances exterior to the galvanometer-resistance and appreciable in connexion with it, the laws of the division of currents practically hold; and as the exterior resistance approaches that of the galvanometer the coincidence with the laws is more marked.

From the above it appears that, under certain conditions, an induced current does not divide according to the laws of divided circuits, but approximates to these laws when there is a resistance exterior to the galvanometer which is appreciable in comparison with that of the galvanometer.—Silliman's *American Journal*, May 1873.

ELECTRICAL FIGURES ON CONDUCTORS. BY H. SCHNEEBELI.

M. Schneebeli has investigated the conditions on which depend the dimensions of Kundt's electrical figures, which result from the adhesion of a fine insulating powder upon a metallic conductor from which a discharge has just issued[*]. In his experiments, the discharge of a Leyden jar took place between a horizontal metal plate, sprinkled with Lycopodium-powder (for the production of the electrical figures), and an electrode in the form of a knob, cone, or point surmounting it. The author has found, like M. Kundt, that, *cæteris paribus*, the diameter of the figure increases with the distance of the electrode from the plate, but not in a constant ratio; the line which represents the ratios is not straight, but an undulated curve. Also the size of the figure augments with the quantity of electricity which produces it.

When the electrode is composed of a certain number of points, a circular figure is formed beneath each of them. If a small disk of glass is interposed in the path of the discharge, there is produced on the plate a space void of powder and having exactly the shape of the disk.

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[*] Kundt's figures are produced with great neatness on the positive electrode, but are only obtained with difficulty upon a plate serving as the negative electrode. That physicist found that the diameter of the figures increases with the length of the discharge, and in proportion as the diameter of the opposite electrode to the plate diminishes (*Archives des Sciences*, 1869, vol. xxxv. p. 212).

With electrodes of a conical form with an angle of 60° or of 30°, or in the shape of a tapering needle, M. Schneebeli ascertained that the electrical figure is as much larger as the angle at the summit of the cone is smaller.

Lastly, the diameter of the figure is greater when the discharge is effected in a rarefied gas than at the ordinary pressure.—*Archives des Sciences Physiques et Naturelles*, vol. xlvi. p. 269.

NOTE ON GASEOUS PRESSURE. BY ROBERT MOON, M.A., HONORARY FELLOW OF QUEEN'S COLLEGE, CAMBRIDGE.

As Mr. Strutt appears indisposed to continue the controversy between us[*], it may perhaps be permitted me to recapitulate its results.

Mr. Strutt impugned my views as to gaseous pressure on two grounds[†]:—

1. That an expression for the pressure involving both velocity and density leads to absurd results.
2. That the analytical argument upon which I based such an expression fails, "the question at issue" being "a purely physical one"—the pressure prevailing in a fluid in motion being, as he considered, settled by Boyle's law, which he assumed to hold in that case.

How far the first part of this criticism was justified I remit to the judgment of those who may think it worth while to refer to my papers in the Philosophical Magazine for August and February last how far the second part is consistent with fact appears from the second paragraph of Mr. Strutt's recent paper.

6 New Square, Lincoln's Inn,
June 2, 1873.

NOTE ON THE USE OF A DIFFRACTION-"GRATING" AS A SUBSTITUTE FOR THE TRAIN OF PRISMS IN A SOLAR SPECTROSCOPE. BY PROF. C. A. YOUNG.

Since the diffraction-spectrum differs from a prismatic spectrum of the same length in having the less-refrangible rays more widely dispersed, it some time ago suggested itself that a so-called "*Gitterplatte*," or "grating" of fine lines, might advantageously replace the prisms in spectroscopes designed for the observation of the solar prominences through the C line. In this idea I was strongly confirmed on seeing last winter some of the beautiful gratings ruled upon speculum-metal by Mr. Chapman, Mr. Rutherford's mechanician. The spectra furnished by these plates far exceed in brilliance and definition any thing of the kind ever before obtained.

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[*] See paper "On the Law of Gaseous Pressure" in the Philosophical Magazine for June 1873.

[†] See paper "On Mr. Moon's Views on Gaseous Pressure" in the Number of the Magazine for July last.

Through the kindness of Mr. Rutherford I have recently come into possession of one of them, having a ruled surface of something more than a square inch, the lines being spaced at intervals of $\frac{1}{6480}$ of an inch. Combining this with the collimator and telescope of a common chemical spectroscope, we get an instrument furnishing a spectrum of the first order, in which the D lines are about twice as widely separated as by the flint-glass prism of 60° belonging to the original instrument. In the neighbourhood of C the dispersion is nearly the same as would be given by four prisms.

The spectra of the higher orders are generally not so well seen, on account of their overlapping each other; but fortunately with one particular adjustment of the angle between the collimator and telescope, the C line in the spectrum of the third order can be made to fall in the vacant space between the spectra of the second and fourth orders, and we thus obtain an available dispersion nearly the same as that of the instrument I am accustomed to use.

On applying the new instrument to the equatorial, I found (under atmospheric conditions by no means favourable, though the best that have presented themselves as yet) that in the first-order spectrum I could easily see the bright chromosphere-lines C, D_3, and F; I could also, though with great difficulty, make out $H\gamma$, (2796 K). On opening the slit the outline of the chromosphere and the forms of the prominences were well seen in the spectra of both the first and third orders, quite as well, I think, as with my ordinary instrument in the same state of the air. The spectra are, of course, fainter; but as this loss of light affects the background upon which the prominences are projected as well as the objects themselves, it does not materially injure their appearance.

The grating is much lighter and easier to manage than a train of prisms; and if similar ruled plates can be furnished by the opticians at reasonable prices and of satisfactory quality, it would seem that for observations upon the chromosphere and prominences they might well supersede prisms.—Silliman's *American Journal*, June 1873.

DUPLEX TELEGRAPHY.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

It may be as well to say that the plan of duplex telegraphy by means of two batteries, working together on the receiving instrument, but counteracting one another on the instrument at the sending end, was patented by me some months since. It is one of the many instances in which a plan occurs to two different people nearly at the same time.

Yours obediently,
H. HIGHTON.

Putney, June 11, 1873.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1873.

X. *Fluorescent Relations of certain solid Hydrocarbons found in Petroleum Distillates.* *By* HENRY MORTON, *Ph.D., President of the Stevens Institute of Technology**.

IN a previous paper embodying the results of some observations upon the fluorescent relations of commercial anthracene†, I have alluded to the existence of an analogous body among the final products of some petroleum-distillations; and I now propose to relate more fully the facts developed in a study of these bodies.

Nearly a year since, Professor E. N. Horsford placed in my hands a small specimen of petroleum-distillate, from which I succeeded in separating a crystalline solid, fluorescing in a remarkable manner with a bright green colour.

The amount of material at my command was too small (being in fact but a few grains, extracted from less than an ounce of the substance sent me) to admit of more than a preliminary examination. I succeeded, however, in establishing its optical (fluorescent and absorptive) relations to commercial anthracene, and its difference from that substance in fusing-point and solubilities.

After encountering some difficulty in tracing up the supply to its source, I was introduced by Professor G. F. Barker to Mr.

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* Communicated by the Author.

† See this Journal, 1872, vol. xliv. p. 345.

John Truax, of Pittsburgh, who has been most obliging in furnishing me with every facility that could be wished.

The preliminary history of the material with which we have to work is as follows:—

When the residues left in the distillation of petroleum for the manufacture of illuminating-oils are redistilled to obtain lubricating-oils and paraffine, there passes over near the end of the operation, and when the still is at the bottom almost or even quite red-hot, a thick resinous matter of a dark honey- or light sepia-colour, which is used as a lubricant for the necks of "rolls" in iron mills. This is the material from which the new body is extracted by the following process.

The tarry matter above described is mixed with about its own volume of benzine (petroleum-naphtha), and is thrown on a stout filter, where it is well washed with the same solvent.

This leaves a dark olive-green flaky powder, constituting about 3 per cent. of the original mass, and very similar in appearance to crude commercial anthracene. We now wash this with alcohol, and even digest it in this liquid, by which means a brown material, whose solution in alcohol has a decided blue fluorescence, is removed.

The substance is then dissolved in hot benzole (coal-tar naphtha) and filtered in a jacketed funnel (by which operation a quantity of black powder is removed), and is allowed to crystallize on cooling.

It then deposits in very small acicular crystals collecting in tufts; and the purification is carried on by re-solution and crystallization from fresh benzole. The substance then has the colour of potassium chromate.

The following Table of solubilities will indicate the motive for the above treatment.

Benzine,	hot (160° F.),	dissolves	1 part in	1155
„	cold (70° F.)	„	„	2900
Alcohol,	hot (160° F.)	„	„	4172
„	cold (70° F.)	„	„	16625
Benzole,	hot (160° F.)	„	„	95
„	cold (70° F.)	„	„	152

This substance dissolves in turpentine pretty freely, and yet more so in carbon-bisulphide and chloroform.

In ether and olive-oil and carbon-bichloride it is hardly as soluble as in benzine.

Methods of Examination.

For the study of the spectrum of the fluorescent light, the following arrangement was made. The porte-lumière A being attached to the shutter of a window facing towards the south, a beam of sunlight was thrown by it horizontally into the room and concentrated by a lens of 12-inch focus placed at B. At C was placed an apparatus (fig. 2) consisting of a circular horizontal table adjustable up and down on a vertical rod and turning with a "click." Around the circumference of this table were eight little "stalls" capable of holding test-tubes or specimen-bottles. By this means eight different specimens could be rapidly compared, each in succession, being brought by the action of the click into an identical position with reference to the exciting light and the spectroscope. This whole apparatus was so placed that an image of the sun was formed on the tube or bottle nearest to the lens B. A glass tank filled with a strong solution of ammonio-cupric sulphate was placed, as indicated, between B and C; and to this was sometimes added a plate of violet glass. The spectroscope D employed in these observations was generally one of Browning's with a single prism of very high dispersion, giving a range of 3° 54′ between C and S of the solar spectrum; but a spectroscope with two similar prisms by the same maker and one by Desaga of less dispersive power were also used occasionally. A small direct-vision spectroscope was also frequently employed to detect the presence of faint lines or bands of absorption.

Fig. 1.

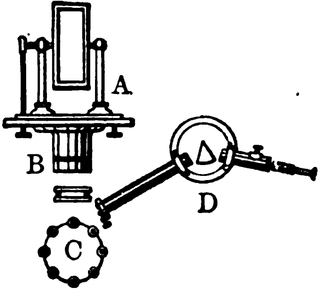
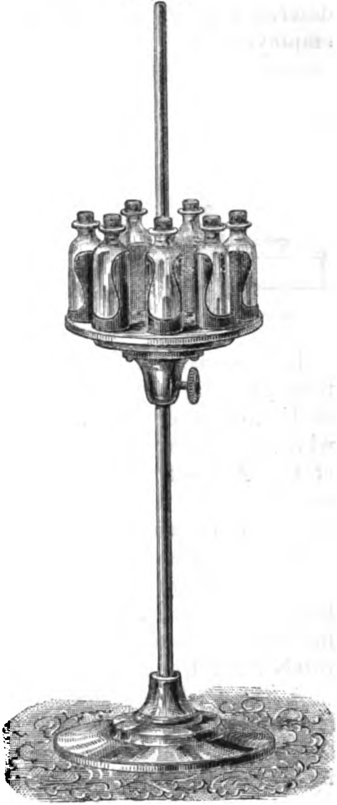
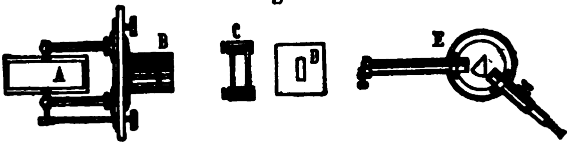


Fig. 2.



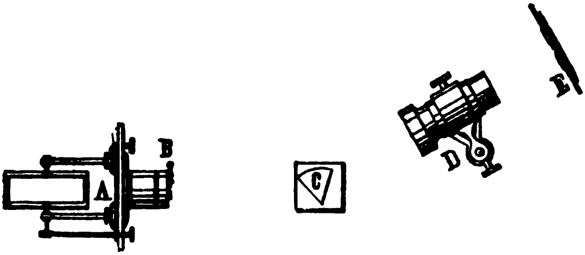
For examining absorption-bands, every thing else remained the same, except that the spectroscope was turned round into the position indicated in fig. 3, and the stand for specimens was replaced by a plane table D, on which the substance to be studied was supported either in a cell of glass or in a bottle. White glass bottles of about one-ounce capacity formed cylindrical lenses of a very convenient character for this purpose.

Fig. 3.



To examine the relative exciting-power of the various colours of the spectrum, an arrangement, nearly identical with that described by Professor Stokes as his "Third Method"*, was employed.

Fig. 4.



In this case the light thrown in by the porte-lumière A passed through a narrow adjustable slit at B, then encountered a prism at C, and after this a lens of about 12-inches focus at D, by which an image of the spectrum was thrown on a screen or tank at E. A spectroscope placed behind this screen was employed, in a manner presently to be described, to measure the refrangibility of certain rays.

Thus, suppose a pure spectrum showing the Fraunhofer lines clearly to be thrown on a screen coated with some fluorescent body placed at E, and that a point of maximum or minimum fluorescence were observed, and we wished to determine accurately the refrangibility of the rays to which this effect was due. A fine needle was made to pierce the paper at this point; and the light thus allowed to pass through the screen was then measured by the spectroscope. Of course the hole, once made, could, by movement of the screen, be brought into coincidence with any point in the spectrum.

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* Philosophical Transactions, 1852, Part II. p 470.

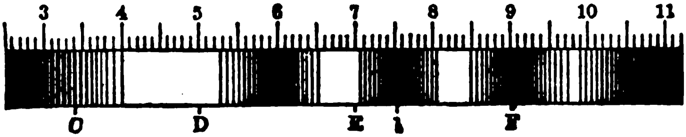
With solutions in tanks the same plan was followed, a small piece of card with a needle-hole being moved into the required position and the transmitted light then measured.

Observations.

Spectrum of Fluorescent Light from Solid Thallene.

When a portion of the yellow crystalline substance above described, and which, to avoid circumlocution, I shall hereafter call thallene, in allusion to the brilliant green colour of its fluorescence, is examined in the manner first described and represented in fig. 1, we obtain a spectrum such as is indicated in fig. 5.

Fig. 5.



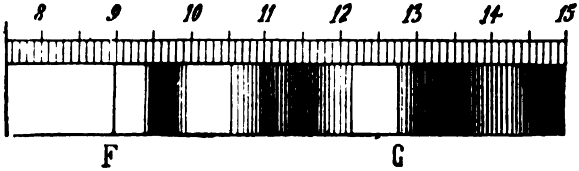
This begins with a very broad bright space in the orange and yellow; then, separated by shades or bands of less brilliancy, follow two green spaces, and lastly one of blue, much less brilliant than the others, and best seen when the violet glass is added to the blue tank (see fig. 1). The scale employed in the above and subsequent drawings of spectra is that with millimetre-divisions introduced by Bunsen.

This spectrum, it will be seen, differs from that of impure anthracene, or chrysogen as found in commercial anthracene, in two respects. First, there is no decided separation of the orange and yellow rays from the red by a dark space in this thallene-spectrum as in the other. Second, there is found in this spectrum a strong bright band in the blue under circumstances in which it cannot be recognized in the spectrum given by the former substance.

Absorption-spectrum of Solid Thallene.

At a temperature of about 460° F., thallene fuses without decomposition; and we may thus obtain a translucent plate of it between slips of mica or on glass in a condition convenient for the study of its absorption in the manner shown in fig. 3. But we may also dissolve it in melted paraffine, or spread it by rubbing on filter-paper, or mix it with varnish and so coat paper with it. In any of these shapes, when examined by transmitted light, as in fig. 2, it shows an absorption-spectrum such as is represented in fig. 6.

Fig. 6.



In this we find a very strong narrow band having the line F in its centre, then a less-defined double band at 100 and 110 of the scale; another, yet less sharply defined, from 120 to 130 connected by a shade with the point about 140, where the absorption becomes total. The marked difference between this and the absorption-spectrum of commercial anthracene consists in the double character of the second band, and in the much lower position of the third band, which ends, above, about where that of the anthracene begins.

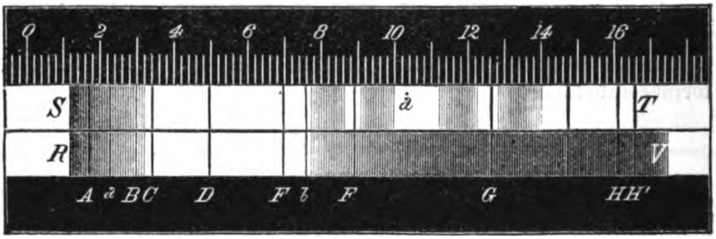
We will now pass to the method of observation indicated already in fig. 4.

The screen upon which the pure solar spectrum was thrown, located at E (see fig. 4), was prepared as follows:—A piece of filter-paper was coated with pure thallene by rubbing it on in powder with the finger. It adheres very well to the paper, and produces in this way a very regular and beautiful surface.

A strip of this is attached with gum to a piece of white card; and this slides in horizontal grooves in a frame E (fig. 3), which is pierced by a large opening.

The spectrum is so adjusted as to fall on the fluorescent strip, and also on the adjacent part of the card; and we have the appearance indicated in fig. 7, where S T represents the part of the spectrum which falls on the thallene-covered paper, and R V that portion which falls on the card. The spectrum upon the card runs, of course, from red to violet, but even below the fixed line G becomes very dim, and between G and H ceases to show any decided light. Where these higher rays fall upon the

Fig. 7.



Erratum in Number 304.

In Dr. H. Morton's paper, fig. 6 (page 94) should be fig. 9, and fig. 9 (page 97) should be fig. 6, and should be transposed as under.

Fig. 6.

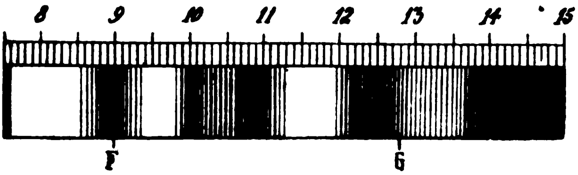
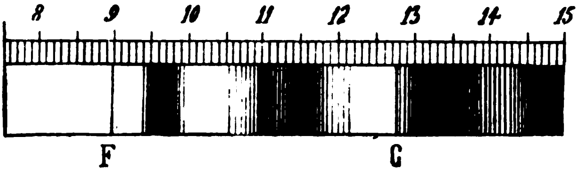


Fig. 9.



***To the Binder.*—Please paste the above figures over those similarly numbered on pages 94 and 97, which, as they now stand, are transposed.**

thallene, however, in place of becoming less brilliant towards the upper end, they glow with remarkable intensity; and all about 14 we find a brilliant green ground on which the lines H, H′ and others appear with beautiful distinctness.

Below this the colour is a bluish white as far as a little below F, beyond which there is no fluorescent action, and the spectrum looks alike whether it fall on the thallene or on the card.

The brightness of the spectrum on the thallene, however, is not uniform between 8 and 14, but presents a series of graduated maxima and minima, which are represented in the woodcut (fig. 7).

To measure the positions of these maxima, the simple device already alluded to was employed. A pinhole was pierced in the fluorescent paper at some convenient point, as at *a* (fig. 7); and by sliding the entire card or screen in the grooves which supported it, this pinhole was brought to the part of the spectrum whose location it was desired to fix (for example, as in the figure, at the middle of the broad maximum *a*); and then, the spectroscope being placed behind E, the refrangibility of the light passing through the pinhole is directly measured.

A comparison of fig. 7 with fig. 6, bearing in mind that the scale of the latter is double that of the former in actual dimensions, though the numbers correspond, will show that the maxima in fig. 7 exactly correspond with the absorption-bands of fig. 6.

This indicates that the absorption in this case is in very close relations with the fluorescence of the substance; and to this conclusion many phenomena to be presently noticed give confirmation. That such a connexion should exist is eminently natural, since those rays which expend their force in fluorescence, *i. e.* in producing vibrations of a less rate, must evidently disappear; but it does not follow that all rays which are absorbed develop fluorescence; and so, as in the uranium salts, we may expect to find absorption-bands which have no immediate relation to fluorescence, although here also there will always be a special absorption corresponding to the maximum of fluorescence-exciting power. This was observed by Stokes at the outset. What we have to notice here is that this body thallene has no appreciable absorption other than that due to its fluorescence—or, in other words, that nearly all the rays absorbed by it are converted into fluorescent motions and not into those of heat.

I have spoken only of the rays as high as H′ when referring to the fluorescence excited by a pure spectrum on a screen of thallene; but it must not be supposed that this is the upper limit of the effect. With lenses and prisms of quartz, I have projected on such a screen a solar spectrum showing the fixed lines for a great distance above H.

The fluorescence excited by rays about and above H does not, however, seem to suffer any change in intensity, but yields a uniform and brilliant green light, on which the solar lines are seen with admirable distinctness. Indeed this substance seems to me to surpass any heretofore known as a screen for the study of the violet and extra-violet rays.

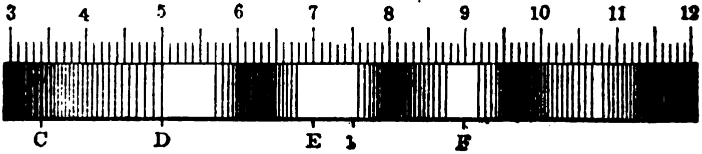
Thallene in Solution.

As had been already noticed, thallene is soluble in a number of liquids; and in all cases it communicates a strong *blue* fluorescence to the solvent, or, more strictly, fluoresces strongly with a blue colour in solution.

When this blue fluorescent light is examined with the spectroscope, it is found, like the green fluorescence of the solid, to break up into bands; but in the solution these are found in all cases to be displaced strongly towards the upper end of the spectrum, which at once explains the change of general tint in the light from green to blue.

Fig. 8, which represents the spectrum of the solution in benzole, will, by comparison with fig. 5, indicate this change; and we shall see that the three most distinct bright bands at 6·8, 8·4, and 9·8 in the spectrum of the solid are moved to 7·2, 8·9, and 10·7 respectively in the spectrum of the solution.

Fig. 8.



If in place of benzole we employ chloroform as a solvent, we find that the bands occupy sensibly the same positions; but if sulpuric ether is used, the bands are somewhat more displaced. With turpentine the displacement seems to be a little less than with ether, and with alcohol about the same, while with olive-oil an intermediate position is occupied, and with carbon-bisulphide the displacement is least of all.

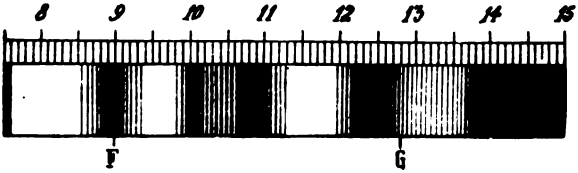
The following Table will give some measurements of the positions of corresponding bands in various solutions:—

Carbon-bisulphide.	Benzole and chloroform.	Olive-oil.	Turpentine.	Ether.
68·0	71·6	72·9	72·9	73·8
87·6	88·5	89·6	89·8	90·1
106·6	107·8	109·2	108·1	

If now we observe the absorptive action of these solutions in the manner indicated in fig. 3, we shall find that the absorption-bands, like those of fluorescence, are displaced upwards in the spectra of the solutions as compared with their position in that of the solid.

This will be illustrated by comparing fig. 9, which represents

Fig. 9.



the absorption-spectrum of thallene dissolved in benzole, with fig. 6, which is the absorption-spectrum of solid thallene.

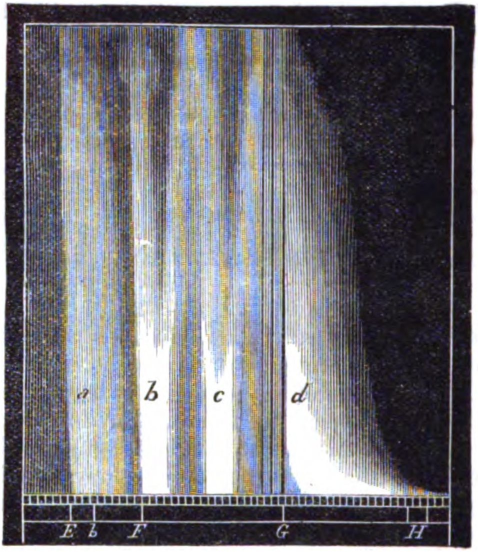
The absorption-bands of the solutions in benzole, chloroform, and olive-oil do not appear to differ in any important degree; but in the etherial solution the bands are higher than in the others. In the solution in turpentine the lower band especially is exceedingly faint, so as to escape recognition entirely in the method of observation employed in the other cases, even when a pint bottle of solution was used; but when the tube of a Duboscq saccharimeter was filled with this solution, this line could be detected with a direct-vision spectroscope.

In carbon-bisulphide solution a similar difficulty was encountered, so that in a preliminary publication I spoke of these bands as not appearing; but renewed observation has enabled me to assure myself that in this solution all the bands exist, and at lower points in the spectrum than in any of the other solutions yet examined, the lowest one in particular occupying a point even below that of the corresponding band in solid thallene. There is also in this solution a decided general absorption of all rays above F, which causes some special difficulty in measuring the bands.

In such cases as this the mode of observation to be next noticed is specially efficient as a guide, indicating what we must look for in each case. A similar displacement of bands by change of solvent has been noticed by Hagenbach in the case of soot, amide of phthalic acid, chlorophyl, purpurine, &c. (Poggendorff's *Annalen*, 1872, vol. cxlvi., p. 533); and I have also found it in a number of the uranium salts.

When a pure spectrum is thrown on the side of a tank of glass or quartz filled with the benzole solution, by the arrangement of prism &c. indicated in fig. 4 we see, on looking down into it from above, long trails of light running through the solution, as shown in fig. 10.

Fig. 10.



These trails are variously coloured as follows: *a* is very faint and olive-green; *b* is very bright and of a vivid green; *c* is bright sky-blue; and *d* is of an indigo tint running into violet towards H. The appearance is indeed exceedingly beautiful, from the richness of the colours and the delicate manner in which they fade off and blend.

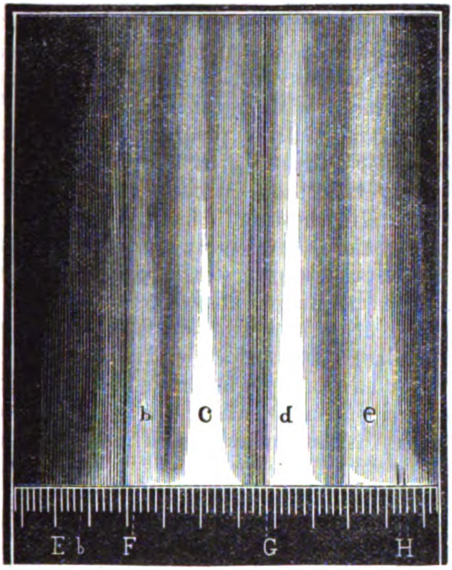
The brightest bands or trails (*b* and *c*) are each penetrated, as it were, from the further side of the tank by a long blade of darkness, the result undoubtedly of the absorptive action of the fluid for the corresponding rays, which causes them to be more rapidly exhausted; while those at either side, being less energetically active and acted upon, penetrate further before their motion is converted into fluorescence. Near H, where the actions of absorption and fluorescence are most intense, the light hardly seems to pass beyond the actual surface of the liquid.

Exact parallels to all these actions were observed by Stokes in chlorophyl (Phil. Trans. 1852, Part II. p. 489); but though I have examined all the substances enumerated by him, I have in no case found so beautiful an exhibition of the phenomena as this solution furnishes.

When the light from these trails is examined with a hand-spectroscope, it is seen that the bright green one *b* wants the upper blue band 10·7, which is present in *c* and *d*. This was a necessary result from Stokes's law, that fluorescent light never contained rays of higher refrangibility than that by which it was excited; for the exciting light of *b* was only a little above F, or 9 to 9·15 of the scale.

Turpentine dissolves only a small portion of thallene, which, however, communicates to it a blue fluorescence. When examined in the manner just described, its appearance resembles in many respects that of a similarly weak solution in benzole; but there are certain points of distinction. Thus the first band above F is at some distance from it; the second band is decidedly double, with its upper half of a paler colour; and the band above G is likewise evidently divided (see fig. 11).

Fig: 11.



The dark blades of absorption are not seen in this or in a dilute benzole solution.

A solution of thallene in carbon-bisulphide having, as we have seen, its absorption-bands very low in the spectrum, produces also in this last method of examination a very characteristic appearance.

The first trail of light is entirely below F, and is pierced by a dark blade; its colour is bright green. The next is midway between F and G, is bright blue, but does not penetrate very far into the solution, and does not show a black blade. The upper trail is of a faint indigo tint, penetrates but little into the liquid, and begins decidedly below G.

In a former paper attention was drawn to the effect which an exposure to sunlight produced upon the bands of absorption exhibited by commercial anthracene. A cold solution of that substance, showing its bands with great distinctness, loses them entirely by an exposure of ten minutes to the direct rays of the sun. Thallene similarly treated loses its lower band in five minutes, but its upper one (*i. e.* the broad double band) only after thirty minutes' exposure.

A saturated hot solution under like conditions loses its lower band in thirty minutes, and its upper one in about two hours. By placing it, however, near the focus of a large burning-glass of 15 inches diameter, ten minutes sufficed for this effect. When a solution of anthracene so exposed for one minute is allowed to cool, it deposits white pearly scales (of pure anthracene), which show no trace of a banded spectrum by fluorescence.

When a solution of the yellow anthracene (obtained by washing the crude material with benzine, dissolving in hot benzole, filtering, and crystallizing by cooling), which contains much chrysogen, is similarly treated, after ten minutes' exposure in the focus of the lens, it deposits on cooling crystals of anthracene nearly white, but still showing a banded spectrum; the bands are weaker than before, but occupy exactly the same positions; the general fluorescence of the substance is also greatly reduced.

Thallene, however, under similar conditions (that is to say, exposed in hot solution in a 4-ounce flask for ten minutes near the focus of the large lens and allowed to cool) deposits crystals almost white but having a slight grey shade; and these show a distinctly banded spectrum, whose bands are located in almost the same positions as those of an etherial solution, but are a little more elevated.

Their centres are in fact located as follows:—7·34, 9·13, 11·17, omitting the lowest band, which is too broad to be well indicated in this way.

The general colour of the fluorescent light emitted by this substance is light blue; and to distinguish it I would propose the name Petrollucene.

A solution of this in benzole shows a slight further elevation of bands, as 7·38, 9·2, and 11·24; and when a pure spectrum is thrown on this solution only two trails of light are seen—one about four fifths of the way from F to G, and the other above G. These are blue and indigo in colour respectively.

As illustrations of the action of fluorescence, these substances (thallene and petrollucene) surpass, I think, any bodies heretofore known.

The green light emitted by thallene surpasses in intensity that furnished by any other substance, the only one which approaches it being platinocyanide of barium in a peculiar condition, which, however, is by no means under control.

There are in the collections of the Stevens Institute of Technology some screens and words painted with this salt on cards of a few inches area, prepared by Albert Sohn, of Frankfort-on-the-Maine, which approach thallene in brilliancy; but the pure salt, even when forming fine crystals, is very inferior in the energy of its fluorescence.

Anthracene forms, as is well known, a compound with chlorine (bichloranthracene), whose solutions in alcohol and benzole fluoresce brightly and give a spectrum (banded in the upper regions) which has been studied by Hagenbach (Poggendorff's *Annalen*, vol. cxlvi. p. 385.

This led me to attempt the preparation of a like compound with thallene.

A quantity of bichloranthracene having been first made, to ensure accuracy in the method of working, thallene was submitted to the same treatment, *i. e.* was mixed with benzole and subjected to a current of chlorine for about an hour, and was again exposed in the dry state to the action of chlorine. A strong reaction was shown, considerable heat being developed, and the entire substance acquiring a chocolate colour. The product, however, showed no tendency to crystallize. In its tarry state and in solution its fluorescence is moderately strong, but yields a continuous spectrum from about C to E.

Having next prepared some bibromanthracene and examined its fluorescence, I submitted thallene to an identical treatment. But here also, though a compound was formed crystallizing in very minute needles from its hot benzole solution, neither the solid nor the solution showed any indications of fluorescence.

I found moreover that, while anthracene combines with about twice its weight of picric acid to form the picrate, which crystallizes in long needles of a rich strawberry colour, thallene requires fully four times its weight of picric acid to saturate it. On cooling from hot solution, a granular mass of minute crystals is formed, having a rich orange-red colour. Examined under the microscope, these crystals are seen to be short prisms with well-defined pyramidal terminations.

Sulphuric acid of density 60° B., which rapidly blackens anthracene even at the ordinary temperature, gives thallene a rich grass-green colour, which remains unchanged even after standing for weeks and exposure to a temperature of 60° C.

In this condition it continues to give a bright fluorescence, yielding a banded spectrum identical with that characterizing it in its normal state.

I should therefore conclude that no true combination had occurred, and that the change from a yellow to a green colour resulted from the presence of minute black particles produced by the action of the sulphuric acid upon some small portion of an adherent impurity. This view is confirmed by the fact that with petrollucene, which is almost colourless, sulphuric acid produces a dark green colour, approaching to black. Continued application of heat causes thallene, treated as above, to dissolve, forming a compound perfectly soluble in water.

The picric-acid compounds, both of anthracene and thallene, show no fluorescence; so that this gives us a convenient method of determining the point of saturation.

Examined with Becquerel's phosphoroscope, the duration of fluorescence in solid thallene was found to be about $\frac{1}{500}$ of a second; but that of its solution was too brief to be appreciable even at the highest velocity which could be given to the instrument.

To exhibit the fluorescence of thallene, I have found the following methods most effective:—

1st. The electric light, without any lenses, is allowed to fall directly on it through a plate of blue or violet glass. In this way one can show various devices, filling an entire stage without difficulty. Allowing the light to pass through the condensing-lenses of a lantern greatly reduces the effect.

2nd. Burning magnesium wire behind a blue glass. The effect here also is very powerful.

3rd. Connecting in series a number of nitrogen-spectrum tubes and exciting them with a powerful coil. The great lack of general illuminating-power in the light so produced makes the effect in this case very striking.

A lime-light behind a blue glass will exhibit the fluorescence of thallene fairly well, but not that of petrollucene.

The violet end of a projected spectrum might be regarded as a good source of exciting light, but is, as a matter of fact, inferior to any of those before mentioned. Nevertheless the otherwise invisible bands of metallic spectra, up to the position of the group O, may be exhibited to the largest audiences by the use of a screen covered with thallene. In painting designs and screens with thallene, it is best to grind it with damar varnish and benzole. A glass goblet coated with thallene is well suited for Gassiot's electric cascade.

XI. *On the Form of the Cells of Bees.* *By* J. W. L. Glaisher, *B.A., Fellow of Trinity College, Cambridge*[*].

THE following account of the history of the problem of the bee-cell is extracted from pp. 428–430 of 'Homes without Hands,' by the Rev. J. G. Wood (London, 1869):—

"If a single cell be isolated, it will be seen that the sides rise from the outer edges of the three lozenges above mentioned; so that there are, of course, six sides, the transverse section of which gives a perfect hexagon. Many years ago Maraldi, being struck with the fact that the lozenge-shaped plates always had the same angles, took the trouble to measure them, and found that in each lozenge the large angles measured 109° 28′, and the smaller 70° 32′, the two together making 180°, the equivalent of two right angles. He also noted the fact that the apex of the three-sided cup was formed by the union of three of the greater angles.

"Some time afterwards, Réaumur, thinking that this remarkable uniformity of angle might have some connexion with the wonderful economy of space which is observable in the bee-comb, hit upon a very ingenious plan. Without mentioning his reasons for the question, he asked Kœnig, the mathematician, to make the following calculation:—Given a hexagonal vessel terminated by three lozenge-shaped plates, what are the angles which would give the greatest amount of space with the least amount of material? Kœnig made his calculations, and found that the angles were 109° 26′ and 70° 34′, almost precisely agreeing with the measurements of Maraldi. The reader is requested to remember these angles. Réaumur, on receiving the answer, concluded that the bee had very nearly solved the difficult mathematical problem, the difference between the measurement and the calculation being so small as to be practically negatived in the actual construction of so small an object as the bee-cell.

"Mathematicians were naturally delighted with the result of the investigation; for it showed how beautifully practical science could be aided by theoretical knowledge; and the construction of the bee-cell became a famous problem in the economy of nature. In comparison with the honey which the cell is intended to contain, the wax is a rare and costly substance, secreted in very small quantities, and requiring much time for its production; it is therefore essential that the quantity of wax employed in making the comb should be as little, and that of the honey contained in it as great as possible.

"For a long time these statements remained uncontroverted.

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[*] Communicated by the Author.

Any one with the proper instruments could measure the angles for himself; and the calculations of a mathematician like Kœnig would hardly be questioned. However, Maclaurin, the well-known Scotch mathematician, was not satisfied. The two results very nearly tallied with each other, but not quite; and he felt that in a mathematical question precision was a necessity. So he tried the whole question himself, and found Maraldi's measurements correct, namely 109° 28′ and 70° 32′.

"He then set to work at the problem which was worked out by Kœnig, and found that the true theoretical angles were 109° 28′ and 70° 32′, precisely corresponding with the actual measurement of the bee-cell.

"Another question now arose. How did this discrepancy occur? How could so excellent a mathematician as Kœnig make so grave a mistake? On investigation it was found that no blame attached to Kœnig, but that the error lay in the book of logarithms which he used. Thus a mistake in a mathematical work was accidentally discovered by measuring the angles of a bee-cell—*a mistake sufficiently great to have caused the loss of a ship whose captain happened to use a copy of the same logarithmic tables for calculating his longitude.*"

It is proper to add that Mr. Wood prefaces the above narrative with the words, "I must acknowledge my thanks to the Rev. Walter Mitchell, Vicar and Hospitaller of St. Bartholomew's Hospital, who has long exercised his well-known mathematical powers on this subject, and has kindly supplied me with the outline of the present history."

The last sentence in the first extract, viz. that about the logarithms (in which the italics are Mr. Wood's), induced me to examine the original memoirs that relate to the history of the problem of the form of the bee-cell; and although the story as given above is on the face of it erroneous, as the determination of the angles that occur in a bee-cell by measurement to the nearest *minute* is clearly impossible, still I was not prepared to find that the whole story narrowly escapes the verdict *tot sententiæ, quot mendacia*[*].

As, however, the manner in which so elaborate and romantic a fable has gradually grown up out of simple and even commonplace facts possesses interest, and as moreover most of the misstatements have received the sanction of previous writers, I think it will be found that the history of the matter is well deserving of attention, although the account quoted, occurring in a work of a popular character, would not, however glaring its inaccuracies, *per se* require refutation in a scientific journal.

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[*] It is only fair so far to anticipate matters as to remark at once that Mr. Wood (or Mr. Mitchell) is not chargeable with the origination of the false statements that occur in the narrative quoted, for most of which in the above shape Lord Brougham seems to be chiefly responsible; but the concluding paragraph about the logarithms (which will be seen to be totally without foundation), as far as I know, first makes its appearance in 'Homes without Hands.'

It is convenient to give at the outset, before entering into any details, a brief account of the facts that did occur, for comparison with that quoted above. Maraldi measured the angles of the rhombs and also those of the trapeziums*, and found that, as nearly as he could determine them, the larger angles in both cases were about 110°, and the smaller the supplement of this angle, viz. 70°; it naturally occurred to him to investigate mathematically what the angles must be on the supposition that those of the rhombs and trapeziums were exactly equal, and he found as a consequence of this hypothesis that the values in question were 109° 28′ and 70° 32′. These angles were not, therefore, the results of direct measurement, but were derived from the solution of a mathematical problem suggested by the actual measurements. Réaumur, suspecting that economy of wax was the reason for the angles being such as they were, explained the form of the cell to Kœnig, and (without mentioning what had been done by Maraldi) asked him to determine mathematically what the angles of the rhombs would be if for a given capacity the surface of the cell were made a minimum. Kœnig solved the problem, and gave as his result 109° 26′ and 70° 34′, differing by only 2′ from what Réaumur (erroneously) regarded as the angles absolutely measured by Maraldi. Kœnig accompanied his solution by some remarks to the effect that the question was one that required the methods of Newton and Leibnitz, so that the bees had solved a problem that was beyond the reach of the old geometry. Maclaurin showed that this was inaccurate, by giving a solution depending only on ordinary geometry; and he found the angles to be 109° 28′ and 70° 32′, agreeing with Maraldi's theoretical values. Maclaurin read Maraldi's memoir correctly, and understood the angles by measurement to be 110° and 70°; and he merely just notices the trifling error made by Kœnig in his calculation, regarding it as quite an unimportant matter.

These are all the facts on which Mr. Wood's narrative is founded; and it therefore follows (1) that Maraldi did not find the angles by measurement to be 109° 28′ and 70° 32′, (2) that Maclaurin was not led to consider the matter by observing the discrepancy between Maraldi's and Kœnig's values, (3) that he did not make any measurements himself, and (4) that the discovery of the error in the logarithmic table is a fiction, the method Kœnig employed never having transpired, so that it is quite as likely as not that he never used logarithms at all.

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* By the angles of the trapeziums in this paper are invariably meant the two angles where the sides of the cell are intersected by the rhombs; the angles at the mouth of the cell are, of course, right angles.

The sentence about the pleasure of the mathematicians is no doubt merely rhetorical; but few persons who have any knowledge at all of the history of science will be able to read without a smile the grave statement that, more than half a century after the publication of the *Principia*, mathematicians were *delighted* at their science having given a proof of its utility by the solution of a simple geometrical question.

Before entering into further detail with regard to the history of the problem of the bee-cell, which presents several other points besides those already mentioned, it is proper to state that the subject was treated, in very much the same way as in this paper, by the late Robert Leslie Ellis, in a short memoir which was first published after his death in the volume of his collected writings (edited by Walton, Cambridge, 1863), pp. 353–357. Ellis's remarks were called forth by Lord Brougham's essay on the subject (which will be noticed further on and seen to be very inaccurate); and he has pointed out that the values 109° 28′ and 70° 32′ were not the result of measurement; he has also corrected Lord Brougham on another point (viz. about Boscovich), which will be referred to presently. The former misconception Ellis speaks of as being "still current and repeated by every writer or nearly so on the subject;" and he refers to a further inaccuracy occurring in Dr. Carpenter's 'Physiology'*. As, however, Ellis's paper occupies only a small portion of the ground covered by the present communication, it will be convenient for the sake of completeness to give the whole account here as if it had not been written; but it must be understood that the portion relating to Maraldi is exhaustively treated by him†. Maraldi's memoir is entitled "Observations sur les Abeilles," and occupies pp. 299–334 of the Memoirs of the French Academy for 1712 (Paris, 1714)‡. The result of the measurements is given on p. 309 in the following words:—

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[*] The passage in question ('Human Physiology,' 3rd edit. 1856, p. 251) concludes with the words, "the *bees* being thus proved to be *right* and the *mathematician wrong*." Dr. Carpenter refers to Lord Brougham (*vide infrà*), from whose essay he takes the account, and who is responsible for all except the statement that he (Lord Brougham) took into account certain small quantities that had been previously neglected, and thus showed that the agreement of theory with observation was perfect.

[†] It should be mentioned that Ellis adds some remarks on the construction of the cells which are not connected with the subject of the present paper.

[‡] But there are different editions; in another edition (Paris, 1731 before me the memoir occupies pp. 297–331, and the spelling and accentuation are more modernized; the quotations are from pp. 307 and 309.

"Chaque base d'Alveole est formée par trois rombes presque toûjours égaux et semblables, qui suivant les mesures que nous avons prises, ont les deux angles obtus chacun de 110 degrés, et par consequent les deux aigus chacun de 70 degrés;" and on p. 312 the result of the equality of the angles of the rhombs and trapeziums is given: "Outre ces avantages qui viennent du côté de la figure de la base, il y en a encore qui dépendent de la quantité des angles des rombes; c'est de leur grandeur que dépend celle des angles dez trapezes, qui forment les six côtéz de l'Alveole; or on trouve que les angles aigus des rombes, étant de 70 degrés 32 minutes et les obtus de 109° 28 minutes, ceux des trapezes qui leur sont contigus, doivent être aussi de la même grandeur." The *étant*, as Ellis remarks, being the prothesis of a hypothetical proposition, *if* they are &c. *then* so and so; and in this way the mathematicians Maclaurin, Boscovich, and Lhuillier have read it. Maraldi was an able mathematician; and no doubt, after his previous statement of what his measurements gave, it no more occurred to him that a non-mathematical reader might imagine these angles were the result of more accurate measurements, than it did that there was any necessity to give the details of the solution of the geometrical problem involved.

Réaumur's researches are to be found on pp. 389-391 of t. v. of his *Mémoires pour servir à l'histoire des Insectes* (Paris, 1740). The following is the account of the proposition of the question to Kœnig:—"Convaincu que les abeilles employent le fond pyramidal qui mérite d'être préféré, j'ai soupçonné que la raison, ou une des raisons qui les avoit décidées, étoit l'épargne de la cire; qu'entre les cellules de même capacité et à fond pyramidal, celle qui pouvoit être faite avec moins de matiére ou de cire, étoit celle dont chaque rhombe avoit deux angles, chacun d'environ 110 degrès, et deux chacun d'environ 70. Sans parler de la grandeur de ces angles, après avoir fait admirer la disposition des rhombes à M. Kœnig .... je lui proposai de résoudre le probleme suivant. Entre toutes les cellules exagonales à fond pyramidal, composé de trois rhombes semblables et égaux, déterminer celle qui peut être construite avec le moins de matiére." Kœnig gave 109° 26′ and 70° 34′ as the angles of the rhombs; and then Réaumur sent him the 'Memoirs of the Academy' for 1712, and he (Kœnig) "was agreeably surprised" to find therefrom that his results only differed by 2′ from Maraldi's 109° 28′ and 70° 32′, which Réaumur speaks of as "les mesures les plus précises de ces angles." Réaumur unfortunately does not give Kœnig's investigation; but he mentions that it was presented to the Academy in 1739. The volume for this year contains no memoir by Kœnig (so that, if he presented one, it was not considered deserving of publication *in extenso*); but the matter is noticed at length in pp. 30–35 of the *Histoire* at the beginning, where, however, only a general account of the matter is given, and no further information about Kœnig's solution is added*. The historian of the Academy states that Réaumur also proposed the question to other mathematicians, from whom, however, he did not receive solutions; he speaks of the bees being only in error to the trifling amount of 2′, and adds that the "grande merveille" is their solution of a problem belonging to the higher geometry.

Maraldi proposed to himself the problem, Supposing the rhombs and trapeziums have equal angles, what are they? and Réaumur proposed to Kœnig the problem, What must the angles of the rhombs be, if the surface is a minimum? The answer to the latter question is, "Equal to the angles of the trapeziums;" so that both problems give the same values for the angles; only Maraldi worked his numerical calculation correctly, Kœnig incorrectly. What happened was that one mathematician was right and another wrong, not that the bees were right and the mathematicians wrong. How Kœnig, who had himself just been considering the mathematical problem of the bee-cell, could on reading Maraldi's memoir have failed to see that the latter's values were the result of a geometrical investigation of the same nature as that which he had just completed, and that they ought to have agreed exactly with his own, is perfectly unaccountable; but it seems to have been the fact.

Réaumur (t. v. p. 390) gives, on Kœnig's authority, the amount of wax saved as equal to the whole quantity that would be required for a hexagonal bottom ("M. Kœnig a pourtant démontré que les abeilles œconomisent la cire, en préférant les fonds pyramidaux aux fonds plats, qu'elles ménagent en entier la quantité de cire qui seroit nécessaire pour un fond plat"). This is incorrect; it will appear further on that the saving is between one fifth and one sixth of the amount here stated.

Maclaurin's paper, "Of the Bases of the Cells wherein the

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* Réaumur's words (t. v. p. 390) are: "Si je ne craignois qu'on se lassât de m'entendre parler géométrie, je rapporterois volontiers les démonstrations de M. Kœnig; mais ceux qui sont curieux de les voir, n'y perdront rien pour ne les pas trouver ici. Le mémoire qui les donne, a été lû à l'Académie en 1739, il en sera fait mention dans l'Histoire de cette même année; elles y seront exposées plus nettement, et mises dans un plus grand jour, par notre Célébre Historien, que je ne le pourrois faire." This implies that Kœnig's solution was presented *in extenso* to the Academy, and that Réaumur expected it would be so published. In point of fact, however, as already mentioned, not one word of the solution itself was printed; and all that can be inferred about it from Réaumur's and the historian's remarks (which latter are not remarkable for their profundity) is, that it depended on the differential calculus.

Bees deposit their Honey," appears in the Philosophical Transactions for 1743, pp. 565–571*. After an accurate† history of the question, he gives a geometrical proof of Kœnig's proposition to disprove the assertion that any "higher geometry than was known to the Antients" was necessary, and correctly finds the angles to be 109° 28′ 16″ and 72° 31′ 44″. He proceeds, "Though Monsieur Maraldi had found by his mensuration these obtuse angles to be of about 110 degrees, the small difference between this and the 109° 28′ 16″, just found by calculation, seems to have been either accidental, or owing to the difficulty of measuring such angles with exactness. Besides that he seems to admit the real equality of the several plane angles, that form as well the apex, as the other solid ones we have been treating of. And, as to the small difference between our angle and that determined by Mr. Koënig, who first considered this problem, but has not yet published his demonstration of it, that can only be owing to his not carrying on his computation so far, and would scarcely have been worth the mentioning, were it not yet in favour of the practice of these industrious little insects."

The whole of the above passage has been quoted, because otherwise the concluding words "were it not in favour &c." might give an erroneous impression of Maclaurin's meaning, which from the context is seen to be that the correct answer is 2′ 16″ nearer to the observed angle (110°) than was Kœnig's angle, a correction favourable to the bees. To a not very careful reader the words might at first sight seem to imply that the writer had vindicated the bees from their supposed error of two minutes. Maclaurin calculates the saving as "almost one fourth part $\left[\frac{\sqrt{3}-\sqrt{2}}{\sqrt{2}}\right]$ of the pains and expense of wax they bestow above what what was necessary for completing the parallel sides of the cells." This is quite correct; the amount mentioned is equal to $\frac{\sqrt{3}-\sqrt{2}}{\sqrt{3}} = \cdot 183\ldots$ of the wax required for a plane hexagonal floor. It is strange that Maclaurin did not so express it, and thus point out the important error made by Kœnig in stating it as equal to the whole floor.

Boscovich's investigations occur in the notes to Stay's poem‡.

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[*] The paper appears in full also in the Abridgement by Hutton, Shaw, and Pearson, vol. viii. pp. 709–713 (1809).

[†] "Monsieur Maraldi had found by mensuration that the obtuse angles of the rhombus's were of 110 degrees nearly; upon which he observed that if the three obtuse angles which formed the solid angles above mentioned were supposed equal to each other, they must each be 109° 28′; from whence it has been inferred that this last was really the true and just measure of them" (p. 566).

[‡] Philosophiæ recentioris a Benedicto Stay .... versibus traditæ

In the space of six pages and a quarter he has treated the matter very ably and in a manner worthy of his high reputation. He has clearly read with care all that had been previously published on the question, except Maclaurin's memoir, which he knew of and had in vain endeavoured to procure; he therefore does not know what method Maclaurin used, or whether any of his own remarks had been anticipated by him; but he thinks it likely that such may have happened, "cum in tanto Geometra nihil desiderari unquam possit, quod aut ad simplicitatem solutionum, aut ad penetrationem, et combinationes consectariorum pertineat," a well-deserved compliment to the Scotch mathematician.

Boscovich points out distinctly that Maraldi's values 109° 28′ and 70° 32′ were not the result of measurement; and he even goes further and expresses a doubt whether, without the knowledge of the solution of the mathematical problem, he would even have been able to measure them to the nearest degree. His own words are worth quoting:—"Mirum sane esset, si Maraldus ex observatione, angulum æstimasset intra minuta, quod in tam exigua mole fieri utique non poterat. At is ut satis patet ex ipsa ejus dissertatione, affirmat se invenisse angulos circiter graduum 110 et 70, nec minuta eruit ex observatione, sed ex æqualitate angulorum pertinentium ad rhombos, et ad trapezia; ad quam habendam Geometria ipsum docuit requiri illa minuta. Quin immo satis vereor, ne hæc ipsa, aut aliqua ejusmodi consideratio dederit ipsum graduum numerum illum tam proximum; nam observatio intra plures gradus incerta esse debuit in mole adeo exigua, nec ita accuratè efformata; cum plures irregularitates, et in hisce rhombis, et in omni reliqua constructione alveolorum observari passim affirmet ipse Reaumurius.

"Verum quidquid sit de graduum numero, certe numerum minutorum Maraldus non proponit tanquam observatione immediata definitum, sed ex illa æqualitatis consideratione, quam diximus, adeoque si Koenigii solutio rite procederet, determinatio minimi non ab angulo observato per illa duo minuta discederet, sed ab angulo requisito ad ejusmodi æqualitatem, cum observato tamen congrueret adhuc intra limites admodum arctos, et qui in ejusmodi argumento omnem superant observatoris industriam."

Réaumur himself (t. v. p. 382) says that some of the rhombs appear square, but that generally they are "lozanges ou rhombes plus ou moins allongés;" and any one who remarks how greatly

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libri x. cum adnotationibus, et supplementis P. Rogerii Josephi Boscovich, S.J. vol. ii. Rome, 1760, 8vo. The note *De Apium Cellulis* refers to verse 2306 of book vi., and is the last note in the second volume (pp. 498–504). The first volume was published in 1755, and, according to the *Biographie Universelle* (Michaud), the third in 1792, the publication having been retarded by Boscovich's delay in the preparation of the notes.

they differ from one another will agree with Boscovich in his surprise that Maraldi should have been able to obtain by measurement the typical angle correctly to within one ninetieth of a right angle. Of course Boscovich does not impute the slightest insincerity to Maraldi; he merely points out that the knowledge of the solution of the theoretical question may have exercised an unconscious bias over his mind, so as to make him announce 110° with more confidence than he might otherwise have felt.

Boscovich gives two solutions of Kœnig's problems, the one by pure geometry, the other by differential calculus. The latter is obtained in exactly the way any one to whom the question was proposed would probably now proceed (at all events it is nearly identical with my own solution undertaken before I had seen any of the investigations on the matter): the correct result is obtained; and it is suggested that Kœnig's error was produced by his having adopted some complicated method leading to a formula which had to be solved by approximation. With regard to the saving of the wax the process is quite correct; only there is a slip in the course of the work which renders the result inaccurate: the ratio of the amount saved to that required to form a hexagonal base is given as $\frac{\sqrt{6}-2}{2}$, whereas it should be $\frac{3-\sqrt{6}}{3}$. The ratio in question is truly found to be $\frac{2\sqrt{3}-2\sqrt{2}}{2\sqrt{3}}$; but in the next line this is by a mistake written $\frac{\sqrt{3}-\sqrt{2}}{\sqrt{2}}$, and the error is retained. This upsets an ingenious suggestion of Boscovich's, who thought that perhaps Kœnig's error was introduced by the accidental substitution of $\sqrt{6-2}$ for $\sqrt{6}-2$ in the numerator of the fraction quoted, which would change the ratio into one of equality*.

One point of very considerable importance in the form of the cell is first noticed by Boscovich, who observes that by the arrangement adopted every plane cuts every other plane at an angle of 120°: thus the three rhombs forming the apex are inclined to one another at this angle, and so is each trapezium to the planes it cuts. This leads to the belief that the bees have some means (instrumenta) of constructing planes inclined to one another at this angle. Ellis (who had not seen what Boscovich had written) practically remarked the same thing, and made a guess at what the *instrumenta* were a century later ('Writings,' p. 356). It thus appears that Boscovich discussed the whole subject with completeness, penetration, and (but for the one trivial slip) accuracy. Had his remarks been published in a work better known and more accessible to naturalists, a detailed refutation of Réaumur and Kœnig a hundred and thirteen years later would have been rendered superfluous.

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* It is scarcely worth speculating how it was that Kœnig did make the error in determining the ratio; and, of course, the facts can never be known with certainty; but the following is a guess. Taking the length of a side of the hexagonal base as unity, the amount of the saving is $\frac{3\sqrt{2}}{4}+\frac{3\sqrt{3}}{2}-\frac{9\sqrt{2}}{4}$ (the first term being the area of the six triangles, the second of the hexagonal base, and the third of the three rhombs); if, then, Kœnig forgot to multiply the area of a single rhomb, viz. $\frac{3\sqrt{2}}{4}$, by 3, the expression would reduce to the middle term, viz. the area of the hexagonal base.

The next memoir* that I know of in which the form of the bee-cell is considered is by Lhuillier, and occupies pp. 277–300 of the Berlin Memoirs for 1781 (published 1783). The introduction (of two pages) and conclusion were written by Castillon, who presented the memoir to the Academy. Lhuillier gives a short account of the history of the subject without adverting particularly to Réaumur's misreading of Maraldi. He argues with Boscovich "que l'égalité supposée des angles des rhombes du fond et de ceux des trapezes des faces d'un alvéole, est le principe qui a guidé M. Maraldi dans l'estimation de ces angles;" and adds a new fact, viz. that Cramer, his fellow-citizen (of Geneva) had given some developments on the subject to Kœnig which had not been published. After stating that one of Boscovich's methods is very similar to Maclaurin's, he begins the next paragraph: "Tous les mathématiciens ont regardé cette matière comme passant les forces de la Géométrie élémentaire," which is untrue, as the solutions in question are geometrical: the error is a curious one for Lhuillier to have made, as he had certainly read Boscovich's remarks, and in all probability seen Maclaurin's paper. He very properly regards the difference of method as of very slight importance in a mathematical point of view (and thereby he shows, I think, a truer appreciation of the relations between the modern and ancient methods than either Kœnig or the historian of the French Academy); and he justifies his own geometrical solution that follows on the ground that it is thus made intelligible to naturalists; for although it is unlikely that the same person should be both an able mathematician and an able naturalist, still a particular study of the one does not prevent a knowledge of the elements of the other. After his geometrical solution he proceeds to calculate the saving of wax; and he finds that the amount used by the bee in the real cell is to the amount that would be required for the prismatic cell with a plane hexagonal floor as $25+\sqrt{6}$ to 28, so that the wax saved is about the one fifth part of that required for the plane floor. Lhuillier merely proves the above value for the ratio; but it is interesting to have the amount of wax required for the different portions of the cells ready for comparison. Taking, therefore, a side of the hexagonal section as unity, and assuming with Lhuillier (after Maraldi's observations) that the ratio of the radius of the inscribed circle of the hexagon to the depth of the prismatic cell of the same capacity as the real cell is as $1\frac{1}{3}$ to 5, we have for this depth (viz. the longest side of the trapeziums in the real cell) the value $\frac{25\sqrt{3}}{12}$, and then to three places of decimals—

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* "Mémoire sur le minimum de cire des alvéoles des Abeilles et en particulier sur un minimum minimorum relatif à cette matière."

Area of the three rhombs . . . $=\frac{9\sqrt{2}}{4} = 3{\cdot}182$, (i)

„ six triangles . . . . $=\frac{3\sqrt{2}}{4} = 1{\cdot}061$, (ii)

„ six sides of equivalent prismatic cell . . $=\frac{25\sqrt{3}}{2} = 21{\cdot}651$, (iii)

„ hexagonal base . . . $=\frac{3\sqrt{3}}{2} = 2{\cdot}598$, (iv)

and the surface of the real cell = (i) + (iii) − (ii) = 23·772, while that of the prismatic cell with a flat hexagonal bottom = (iii) + (iv) = 24·249; so that the saving is ·477, and the ratio is as stated above.

It forms no part of my present purpose to give a detailed account of the form of Lhuillier's *minimum minimorum* cell; suffice it to say that he extends Réaumur's problem, and proceeds to inquire what must be the proportion of the depth of the cell to the width of its mouth that it may require the minimum of wax (the *shape* of the cell, viz. the inclination of the rhombs to the trapeziums and to one another, being supposed as in the bee-cell). A few remarks on this paper will be made further on.

Lhuillier refers to Lambert (*Beyträge zum Gebrauche der Mathematik,* t. iii. p. 387 *et seq.* Berlin, 1772); but I find on reference there is nothing that very closely relates to the subject of this paper. Lambert's remarks occur in the course of an essay on the Art of Building; and he prefaces chap. v. (on minima in roofs) with the statement that if it were usual to build hexagonal houses we should obtain our model from the bees. In point of fact hexagonal summer-houses, sentry-boxes, towers, &c. are not very uncommon; so that he investigates the condition of roofs of minimum surface both in this case and when the base is square. As the result of the former problem he obtains the bee-cell; and when the base is square the roof is formed of four rhombs, which happen to be of the same form as in the bee-cell (viz. ratio of diagonals $= \sqrt{2}$), with the only difference that when the base is square they are placed with the shorter diagonals horizontal, and when the base is hexagonal the longer. He thence concludes that with twelve such rhombs a regular figure of twelve sides could be made exhibiting both the roofs of the 4- and 6-angled buildings, and therefore also of the bee-cell; after which he adds that this last fact was noted by Kepler in his *Harmonice,* so that the question Réaumur proposed to Kœnig was not so new as he thought (nicht mehr so ganz neu, als es Reaumur geglaubt zu haben scheint). Lhuillier says Lambert's assertion about the bee-cell is contrary to the contents of his (Lhuillier's) paper (meaning that certain other forms would be preferable for the house); and he is right. In fact Lambert has not at all considered the general problem, what would be the best form for the hexagonal house? but merely the particular case which arises in connexion with the bee-cell, viz., if the base and capacity are given and the roof is to be rhomboidal, what must the angles of the rhombs be?

The next discussion of the form of the bee's cell that I have to notice is by Lord Brougham; and it occurs in vol. i. of his 'Dissertations on subjects of Science connected with Natural Theology, being the concluding volumes of the new edition of Paley's Works,' two vols. 1839*. The essay in question occupies pp. 218–368; but it is only with the parts of it that relate to the matters already noticed in this communication that we are here concerned, and it must be understood that it is only to these portions that any remarks that may be made have reference, unless otherwise stated.

In 1858 Lord Brougham extracted the more salient points from his earlier essay and communicated them to the French Academy†. I cannot find that his memoir has yet been printed by the Academy; but an abstract appears on p. 1024 *et seq.* of vol. xlvi. (1858) of the *Comptes Rendus*; and it is published *in extenso* by its author as Tract vi. (pp. 103–121) of 'Tracts, Mathematical and Physical' (London and Glasgow, 1860). All the statements that will be noticed in this paper appear also in the French memoir, although the wording &c. is sometimes modified and the whole contracted. I shall refer to and quote from the original edition of 1839; but wherever the French memoir of 1858 shows any discrepancy it will be pointed out either in the text or a note. As a *résumé* of moderate length was given in the *Comptes Rendus* for 1858, it seemed not unlikely that some one might have anticipated the ensuing remarks; but an examination of the *Comptes Rendus* from 1858 to 1871 showed that only one communication had in this time been made to the Academy on the subject, viz. by the late Mr. Willich (vol. li. p. 633, 1860), who gives a method of constructing a bee-cell which resembles (and is perhaps identical with) that suggested by Leslie Ellis in his Essay.

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* The essay appears in vol. vi. of the collected works of Lord Brougham, by A. and C. Black, Edinburgh; but the *Appendix of Demonstrations*, containing the solutions of the mathematical questions referred to in the text, is left out; and this is done without even an accompanying statement of so important an omission having been made.

† It is not, however, stated in the memoir (at all events as printed in the 'Tracts' .... 1860) that the facts contained in it had been previously published.

I may as well mention at once that Lord Brougham's essay shows that he has read carefully Réaumur and Lhuillier, looked at Maraldi and Maclaurin, and not seen Boscovich; he has, however, written with great confidence, and on him falls the responsibility of perpetuating and stamping with a fresh mark of authority the old fiction that had been exposed three quarters of a century before. His works have been so widely read, and will no doubt continue to be so well known in the future, that he has in all probability given a new and long lease of life to the old and silly fable: his statements appear on the face of them to be the results of such careful study of the original authorities, and they are presented with such assurance, that few readers will imagine that they can be otherwise than correct. The story, as given by him, is in its facts the same as it appears in the extract from Mr. Wood's work at the beginning of this paper* (omitting the paragraph about the logarithms); only he does not state that Maclaurin made any measurements; in fact he expressly points out in a note on p. 244 that Dr. Reid, in his Essays†, is wrong in stating that Maclaurin ascertained the angles "by the most exact mensuration the subject would admit," or solved the problem "by a fluxionary calculus."

In reference to Kœnig's blunder of 2′, Lord Brougham says (p. 244), "possibly it is in the logarithms that he has, by neglecting some decimal places, gone wrong;" and on p. 345, "the error into which Mr. Kœnig fell . . . . . originated most probably in the tables of sines or in the logarithms which he used*. Now to both of these suggestions, and to the second especially, there are great objections. Kœnig gave a wrong result as the answer to an arithmetical question; and all that can be said is that he somewhere must have made a mistake in his work: if he neglected too many decimal places, that is of course a blunder as much as if he had written a 5 for a 3; and, as far as I can see, there is no reason to assume that he made this error rather than any other. The second suggestion, so far being most probable, is possible, but highly improbable. Given that a wrong numerical result is published and nothing further, it is 50 to 1 that the blunder was in the computations, not in the tables; besides, Kœnig calculated the saving of wax† (which required no tables or numerical computations) wrongly, so that there is *prima facie* evidence that he did not use sufficient care. Lord Brougham was especially afraid of any error in the tables, and therefore obtained two solutions of Kœnig's problem, in one of which the dihedral angle between two of the rhombs was the quæsitum; and he also desired a friend to investigate the question independently. But all this was quite superfluous; if θ be the smaller angle of the rhombs, the analysis readily gives $\cos\theta=\frac{1}{3}=\cdot3333333$, so that θ can be obtained at once from a table of natural sines; and any one who knows any thing at all about mathematical tables can see at once, by taking a few differences, whether the table contains an error (particularly so large an error as to produce an alteration of $2'$); but even if this is not done, an obvious transformation gives

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* In a note in the French memoir a more detailed allusion is made to Maraldi's measures, and the 110° and 70° are regarded as approximate and the 109° 28′ and 70° 32′ as exact: this was Réaumur's view.

† The passage referred to will be found in chap. ii. (Instinct) of Essay III. of Dr. Thomas Reid's 'Essays on the Active Powers of Man' (orig. ed. 1788); but no history of the problem of the bee-cell is there given; in fact the only sentence that has reference to the matter is the one containing the two errors noticed in the text; and it simply states that the bees have solved the *minimum* question correctly.

$$\tan\tfrac{1}{2}\theta=\sqrt{2}=1\cdot4142136,$$

and $\frac{1}{2}\theta$ (θ being here the larger angle) can be found directly either from a natural or a logarithmic canon at once; or any number of similar transformations can be made. Lord Brougham uses (p. 246) the singular phrase "difference introduced by the logarithmic approximations;" perhaps the last two words are merely a euphemism for "erroneous table," as it is needless to say that 7-figure logarithms generally give results true to a tenth of a second at least, so that an error of two minutes is out of the question.

Whence Mr. Wood or Mr. Mitchell derived the statement that Kœnig was not to blame, but that the error really was in in the logarithms, I am unable to say; but I think it is a fair hypothesis to suggest that Lord Brougham's, or some similar statement to the effect that the error was most likely in the

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[*] This is intensified in the French memoir, "M'étant assuré . . . . que Kœnig était tombé dans l'erreur par les tables de sinus ou des logarithmes &c.," p. 112.

[†] Lord Brougham does not mention the fact of Kœnig having erroneously calculated the saving.

tables, has been developed into it by persons not very careful about the truth. The story given by Lord Brougham was sufficiently romantic; but the person who added the climax, and gave it point by the sensational allusion to the ship, certainly deserves to have his services remembered.

Maclaurin suggested that Kœnig did not carry his approximations far enough; but I imagine that he either felt a delicacy about saying in so many words that Kœnig had made a blunder, or that he had in his mind (like Boscovich) the probability of his having obtained a complicated equation and solved it by approximation. But Lord Brougham could not have felt this delicacy a century later; and he certainly did not attach Boscovich's meaning to the word *approximations*.

Lord Brougham objects most strongly to the results obtained by Lhuillier; and his arguments are of so remarkable a character that I hope I shall be excused for quoting his exact words, and the more so as the reasoning certainly must have appeared to its author to carry weight, as it is reproduced (although somewhat more briefly) in the French memoir.

He denies that Lhuillier is right in calculating the saving of wax as only $\frac{1}{51}$; he writes (p. 291):—"It is extremely erroneous to represent the saving as only $\frac{1}{51}$ part. . . . The proportion $\frac{1}{51}$ is obtained by comparing the saving upon the base with the whole wax of the cell, including the walls, and supposing the height of the wall to be to the sides as 5 to 1·387. But why is the wax of the wall to be imported into the calculation, with which it has nothing to do? The question is between two forms of the bottom, not of the whole cell. Suppose two kinds of roof for a house were to be compared in order to choose the one that required least timber; though the whole house might be made of wood, we should only compare the expense of the roofs, and leave out the walls that would be common to both plans; otherwise the relative amount of the saving would depend on the height of the house as well as the shape of the roof. This becomes the more evident in the case of the cells from the circumstance of their depth varying in the same comb, and for the same bee, according to many accidental circumstances. . . . [The author then states the great variations that occur in the depth of cells.] The saving therefore is somewhere about a ninth, and not somewhat less than a fifty-first part."

"But there is another consideration which shows still more strikingly the fallacy of the argument derived from taking the whole walls of the cell into calculation. The thickness of the wax is very different in different parts of the cell, being much greater in the base, that is, in the rhomboidal plates, and the part of the walls adjoining, the six small triangles, which are formed by a line drawn parallel to the base through the points where the rhomboidal plates cut the walls. This is manifest upon inspection; and I have tried it by weighing equal parts in superficial extent, as far as it was possible, of the base and of the sides, and uniformly found the latter sensibly lighter. It did not seem that the proportion was always the same; but I never found the difference less than in the proportion of 3 to 2. The thickness of the walls varies much more than that of the base in different combs. But any considerable difference between the two portions at once destroys the argument of M. L'Huillier. If it is as 3 to 2, then the saving is nearly an eighth upon the thicker part, and consequently about $\frac{1}{35}$ instead of $\frac{1}{51}$ of the whole."

The ratio of one ninth is found by comparing the bottoms of the cells, the bottom of the actual cell being taken as the three rhombs and six triangles, and the bottom of the hypothetical prismatic cell as the hexagonal floor and the portions of the sides below a horizontal plane through highest points of the rhombs (supposing the cell placed with its axis vertical and mouth upwards); and the ratio of $\frac{1}{35}$ follows by considering the ratio of the saving to the whole amount of wax, the bottom (defined as above) being supposed half as thick again as the rest of the cell.

The above quotations contain, I venture to think, as striking instances of bad reasoning as are often met with in writings relating to mathematical subjects. The comparison is between two kinds of cells; the one requires an amount n of wax, the other an amount $n+\frac{1}{50}n$; and the amount saved is $\frac{1}{50}n$, that is to say, if they built flat-bottomed cells, the bees would have to manufacture $\frac{1}{50}$ more wax than in reality they do manufacture. If a house with a V-roof costs £1000, and with a flat roof £1020, the builder who prefers the former saves £20 out of an expenditure of £1000, or $\frac{1}{50}$ of what he spends*; his saving is not a fifth *of his expenditure* because the walls cost £900, and are the same for both. Lord Brougham says, "Why is the wax of the wall to be imported into the calculation, with which it has nothing to do?" and the reply is, "Why is the wax of the wall to be left out of the calculation? it is manufactured by the bees in exactly the same way as is the wax for the rhombs and triangles, and is part of the cell built by the bees." In effect, Lord Brougham replaces "the bees must have walls for their cells" by "the bees do not manufacture the wax for the walls of their cells," and regards the two assertions as identical. The fact that the depth of the cells is variable is exactly analogous to the fact that the angles of the rhombs vary: none of the cells are perfect mathematical figures; but we consider merely the figure which we believe to represent the mean or typical cell.

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* Or, to state the same thing differently, if m be the wax required for the hypothetical cell, $m-\frac{1}{51}m$ represents the amount used in the real cell, so that the saving is $\frac{1}{51}$ of the amount that would have been needed for the hypothetical cell; it is because Lhuillier so regarded it that he writes $\frac{1}{51}$ and not $\frac{1}{50}$.

In reality Lhuillier's calculation of the saving is under the mark; for if, as Lord Brougham so strongly insists in reference to the *minimum minimorum*, the hexagonal plate that closes the mouth of the cell be taken into account also, then the amount of wax in the real cell is 26·370 against 26·847 in the hypothetical cell, and the saving is only $\frac{1}{55}$ of the wax used, or even less if we consider a comb, as each cell has its own hexagonal plate, while the sides and rhombs are, except in the outside cells, common to two contiguous cells.

With regard to the argument drawn from the increased thickness of the rhombs and triangles, it is only necessary to remark that, unless we know the reason for this thickening (which we do not), its recognition removes the question of the bee-cell from the province of mathematics (or exact reasoning) entirely. We are comparing the real cell that the bees do make with a hypothetical cell which, *à priori* might have appeared more simple and suitable; and unless we are sure why the bottom (using the word as above defined) is thicker than the walls, how can we know how much of the hypothetical cell would require to have been thickened if it had been adopted? If the change of thickness destroys Lhuillier's argument, it at the same time destroys all mathematical reasoning as applied to the question. Lord Brougham assumes that what I have called the bottom of the hypothetical cell would have needed thickening had that form been adopted; but this assumption is purely arbitrary, and how much (if any) of the walls would have required to have been thickened is merely matter of opinion; and the question is thereby removed out of the reach of mathematics, where it is not my object to follow it.

I purposely abstain from any remarks on Lhuillier's *minimum minimorum* and Lord Brougham's objections, partly because I may possibly make the problem there treated of the subject of a separate communication at some future time, and partly because so shallow a cell (whether the hexagonal plate be excluded or not) is clearly unsuitable for the other purposes to which the bees apply their cells besides storing honey. It is unnecessary here to do more than allude to Lord Brougham's remarks on Lhuillier and Castillon in reply to the argument marked *thirdly* in his essay, as any one who has read in this paper what Maraldi, Boscovich * &c.

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* Leslie Ellis corrects Lord Brougham for representing that Bosco-

did say will see at a glance how much is and how much is not correct. I pass over also one or two minor slips. Of course Lhuillier deserves Lord Brougham's censure for having failed to notice that Maclaurin's and Boscovich's proofs were geometrical.

I have not hesitated to state plainly the value of Lord Brougham's arguments in my opinion, as the frankness with which he has expressed his sentiments with regard to the writings of those with whom he did not agree renders any particular display of courtesy towards his own views unnecessary. Even Leslie Ellis writes, "Either the habit of an advocate's mind—for Lord Brougham may be regarded as counsel for the bees—or his not having read Maraldi's paper must have been the cause of this omission" [viz. of Maraldi's statement that the angles measured were 110° and 70°: this is partly supplied, as has been noticed, in the French memoir]; and I must say that the quiet dignity of the mathematical writings on the subject contrasts most favourably with his violent advocacy. The impression any one would receive from reading Lord Brougham's remarks on Lhuillier and Castillon would be that they had treated the matter ignorantly and unfairly; whereas their memoir seems to me (and I believe it would to any one else) an impartial (though not very brilliant) attempt to add to our knowledge and advance truth. As the result of a tolerably careful examination of the whole question, I may be permitted to say that I agree with Lhuillier in believing that the economy of wax has played a very subordinate part in the determination of the form of the cell; in fact I should not be surprised if it were acknowledged hereafter that the form of the cell had been determined by other considerations, into which the saving of wax did not enter (that is to say, did not enter sensibly; of course I do not mean that the amount of wax required was a matter of absolute indifference to the bees). The fact of all the dihedral angles being 120° is, it is not unlikely, the cause that determined the form of the cell.

Whenever a prediction is subsequently verified by experiment, there is always a strong tendency to believe in the truth of the theory whereby the prediction was obtained, even if the course of subsequent research lends no additional support to it, but rather the reverse. And the equality of the angles of the rhombs and trapeziums is no doubt the solution of so many different

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vich's remarks imputed dishonesty to Maraldi; and although he had not seen them, he feels no doubt that what Boscovich really did say was merely a reproduction of what Maraldi had himself said. This is so, and Ellis was right; but, curiously enough, although Lord Brougham did not know it and had no ground for supposing it, Boscovich does, as noticed *supra*, suggest that perhaps Maraldi was helped by theory.

questions, that the success of Réaumur's problem no more proved the economy theory than did the discovery of conical refraction prove Fresnel's wave theory. No one doubts that the bee-cell is the best that could be contrived for the purposes to which the bees apply it; but the question is, what is the special reason for the three rhombs and their particular form? Lhuillier's *minimum minimorum* is of little use, as he himself points out; for no doubt the shape of the insect &c. require a cell of some depth (and perhaps an apex); but his conclusion that economy is not the primary reason is, I think, in accord with the evidence.

In an interesting memoir in the Cambridge Transactions*, Whewell has drawn special attention to the manner in which hypotheses may become gradually transformed as new facts are discovered, so as sometimes to become almost the opposite of what they were originally, without their adherents acknowledging any defeat. Thus the Cartesians, little by little, modified their vortices by rejecting or altering portions now and then as they were shown to be in opposition to the facts, introducing fresh suppositions, &c., until at length a vortex merely became a complicated piece of machinery for producing a central force, and the Cartesians and Newtonians then ceased to differ, as the vortex, being no longer essential, could be ignored at pleasure—and this without the former ever yielding or formally confessing defeat. Now the problem of the bee-cell seems very like an hypothesis undergoing transformation. Réaumur (though he only speaks of it as the reason, or one of the reasons) and Maclaurin evidently thought saving of wax paramount; Boscovich gave prominence to other reasons also; and Lhuillier believed economy played only a very subordinate part. Lord Brougham vehemently supports the economy hypothesis, but says it is only one reason among several; and he ridicules Lhuillier for thinking that Réaumur &c. asserted it to be the *only* reason. This is the first step in the transformation; and it is quite possible the economy-theorists may in the future, while still contending for their hypothesis, gradually admit that more and more weight is due to the other reasons, till at length they may be contented with an acknowledgment that economy was not ignored—when the transformation will be effected.

I think the bee-cell problem is of sufficient importance, as one of the most remarkable instances of instinct, to merit the space that has been devoted to it. It seems curious that so obvious and easily exposed a story should have been so tenacious of life; but the explanation no doubt is to be sought in the fact that it lies on the borders of two sciences. In a mathematical point of view the problem is simple, not to say trivial, and possesses but slight interest to a mathematician; and to the naturalist it is too mathematical to be pleasing. A mathematician cannot sometimes help feeling some regret that the great discoveries and advances in his science can be only known to the very few who have learnt to read the language that can alone express them; but from the contents of this communication more than one argument might be drawn to show that it is any thing but an unmixed gain to a science to admit of popularization.

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* "On the Transformation of Hypotheses in the History of Science," vol. ix. part ii.

Cambridge, June 10, 1873.

XII. *On the Integration of the Accurate Equation representing the Transmission in one Direction of Sound through Air, deduced on the Ordinary Theory.* *By* Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge**.

THE following investigation, independently of its bearing on the problem to which it ostensibly relates, I offer as an essay towards a complete theory of the solution of linear partial differential equations of the second order.

The transmission of sound through air in one direction may be represented on the ordinary theory by the single equation

$$0=\frac{d^2\alpha}{dt^2}-\frac{a^2}{\alpha_x^2}\frac{d^2\alpha}{dx^2}, \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where α and x respectively denote the ordinate of the particle at the time t and in the state of rest, and where $\alpha_x=\frac{d\alpha}{dx}$—or by the pair of equations,

$$\left.\begin{aligned}0&=\frac{dv}{dt}-\frac{a^2}{\alpha_x^2}\frac{d\alpha_x}{dx},\\ 0&=\frac{dv}{dx}-\frac{d\alpha_x}{dt},\end{aligned}\right\} \qquad . \quad . \quad . \quad . \quad . \quad (2)$$

where v and α_x are no longer to be regarded as the partial differential coefficients of α, but as independent functions of x and t.

The solution of this last pair of equations must consist of a pair of relations between the variables $v\alpha_x x$ and t, such as

$$\mathrm{F}_1(xtv\alpha_x)=0, \quad \mathrm{F}_2(xtv\alpha_x)=0; \quad . \quad . \quad . \quad . \quad (3)$$

from which it is evident that (1) must be derivable from a pair of first integrals of the form indicated by equations (3) when we substitute in them $\frac{d\alpha}{dt}$, $\frac{d\alpha}{dx}$ for v and α_x respectively.

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* Communicated by the Author.

I. Supposing (1) to be derivable from one only of equations (3)*, as, for instance, the first, the form of F_1 can readily be determined. For, differentiating $F_1=0$ with respect to t and x separately, we get

$$\left.\begin{aligned} 0&=F'_1(t)+F'_1(v)\frac{d^2\alpha}{dt^2}+F'_1(\alpha_x)\frac{d^2\alpha}{dx\,dt},\\ 0&=F'_1(x)+F'_1(v)\frac{d^2\alpha}{dx\,dt}+F'_1(\alpha_x)\frac{d^2\alpha}{dx^2},\end{aligned}\right\}\quad .\ .\ (4)$$

the elimination of $\frac{d^2\alpha}{dx\,dt}$ between which gives

$$0=F'_1(v)F'_1(t)-F'_1(\alpha_x)F'_1(x)+\overline{F'_1(v)}^2\frac{d^2\alpha}{dt^2}-\overline{F'_1(\alpha_x)}^2\frac{d^2\alpha}{dx^2};$$

and in order that this may coincide with (1), we must have

$$\frac{\overline{F'_1(\alpha_x)}^2}{F'_1(v)}=\frac{a^2}{\alpha_x^2},$$

$$F'_1(v)F'_1(t)-F'_1(\alpha_x)F'_1(x)=0.\quad .\ .\ .\ (5)$$

The first of these gives us

$$\frac{F'_1(\alpha_x)}{F'_1(v)}=\pm\frac{a}{\alpha_x},$$

or

$$0=F'_1(\alpha_x)\mp\frac{a}{\alpha_x}F'_1(v);\quad .\ .\ .\ .\ (6)$$

the auxiliary equations for the integration of which by Lagrange's method are

$$0=dv\pm\frac{a}{\alpha_x}d\,.\alpha_x,\quad 0=dx,\quad 0=dt;$$

whence we have

$$\begin{aligned} v\pm a\log_e\alpha_x&=\text{const.},\\ x&=\text{const.},\\ t&=\text{const.};\end{aligned}$$

and therefore

$$\begin{aligned} F_1(txv\alpha_x)&=\phi\{(v\pm a\log_e\alpha_x),x,t\}\\ &=\phi\{\omega,x,t\},\end{aligned}$$

suppose, where $\omega=v\pm a\log_e\alpha_x$.

But this value of F_1 must satisfy (5); hence, since

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* This is, of course, not a necessary supposition. It will obviously be sufficient if (1) can be derived from any combination of the pair of equations constituting the solution and their derivatives.

$$F'_1(v) = \phi'(\omega),$$
$$F'_1(\alpha_x) = \pm \frac{a}{\alpha_x} \phi'(\omega),$$
$$F'_1(t) = \phi'(t),$$
$$F'_1(x) = \phi'(x),$$

we must have

$$0 = \phi'(\omega) . \left\{ \phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) \right\};$$

i. e. we must have either $\phi'(\omega) = 0$, or $\phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) = 0$.

The first of these would give us

$$F_1(txv\alpha_x) = \phi(x, t),$$

which is obviously an inadmissible result. Adopting, therefore, the alternative hypothesis, or

$$\phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) = 0,$$

and integrating this equation by Lagrange's method, the auxiliary equations for which in this case are

$$0 = dx \pm \frac{a}{\alpha_x} dt, \quad 0 = dv, \quad 0 = d\alpha_x,$$

we shall get

$$\phi = \phi \left\{ \left(x \pm \frac{a}{\alpha_x} t \right), v, \alpha_x \right\}.$$

But we have already seen that ϕ is of the form shown by the equation

$$\phi = \phi\{\omega, x, t\};$$

and these forms of ϕ are incompatible except on the supposition that $\phi'(t) = \phi'(x) = 0$—in other words, unless we have

$$F_1(txv\alpha_x) = \phi(\omega).$$

Therefore the first of equations (3) becomes

$$\phi(\omega) = 0,$$

or, which amounts to the same thing,

$$\omega = 0,$$

or

$$v \pm a \log_e \alpha_x = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When (7) holds, substituting in (1) we get

$$0 = \frac{d\alpha_x}{dt} \pm \frac{a}{\alpha_x} \frac{d\alpha_x}{dx},$$

which, integrated by Lagrange's method, gives

$$\alpha_x=\psi\left\{x\mp\frac{a}{\alpha_x}t\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Equations (7) and (8) together constitute a solution of (1) which, making allowance for the difference of the independent variables employed, is identical with that given by Poisson in the *Journal de l'École Polytechnique* for 1807*.

II. The foregoing result, it will be remembered, has been derived on the assumption that (1) is derivable from a single one of the equations (3) and its derivatives. I now propose to investigate the forms which F_1 and F_2 must assume in order that (1) may be derivable from a combination of equations (3) and their derivatives.

The differentiation of $F_2=0$ gives us

$$\left.\begin{aligned} 0&=F'_2(t)+F'_2(v)\frac{d^2\alpha}{dt^2}+F'_2(\alpha_x)\frac{d^2\alpha}{dx\,dt},\\ 0&=F'_2(x)+F'_2(v)\frac{d^2\alpha}{dx\,dt}+F'_2(\alpha_x)\frac{d^2\alpha}{dx^2}.\end{aligned}\right\} \quad . \quad . \quad (9)$$

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* When the motions are small the result is altogether different; for in that case (6) becomes

$$0=F'_1(\alpha_x)\mp aF'_1(v),$$

which, the auxiliaries for its integration being

$$0=dv\pm a\,d\alpha_x, \quad 0=dx, \quad 0=dt,$$

gives us

$$F_1=\phi\{\bar{\omega}, x, t\},$$

where $\bar{\omega}=v\pm a\alpha_x$. Hence we have

$$F_1'(v)=\phi'(\bar{\omega}), \qquad F_1'(t)=\phi'(t),$$
$$F_1'(\alpha_x)=\pm a\phi'(\bar{\omega}), \quad F_1'(x)=\phi'(x);$$

and substituting these values, (5) becomes

$$0=\phi'(t)\mp a\phi'(x).$$

The auxiliaries for the integration of this last are

$$0=dx\pm a\,dt, \quad 0=d\bar{\omega},$$

which give us

$$\psi=\phi\{(v\pm a\alpha_x), (x\pm at)\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8a)$$

instead of, as in the case of the accurate equation treated of in the text,

$$\phi=\phi(v\pm a\log_e \alpha_x).$$

Hence, when the motions are small, we may assign to F_1, F_2 the alternative values of ϕ given by (8*a*); or, which is the same thing, we may take for our solution

$$v+a\alpha_x=\psi_1(x+at),$$
$$v-a\alpha_x=\psi_2(x-at),$$

which is identical with the ordinary solution given in this case. We cannot, however, adopt the same course in the general case when the motions are not small.

Eliminating $\frac{d^2\alpha}{dx\,dt}$ between the first of equations (4) and the last of (9), and between the last of (4) and the first of (9), we get

$$0=F'_2(v)F'_1(t)-F'_2(x)F'_1(\alpha_x)+F'_2(v)F'_1(u)\frac{d^2\alpha}{dt^2}-F'_2(\alpha_x)F'_1(\alpha_x)\frac{d^2\alpha}{dx^2},$$

$$0=F'_2(\alpha_x)F'_1(x)-F'_2(t)F'_1(v)+F'_2(\alpha_x)F'_1(\alpha_x)\frac{d^2\alpha}{dx^2}-F'_2(v)F'_1(u)\frac{d^2\alpha}{dt^2},$$

the addition of which equations gives

$$0=F'_2(\alpha_x)F'_1(x)-F'_2(t)F'_1(v)+F'_2(v)F'_1(t)-F'_2(x)F'_1(\alpha_x).\ (10).$$

Again, eliminating $\frac{d^2\alpha}{dx\,dt}$ between the first of each of the pairs (4) and (9), and also between the last of each of the same pairs, we get

$$0=F'_2(\alpha_x)F'_1(t)-F'_2(t)F'_1(\alpha_x)+\{F'_2(\alpha_x)F'_1(v)-F'_2(v)F'_1(\alpha_x)\}\frac{d^2\alpha}{dt^2},$$

$$0=F'_2(v)F'_1(x)-F'_2(x)F'_1(v)+\{F'_2(v)F'_1(\alpha_x)-F'_2(\alpha_x)F'_1(v)\}\frac{d^2\alpha}{dx^2}.$$

Hence, if (1) holds, we shall have, eliminating $\frac{d^2\alpha}{dt^2}$, $\frac{d^2\alpha}{dx^2}$ between the last two equations and (1),

$$0=F'_2(\alpha_x)F'_1(t)-F'_2(t)F'_1(\alpha_x)+\frac{a^2}{\alpha_x^2}\{F'_2(u)F'_1(x)-F'_2(x)F'_1(u)\}.(11)$$

Equations (10) and (11) express the conditions to be satisfied by F_1, F_2 in order that (1) may be derivable from a combination of the derivatives of (3). Transform these equations by putting

$$F_1(xtv\alpha_x)=f_1(xt\omega_1\omega_2),$$
$$F_2(xtv\alpha_x)=f_2(xt\omega_1\omega_2),$$

where

$$\left.\begin{aligned}\omega_1&=v+a\log_e\alpha_x,\\ \omega_2&=v-a\log_e\alpha_x,\end{aligned}\right\}\quad\ldots\ldots\quad(12)$$

then we shall have

$$\begin{array}{l|l}
F_1'(v)=f_1'(\omega_1)+f_1'(\omega_2), & F_2'(v)=f_2'(\omega_1)+f_2'(\omega_2),\\
F_1'(\alpha_x)=\frac{a}{\alpha_x}\{f_1'(\omega_1)-f_1'(\omega_2)\}, & F_2'(\alpha_x)=\frac{a}{\alpha_x}\{f_2'(\omega_1)-f_2'(\omega_2)\},\\
F_1'(t)=f_1'(t), & F_2'(t)=f_2'(t),\\
F_1'(x)=f_1'(x). & F_2'(x)=f_2'(x).
\end{array}$$

Substituting these values in (10) and (11), they become

$$0=\frac{a}{\alpha_x}f_1'(x)\left\{f_2'(\omega_1)-f_2'(\omega_2)\right\}+f_1'(t)\left\{f_2'(\omega_1)+f_2'(\omega_2')\right\},$$

$$-f_2'(t)\left\{f_1'(\omega_1)+f_1'(\omega_2)\right\}-\frac{a}{\alpha_x}f_2(x)\left\{f_1'(\omega_1)-f_1'(\omega_2)\right\},$$

$$0=\frac{a}{\alpha_x}f_1'(t_1)\left\{f_2'(\omega_1)-f_2'(\omega_2)\right\}+\frac{a^2}{\alpha_x^2}f_1'(x)\left\{f_2'(\omega_1)+f_2'(\omega_2)\right\}$$

$$-\frac{a}{\alpha_x}f_2'(t)\left\{f_1'(\omega_1)-f_1'(\omega_2)\right\}-\frac{a^2}{\alpha_x^2}f_2'(x)\left\{f_1'(\omega_1)+f_1'(\omega_2)\right\},$$

or

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)$$

$$-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1)+\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2),$$

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)+\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)$$

$$-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1)-\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2).$$

The addition and subtraction of the last two equations give us

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1),$$

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)-\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2);\ (13)$$

he first of which, when arranged according to the partial differential coefficients of f_2, will stand

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\frac{a}{\alpha_x}f_1'(\omega_1).f_2'(x)-f_1'(\omega_1)f_2'(t);\quad (14)$$

the auxiliary equations for the integration of which by Lagrange's method are

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}dx+\frac{a}{\alpha_x}f_1'(\omega_1)d\omega_1,$$

$$0=dx-\frac{a}{\alpha_x}dt,$$

$$0=d\omega_2.$$

Of these equations, the first by means of the second may be put under the form

$$0=f_1'(x)dx+f_1'(t)dt+f_1'(\omega_1)d\omega_1,$$

or, since $d\omega_2=0$,

$$0=f_1'(x)dx+f_1'(t)dt+f_1'(\omega_1)d\omega_1+f_1'(\omega_2)d\omega_2$$
$$=d.f_1.$$

Hence we may take for the auxiliary equations,

$$\left.\begin{aligned}0&=df_1,\\0&=dx-a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.dt,\\0&=d\omega_2\end{aligned}\right\}\quad\ldots\ldots\quad(15)$$

(since by (12) we have $\log_\epsilon \alpha_x=\frac{\omega_1-\omega_2}{2a}$), the integrals of which are

$$f_1=C,$$

$$\int\mu\{dx-a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.dt\}=C'{}^{*},$$

$$\omega_2=C''.$$

Hence, putting m for the equivalent of C', we get for the value of f_2 derived from the integration of (14),

$$f_2=f_2\{f_1, m, \omega_2\},$$

or, since $f_1=0$,

$$f_2=f_2\{m, \omega_2\}.$$

On the other hand, equation (13) may be written

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)-\frac{a}{\alpha_x}f_1'(\omega_2)f_2'(x)+f_1'(\omega_2)f_2'(t).\ (16)$$

1. Hence, if this last be a substantive equation between the partial differential coefficients of f_2, we shall have for its integration, following a method precisely similar to that already adopted, the auxiliary equations

$$0=df_1,$$

$$0=dx+a\epsilon^{\frac{\omega_2-\omega_1}{2a}}dt,$$

$$0=d\omega_1;$$

and, as before, we shall arrive at a solution of the form

$$f_2=f_2\{n, \omega_1\}.$$

It is clear, however, that if we have

$$f_2=\text{funct.}\ (m, \omega_2)$$

and also

$$f_2=\text{funct.}\ (n, \omega_1),$$

we must have

$$m=\text{funct.}\ (n, \omega_1)$$

and

$$\omega_2=\text{funct.}\ (n, \omega_1).$$

---

[*] Note that in the integration of (15) we shall have ω_2 constant; so that by means of the equation $f_1=C$ we may express ω_1 in terms of x and t and of quantities which are to be regarded as constant. Thus (15) will always be integrable by means of a factor μ.

Hence, eliminating n between these last, we shall get

$$m = \text{funct.}\ (\omega_1\omega_2),$$

$$\therefore f_2 = f_2(m\omega_2) = \text{funct.}\ (\omega_1\omega_2);$$

therefore one of the first integrals of (1) will be of the form

$$\text{funct}\ (\omega_1\omega_2) = 0,$$

whence it is clear that we must have

$$v = f(\alpha_x).$$

The substitution of this value of v in equations (2) gives us

$$0 = f'(\alpha_x)\frac{d\alpha_x}{dt} - \frac{a^2}{\alpha_x^2}\frac{d\alpha_x}{dx},$$

$$0 = f'(\alpha_x)\frac{d\alpha_x}{dx} - \frac{d\alpha_x}{dt};$$

from which it follows that we must have

$$\overline{f'(\alpha_x)}|^2 = \frac{a^2}{\alpha_x^2},$$

and

$$f'(\alpha_x) = \pm\frac{a}{\alpha_x};$$

$$\therefore\ v = \pm a \log_\epsilon \alpha_x + \text{C},$$

or, since v vanishes when $\alpha_x = 1$,

$$v = \pm a \log_\epsilon \alpha\ ;$$

from which it is clear that the solution obtainable in this manner must be identical with that already deduced.

2. The only way of escape from this conclusion is to suppose that (16) is illusory, considered as a relation between the partial differential coefficients of f_2—or, in other words, that we have

$$\frac{a}{\alpha_x}f_1'(\alpha_x) - f_1'(t) = 0,$$

$$f_1'(\omega_2) = 0.$$

The last of these implies that f_1 is a function of ω_1, x, and t only; hence, writing the former

$$a\epsilon^{\frac{\omega_2-\omega_1}{2a}}f_1'(x) - f_1'(t) = 0,$$

the auxiliary equations for its integration are

$$0 = a\epsilon^{\frac{\omega_2-\omega_1}{2a}}dt + dx,\quad 0 = d\omega_1,$$

which give us

$$f_1 = f_1\{\omega_1, (a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t+x)\}$$
$$= f_1\{\omega_1, k\}$$

suppose, where $k = a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t+x$.

But

$$f_1'(\omega_2) = \frac{1}{2a}.\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t.f_1'(k);$$

therefore, since $f_1'(\omega_2) = 0$, we must have $f_1'(k) = 0$, and therefore

$$f_1 = f_1(\omega_1);$$

that is, one of the equations of our solution must be of the form

$$f_1(\omega_1) = 0,$$

a conclusion the consequences of which have been already pointed out.

On the whole, therefore, it appears that the pair of equations

$$v = \pm a \log_\epsilon \alpha_x,$$
$$\alpha_x = \psi\left\{x \mp \frac{a}{\alpha_x}t\right\}$$

constitute the most general solution of which the equations (2) and (1) are susceptible when the motions are not small. But $\alpha_x = \frac{D}{\rho}$, where D denotes the density of equilibrium. Hence, as it is clearly impossible that we shall *always* have the relation between the velocity and density indicated by the equation

$$\frac{v}{a} = \pm \log_\epsilon \frac{\rho}{D}$$

when the motions are not small, it follows that the equations (2) and (1) must be considered defective in their representation of the motion.

6 New Square, Lincoln's Inn,
March 1873.

XIII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* *By* JAMES D. DANA.

[Continued from p. 54.]

5. *Mountain-making slow work.*

TO obtain an adequate idea of the way in which lateral pressure has worked, it is necessary to remember that mountain-elevation has taken place after immensely long periods of quiet and gentle oscillations. After the beginning of the Primordial, the first period of disturbance in North America of special note was that at the close of the Lower Silurian, in which the Green Mountains were finished; and if time from the beginning of the Silurian to the present included only fifty millions of years—which most geologists of the present day would consider much too small an estimate—the interval between the beginning of the Primordial and the uplifts and metamorphism of the Green Mountains was at least ten millions of years. The next epoch of great disturbance in the same Appalachian region was that at the close of the Carboniferous era, in which the Alleghanies were folded up—by the above estimate of the length of time, thirty-five millions of years* after the commencement of the Silurian; so that the Appalachians were at least thirty-five millions of years in making, the preparatory subsidence having begun as early as the beginning of the Silurian. The next on the Atlantic border was that of the displacements of the Connecticut-river sandstone, and the accompanying igneous ejections, which occurred before the Cretaceous era—at least seven millions of years, on the above estimate of the length of time, after the Appalachian revolution. Thus the lateral pressure resulting from the earth's contraction required an exceedingly long era in order to accumulate force sufficiently to produce a general yielding and plication or displacement of the beds, and start off a new range of prominent elevations over the earth's crust.

6. *System in the mountain-making movements on the opposite borders of the North American Continent and over the Oceanic areas.*

A summary of the general system of movements and mountain-making on the opposite borders of the continent, and over the oceanic areas, will, I think, render it apparent that the views here sustained have a broad foundation.

I omit any special reference to the Archæan elevations, and also the local disturbances in the Primordial of Newfoundland, as well as the facts relating to minor changes of level.

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* These estimates of the relative lengths of ages are based on the maximum thickness of their rocks—very uncertain data, but the best we have.

a. Mountain-making on the Atlantic border.

1. At the close of the Lower Silurian, or a little earlier, a culmination of the great Appalachian geosynclinal resulted in displacements, plications, and metamorphism, and the making of a *synclinorium* along the *Green-Mountain* region, these mountains (some summits at present over 4000 feet high above the sea) being the result. The depth to which the region subsided during the Lower Silurian era, and the thickness of the accumulations, are not ascertained; probably the extent was not less than 20,000 feet.

2. Simultaneously a *permanent anticlinorium* was made over the Cincinnati region, from Lake Erie into Tennessee, parallel to the Alleghanies of Virginia, 250 miles to the north-west.

3. The Acadian region (embracing western Newfoundland, St.-Lawrence Bay, the Bay of Fundy, and part of Nova Scotia and New Brunswick adjoining, and probably the sea south-west between St. George's Bank and the coast of Maine, with also an area in Rhode Island) was the course of a great geosynclinal, or a series of them, parallel in general direction to that of the Appalachian region; it continued in progress, but with mountain-making interruptions and some shift of position to the eastward, from the Silurian to the close of the Jurassic.

At the close of the Lower Silurian no general disturbances occurred in this Acadian region, so far as is known. In the Anticosti seas, or northern part of St. Lawrence Bay, limestones, as Logan states, were uninterruptedly in formation from the beginning of the Hudson period of the Lower Silurian to the middle of the Upper Silurian, showing that the Acadian geosynclinal was then in regular progress. It so continued until the close of the Devonian, when disturbances, plication, and metamorphism took place in eastern Canada, Nova Scotia, and the bordering region of New Brunswick, and the most extensive of Acadian Palæozoic synclinoria resulted, according to the observations of Dr. Dawson and others.

4. The close of the Carboniferous age was an epoch of mountain-making in the Alleghany region—the Alleghanies from New York to Alabama having been then made, as already explained.

5. At the same time there were disturbances and synclinorian plications in the Acadian region. During the Carboniferous era, according to Logan and Dawson, 16,000 feet of rock had in some parts accumulated; and therefore a geosynclinal of 16,000 feet formed—the rocks in their many coal-beds and root-

bearing layers bearing evidence, to the last, of oscillations involving an intermittent but progressing subsidence. The synclinorium, the resultant, was much less marked than that at the close of the Devonian.

6. During the Palæozoic, along the sea-border, a more or less perfect barrier was made by a geanticlinal uplift (anticlinorium), which was a counterpart to the geosynclinal of the Appalachian region*.

7. The middle or close of the Jurassic period was an epoch of displacements, and the making of a series of imperfect synclinoria along the Triassico-Jurassic areas from Nova Scotia to southern North Carolina, as sufficiently described.

8. During the era of the Connecticut-river sandstone (Triassico-Jurassic) a nearly complete sea-border anticlinorium existed, a counterpart to the progressing geosynclinal. Its existence is proved by the absence of all marine fossils from the beds*.

9. The era closing the Cretaceous, and that of the Tertiary, witnessed but small uplifting and some local displacements of the rocks of these eras on the Atlantic border. The principal movement was geanticlinal; and it involved probably the whole Alleghany region.

10. In the Quaternary there were extended movements of geanticlinal and geosynclinal character, which need not be here described.

b. Mountain-making after Archæan time on the Pacific border, within the territory of the United States.

1. At the close of the Lower Silurian, none yet known.

2. At the close of the Devonian, none yet known†.

3. At the close of the Carboniferous age, or the Palæozoic,

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* In my 'Manual of Geology' the probable existence of such a barrier is recognized, in connexion with the remarks on the geography of the Trenton period in America; and it is particularly dwelt upon and illustrated by a map in the chapter on the Triassic; but it is not spoken of as connected in origin with the geosynclinal that was in progress to the west of it. Evidence with regard to this anticlinorium is given in the following part of this memoir.

† Although no case of unconformability between the Carboniferous and the underlying Palæozoic is yet distinctly made out in the Sierra Nevada, the Great Basin, or the Wahsatch, such occur further south, according to Mr. J. W. Powell, in the vicinity of the Grand Cañon of the Colorado (see Amer. Journ. Sci. 3rd ser. vol. v. p. 457). The fact that Whitney has found no rocks lower than Carboniferous in the Sierra may be a consequence of the same unconformability beneath these mountains. But in the region of the Cañon, the Carboniferous, Triassic, Jurassic, Cretaceous, and Tertiary beds are all conformable. The epochs of mountain-making over the Pacific slope south of the latitudes of the Wahsatch range, and also of that to the north, were different from those within these latitudes.

none yet known; and if none really occurred, then the contracting globe at that time, as far as U. S. North America is concerned, must have expended its energies (which it had been gathering during the Palæozoic) in making the Alleghanies, and in some minor plications along the Acadian region.

The "Great Basin," between the Sierra Nevada on its western border and the Wahsatch range on its eastern (lying along the meridian just east of the Great Salt Lake), contains a number of short ridges parallel to these lofty border ranges, some of which are quite high*; and they consist, according to King, "of folds of the infra-Jurassic rocks;" and "it is common to find no rocks higher than the Carboniferous," owing, it is stated, to the erosion that has taken place. It is not clear that part, at least, of the Great Basin plications may not have taken place before the Jurassic era. If not, then the movements must have been in some way involved with those of the Sierra and Wahsatch regions†.

4. At the close of the Jurassic two great geosynclinals, which had been in progress through the Palæozoic and until this epoch in the Mesozoic, culminated each in the making of a lofty synclinorium:—one the Sierra Nevada, some of whose summits are over 14,000 feet high; the other the high Wahsatch, a parallel north and south range.

Whitney has proved that the Carboniferous and Jurassic rocks are comformable in the Sierra-Nevada range, and that the close of the Jurassic was the epoch of its origin; but direct proof is not yet found that the Devonian and Silurian formations are included. The granite axis of the chain probably indicates, as LeConte has suggested, the region of maximum disturbance and metamorphism.

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* An admirable chart, giving in detail the topography of this whole region and including the Wahsatch, has been prepared by Mr. James T. Gardner, after careful surveys by himself, topographical surveyor of the Exploration of the Fortieth Parallel under Clarence King, and is now ready for the engraver. Mr. King has published thus far only brief chapters on the geological results of his survey in the volume of J. T. Hague on Mining Industry, vol. iii. He has ready for publication vols. i. and ii., on Systematic and Descriptive Geology. The Botanical Report of the Survey, vol. v., has been issued; but vol. iv., on Zoology and Palæontology, remains to be completed.

† Mr. James T. Gardner, in a letter of May 8th, informs me that in his opinion all the more important mountain-ranges of the Great Basin (in Nevada) are included in a chain trending about N. 40° E., the whole having a breadth across the trend of 120 miles. Austin, in Nevada, lies near the centre line of the chain. To the west of this elevated region is the great depression where the rivers of Nevada evaporate in Carson, Humboldt, Pyramid, Mud, and other lakes; and to the east is the great depressed area of Salt Lake. On this view, if these mountains were made at the close of the Jurassic, there were formed at this epoch three lofty synclinoria (the Sierra, the Humboldt, and the Wahsatch) on the western-border, central, and eastern-border chains of the Great Basin. The precise determination of the epoch of origin of the Humboldt chain is therefore of much importance.

The Wahsatch contains, according to Clarence King, formations of all the ages from the Lower Silurian to the Jurassic; and the whole are throughout conformable; and a great thickness of crystalline rocks exists beneath, supposed to be Archæan, which he states are conformable also. The plications and mountain-making took place, as King states, contemporaneously with the same in the case of the Sierra, before the Crétaceous era, the Cretaceous beds lying on the Jurassic unconformably.

These two synclinoria are 400 miles apart. The preparatory geosynclinal of the Wahsatch, and probably that of the Sierra, took for its completion, supposing it to have begun with the opening Silurian, a period at least a fifth longer than the whole Palæozoic.

5. At the close of the Cretaceous another pair of geosynclinals, parallel with the coast, but geosynclinals of only Cretaceous origin, culminated in synclinoria.

One of the Cretaceous geosynclinals was in progress *east of the Wahsatch*, along the whole summit-region of the Rocky Mountains in the United States. Directly east of the Wahsatch, according to King, the beds are 9000 feet thick, or more; and, as Hayden states, they have a great thickness in the Laramie Plains, and little less over the upper Missouri region, so that the downward movement was in some parts a profound one, and affected a very wide extent of country. Hayden and King make this disturbance to have taken place after part, or all, of the Eocene period had passed, while Prof. Marsh holds that it occurred at the close of the Cretaceous period[*].

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[*] Clarence King has very briefly described the Wahsatch region, as well as the country to the west, in the third volume (4to, 1870) of his United-States Geological Exploration of the 40th parallel; and on page 454 he says:—"Subsequent to the laying down of the old cretaceous system, and of those conformable freshwater beds which close the coal-bearing period, another era of mountain-uplifts occurred, folding the coal series [Cretaceous and Lower Tertiary] into broad undulating ridges having a general trend north-east." He then observes that freshwater Tertiary beds of sand and clay, "an immense accumulation," were laid down *unconformably* over this upturned Cretaceous, and, after the Miocene era, were subjected to "orographic" disturbances and "tilted to an angle of 15° to 20°, or thrown into broad and gentle undulations wherever they lie in the neighbourhood of the older ranges, such as the Wahsatch and Uintah." These disturbances were confined to within fifteen miles of the Wahsatch. The period in which they occurred witnessed also great outflows of trachytic rocks in this and other parts of the Rocky-Mountain region. Mr. King adds, on page 455, that there is no question as to the identity of the beds that *overlie unconformably* the Cretaceous folds along the eastern flank of the Wahsatch with the horizontal Tertiary deposits of the G

The other geosynclinal belt of the Cretaceous era was to the *west of the Sierra Nevada,* as described by Whitney. This coast geosynclinal ended in extensive displacements and plications, much metamorphism, and a high synclinorium.

6. The intermediate region, the Great Basin, which had been widened at the close of the Jurassic by the annexation of the plicated and consolidated Sierra and Wahsatch, was the era of a geanticlinal, or at least of absence of subsidence; for King says no Cretaceous rocks occur over it.

7. With the close of the Cretaceous, or when the Cretaceous synclinorian movements of the sea-coast and mountains were ending, a geanticlinal movement of the whole Rocky-Mountain region began, which put it above the sea-level, where it has since remained. This upward movement continued through the Tertiary.

8. During the Tertiary age, until the close, probably of the Miocene Tertiary, another pair of parallel geosynclinals, but geosynclinals of Tertiary formation, were in progress. The Cretaceous synclinoria had given still greater breadth and stability to the relatively stable region between them; and one of these new troughs is hence further east on the mountain side, and the other further west on the coast side.

In the coast geosynclinal, marine tertiary beds were accumulated to a thickness of 4000 to 5000 feet; and then followed the epoch of disturbance ending in another coast synclinorium, a coast range of mountains, in some places metamorphic, and having ridges, many of which are at present 2000 feet or more in height above the sea, and some in the Santa-Cruz range, according to Whitney, over 3500 feet.

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basin, and that over this basin between the Green River and the Wahsatch no single instance of conformity occurs between the coal-beds and the overlying horizontal freshwater strata. As stated above, he makes the epoch of Cretaceous uplifting to have followed, not the Cretaceous period, but the earliest period of the Tertiary, Eocene beds being, in his view, included with the Cretaceous in the folds referred to.

Dr. Hayden has investigated with much detail the Green-River basin and the region east of it, and years since announced that the Lower Tertiary beds in some parts of the Rocky-Mountain region were tilted at a high angle. He has held that all the coal-bearing strata were Lower Tertiary, but now agrees with the view expressed by King, and first suggested by Meek, that part are Cretaceous, while another part are Lower Tertiary, and considers the later Tertiary beds (which lie unconformably on the beds below in the regions of disturbance) Miocene and Pliocene. He states that the thickness of the Cretaceous formation in the Laramie plains is 8000 to 10,000 feet. He observes in a recent letter to the writer that the coal-bearing strata and Cretaceous are never unconformable, but instead are often folded together, and sometimes stand at a high angle, even vertical in many places—as in the Laramie Plains south of Fort Sanders, along the

The other is to the east of the Cretaceous axis in the summit region of the Rocky-Mountain chain. A great thickness of freshwater beds was made in the Green-River region and some other places about the Rocky-Mountain summits, and thinner deposits to the eastward. The thickness, in connexion with evidences of shallow-water origin, indicates a progressing geosynclinal, although the ocean gained no entrance to it. The downward bending ended probably just after the Miocene period, without general displacements; but there were tiltings along the more western border of the Tertiary in the vicinity of the Wahsatch and other mountains. (See note on page 135.)

9. Since the Miocene era, and on through much of the Quaternary, there have been vast fissure-eruptions over the western Rocky-Mountain slopes. They had great extent, especially in the Snake-River region, where the successive outflows made a stratum 700 to 1000 feet thick, over an area 300 miles in breadth. There are other similar regions, but of less area.

It is thus seen that along the Pacific side of the continent the crust, under the action of lateral pressure, first bent downward profoundly, and then yielded and suffered fracture and plications, directly along a belt, parallel with the coast, either side of the Great Basin (and perhaps over this basin to some extent), the two great lines 400 miles apart. The plicated regions thus made, having become firm by the continued pressure and the engendered heat and resultant solidification, the crust next bent, and then yielded, in a similar way, along an axis outside of the former regions of disturbance, the two axes over 600 miles apart; and again all was mended in the same way. Then it bent a third time, just outside of the last range, on each side of the same great area, the lines over 700 miles apart; and then over the western of the two ranges the beds were displaced,

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Big-Horn region between Long's Peak and Pike's Peak, near Denver in Colorado, &c. Near the mouth of the Big Horn, the Chetish or Wolf Mountains, consist of these upturned strata, and have a height of 1500 to 2000 feet above the Yellowstone. He found the later Tertiary beds sometimes tilted at a small angle, never over 10°.

The discovery of Dinosaurian remains in some of the coal-beds, announced by Marsh and Cope, and of *Inocerami*, as ascertained by Meek, is one part of the evidence on which the lower parts of the coal-beds are determined to be Cretaceous. Besides this, there is the fact that the supposed Miocene of the Green-River basin contains remains of mammals that are decidedly Eocene in character; and if these are Eocene, then the coal-beds are something older. Professor Marsh is very strongly of the opinion that all the *coal-beds are Cretaceous.*

On the other side, Lesquereux states that the evidence from fossil plants is totally opposed to making any of the coal-strata Cretaceous.

The method of mountain-making, and the principle involved, ar
whatever be the decision as to the exact epoch of the Cretaceo

solidified, and left in high ridges, but over the eastern the final disturbances were local and slight.

There were hence two parallel series, contemporaneous in steps of progress, situated on opposite borders of the Great Basin, a *coast* series and a *mountain* series, each having its highest member toward the basin—the coast series the grandest in its three parts and leaving evidences of the profoundest disturbance and the greatest amount of metamorphism. The Wahsatch range is nearly as high as the Sierra; but probably a fourth of its height is due to the final elevation of the Rocky-Mountain region.

The last bendings were more local than the preceding, because the crust had become stiffened by its plicated and solidified and partly crystallized coatings, as well as by thickening beneath; and, therefore, while the Tertiary movements were in progress the part of the force not expended in producing them carried forward an upward bend, or geanticlinal, of the vast Rocky-Mountain region as a whole. For the same reason profound *breakings* took place where bending was not possible, and thereby immense floods of liquid rock were poured out over the surface. (Most of the great mountains of the globe were lifted about this time—that is, in the course of the Tertiary era—and many of the great volcanoes were made.)

There were irregularities, or exceptional courses, in connexion with this system of movements and their effects. But these show only that in the same area the lateral pressure at work was not alike either in amount or in direction in different latitudes, nor was the resistance before it the same.

The results correspond with the well-understood effects of lateral pressure. Suppose a long beam, having an even texture, except that a portion toward the middle (say a sixth of the whole length) is stouter than the rest, to be subjected at its extremities to direct pressure. The first yielding and fracture would take place toward the stouter portion on either side. If this break were mended by splicing and cementing until firmer than before, the next region of yielding would be just outside of the former. In brief, the fracturing would be in each case near the stouter portion of the beam. Moreover the extent of the yielding and fracture on each side would have some relation to the amount of pressure against that side. Just so has it been with the earth's crust under the action of lateral pressure. The facts further illustrate the truth, before announced, that the force from the ocean side had in some way the advantage, and in fact was the greater. But the full difference is not indicated by the difference in the results of disturbance, since the shoving force on the side of greatest pressure would not be limited in its action to its own side, unless the intermediate stouter region were wholly immovable.

c. *Movements over Oceanic areas.*

The history of the changes of level over the oceanic areas is necessarily a meagre branch of geological science. There are, however, some great truths to be gathered which are of profounder import than is generally acknowledged. They show that the oceanic crust has sometimes acted in the capacity of a single area of depression, although of so immense extent. I allude briefly here to only two of the facts, referring the reader to my former articles for a fuller discussion of the subject:—

First, the remarkable one that nearly all the ranges of islands over the Pacific ocean, and even the longer diameters of the particular islands, lie nearly parallel with the great mountain-ranges of the Pacific coast of North America. There is a dynamical announcement in this arrangement—which is partly recognized, when we refer it, as I have proposed, to the existence of directions of easiest fracture in the very nature of the infra-Archæan crust, and regard the courses of these feature-lines of the oceans and continents as having reference to one of these directions. But, besides this, there is a declaration with regard to the *direction* of the pressure that acted against the continents, and reacted over the oceanic areas.

The other fact is that of the coral-island subsidence, already referred to, which affected the tropical ocean for its whole breadth, or more than a quarter of the circumference of the globe—sinking the sea-bottom at least 3000 feet over a large part of the area, and probably over its axial portions two or three times this amount*. The oceanic basin was evidently one basin in its movement; but the areas of less and greater subsidence, of parallel N.W. by W. trend, so alternate along the southern border of the region of subsidence that we may conclude there were great parallel waves, made by lateral pressure in the crust, as I have elsewhere explained†—that is, geosynclinals and geanticlinals, such as are the only possible conditions of the crust under the lateral pressure of the contraction. Now this great oceanic subsidence, involving the breadth of the ocean, if begun in the Tertiary era, as is probable, was going forward at the very time when the Rocky Mountains, and other great mountains of the globe, were in progress of elevation, as if these were counterpart movements in the earth's surface; and it continued on during the Glacial era, when the continental elevations appear to have reached their highest limit.

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* Author's Rep. Geol. Wilkes U.S. Expl. Exped. 4to, 1849, and 'Corals and Coral Islands,' 8vo, 1872, p. 329.

† Rep. Geol. Expl. Exped. p. 399; 'Corals and

We gather from these facts how it is that a general submergence, or an emergence, might characterize cotemporaneously large areas of North America and Europe—as, for example, in the Subcarboniferous, Carboniferous, and Permian periods, during which the rocks show that there was a general parallelism in the movements. If a geanticlinal were in progress over the middle of the Atlantic crust as a result of the lateral thrust in the continental and oceanic crusts there might also be a reverse movement or general sinking along the continental borders, as well as a rise of water about the continents from the diminution in the ocean's depth; and when the oceanic geanticlinal flattened out again through subsidence, the subsiding crust would naturally produce a reverse movement along one or both continental borders.

From the various considerations here presented, derived from both the continental and oceanic areas, it is apparent that the earth has exhibited its oneness of individuality in nothing more fundamentally and completely than in the heavings of its contracting crust.

The subjects of metamorphism, the earth's interior, igneous eruptions and volcanoes remain for discussion. In addition I propose to consider the steps in the origination of the continental plateaux and oceanic basins, and also to present some facts bearing on the general nature of the infra-Archæan crust—that is, the part below the earth's superficial coatings.

[To be continued.]

XIV. *On Magnetic Permeability**, *and the Maximum of Magnetism of Iron, Steel, and Nickel.* *By* HENRY A. ROWLAND, *Instructor in Physics in the Rensselaer Polytechnic Institute, Troy, N.Y.*†

[With Two Plates.]

MORE than three years ago I commenced the series of experiments the results of which I now publish for the first time. Many of the facts which I now give were obtained then; but, for satisfactory reasons, they were not published at that time. The investigations were commenced with a view to determine the distribution of magnetism on iron bars and steel magnets; but it was soon found that little could be done without new experiments on the magnetic permeability of substances.

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* The word "permeability" has been proposed by Thomson, and has the same meaning as "conductivity" as used by Faraday ('Papers on Electricity and Magnetism,' Thomson, p.484; Maxwell's 'Electricity and Magnetism,' vol. ii. p. 51).

† Communicated by Professor J. Clerk Maxwell, M.A., F.R.S.

Few observations have been made as yet for determining the magnetic permeability of iron, and none, I believe, of nickel and cobalt, in absolute measure. The subject is important, because in all theories of induced magnetism a quantity is introduced depending upon the magnetic properties of the substance, and without a knowledge of which the problem is of little but theoretical interest; this quantity has always been treated as a constant, although the experiments on the maximum of magnetism show that it is a variable. However, the form of the function has never been determined, except so far as we may deduce it from the equation of Müller,

$$\mathrm{I}=220d^{\frac{3}{2}}\tan\frac{m}{\cdot00005d^2},$$

which, as will be shown, leads to wrong results. The quantities used by different persons are as follows:—

κ, Neumann's coefficient, or magnetic susceptibility (Thomson).

k, Poisson's coefficient.

μ, coefficient of magnetization (Maxwell), or magnetic permeability (Thomson).

λ, introduced for convenience in the following paper.

The relations of these quantities are given by the following equations:—

$$k=\frac{4\pi\kappa}{4\pi\kappa+3}=\frac{\mu-1}{\mu+2}=\frac{\lambda-4\pi}{\lambda+8\pi},$$

$$\kappa=\frac{\mu-1}{4\pi}=\frac{3k}{4\pi(1-k)}=\frac{\lambda-4\pi}{16\pi^2},$$

$$\mu=\frac{1+2k}{1-k}=4\pi\kappa+1=\frac{\lambda}{4\pi}.$$

The first determination of the value of any of these quantities was made by Thalén. But more important experiments have been made by Weber, Von Quintus Icilius, and more recently by M. Reicke and Dr. A. Stoletow*. The first three of these in their experiments used long cylindrical rods, or ellipsoids of great length; the last, who has made by far the most important experiments on this subject, has used an iron ring. The method of the ring was first used by Dr. Stoletow in September 1871; but more than eight months before that, in January 1871, I had used the same method, but with different apparatus, to measure the magnetism. He plots a curve showing the variation of κ; but he plots it with reference to R as abscissa instead of R κ, and thus fails to determine the law. His method of experiment is much more complicated than mine, so that he could only obtain results for one ring; while by my method I have experimented on about a dozen rings and on numerous bars, so that I believe I have been enabled to find the true form of the function according to which μ varies with the magnetism of the bar or the magnetizing-force.

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* Phil. Mag. January 1873.

Many experiments have been made on the magnetism of iron without giving the results in absolute measure. Among these are the experiments of Müller, Joule, Lenz and Jacobi, Dub, and others. The experiments have been made by the attraction of electromagnets, by the deflection of a compass-needle, or, in one case, by measuring the induced current in a helix extending the whole length of the bar. By the last two methods the change in the *distribution* of magnetism over the bar when the magnetism of the bar varies is disregarded, if indeed it was thought of at all: even in a recent memoir of M. Cazin* we have the statement made that the position of the poles is independent of the strength of the current. He does not give the experiment from which he deduces this result. Now it is very easy to show, from the formula of Green for the distribution of magnetism on a bar-magnet combined with the known variation of κ, that this can only be true for short and thick bars; and it has also been remarked by Thomson that this should be the case†. An experiment made in 1870 places this beyond doubt. A small iron wire (No. 16), 8 inches long, was wound with two layers of fine insulated wire; a small hard steel magnet ¼ inch long suspended by a fibre of silk was rendered entirely astatic by a large magnet placed about 2 feet distant; the wire electromagnet was then placed near it, so that the needle hung 1½ inch from it and about 2 inches back from the end. On now exciting the magnet with a weak current, the needle took up a certain definite position, indicating the direction of the line of force at that point. When the current was very much increased, the needle instantly moved into a position more nearly parallel to the magnet, thus showing that the magnetism was now distributed more nearly at the ends than before. This shows that nearly all the experiments hitherto made on bar-magnets contain an error; but, owing to its small amount, we can accept the results as approximately true.

I believe mine are the first experiments hitherto made on this subject in which the results are expressed and the reasoning carried out in the language of Faraday's theory of lines of magnetic force; and the utility of this method of thinking is shown in the method of experimenting adopted for measuring magnetism in absolute measure, for which I claim that it is the simplest and most accurate of any yet devised. Whether Faraday's theory is correct or not, it is well known that its use will give correct results; at the present time the tendency of the most advanced thought is *toward* the theory*; and indeed it has been pointed out by Sir William Thomson that it follows, from dynamical reasoning upon the magnetic rotation of the plane of polarization of light, that the medium in which this takes place must itself be in rotation, the axis of rotation being in the direction of the lines of force†. Some substances must of necessity be more capable of assuming this rotary motion than others; and hence arises the notion of magnetic "conductivity" and "permeability."

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* *Annales de Chimie et de Physique*, Feb. 1873, p. 171.

† Papers on Electricity and Magnetism, p. 512.

Thomson has pointed out several analogies which may be used in calculating the distribution and direction of the lines of force under various circumstances. He has shown that the mathematical treatment of magnetism is the same as that of the flow of heat in a solid, as the static induction of electricity, and as the flow of a frictionless incompressible liquid through a porous solid. It is evident that to these analogies we may add that of the conduction of electricity‡. We readily see that the reason of the treatment being the same in each case is that the elementary law of each is similar to Ohm's law. Mr. Webb§ has shown that this law is useful in electrostatics; and I hope, in a sequel to this paper, to apply it to the distribution of magnetism: I give two equations derived in this way further on.

The absolute units to which I have reduced my results are those in which the metre, gramme, and second are the fundamental units. The unit of magnetizing-force of helix I have taken as that of one turn of wire carrying the unit current per metre of length of helix, and is 4π times the unit magnetic field. This is convenient in practice, and also because in the mathematical solution of problems in electrodynamics the magnetizing-force of a solenoid naturally comes out in this unit. The magnetizing-force of any helix is reduced to this unit by multiplying the strength of current in absolute units by the number of coils in the helix per metre of length. These remarks apply only to endless solenoids, and to those which are very long compared with their diameter. The unit of number of lines of force I have taken as the number in one square metre of a unit field measured perpendicular to their direction. As my data for reducing my results to these units, I have taken the horizontal force of the earth's magnetism at Troy as 1·641, and the total force as 6·27.

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* "On Action at a Distance," Maxwell, 'Nature,' Feb. 27 and March 6 and 13, 1873.

† Thomson's 'Papers on Electricity and Magnetism,' p. 419, note; and Maxwell's 'Treatise on Electricity and Magnetism,' vol. ii. chap. xxi.

‡ Maxwell's 'Treatise on Electricity and Magnetism,' arts. 243, 244, and 245.

§ "Application of Ohm's Law to Problems in Electrostatics," Phil. Mag. S. 4. vol. xxxv. p. 325 (1868).

The total force, which will most seriously affect my results, is well known to be nearly constant at any one place for long periods of time.

From the analogy of a magnet to a voltaic battery immersed in water I have obtained the following, on the assumption that μ is constant, and that the resistance to the lines of force passing out into the medium is the same at every point of the bar.

Let R = resistance to lines of force of one metre of length of bar.
R' = resistance of medium along 1 metre of length of bar.
Q' = lines of force *in* bar at any point.
Q_e = lines of force passing from bar along small distance I.
ϵ = base of Napierian system of logarithms.
x = distance from one end of helix.
b = total length of helix.
s' = resistance at end of helix of the rest of bar and medium.
M = magnetizing-force of helix.

We then obtain

$$*Q_e=\frac{\mathrm{MI}}{2\sqrt{\mathrm{RR}'}}\,\frac{1-\mathrm{A}}{(\mathrm{A}\epsilon^{rb}-1)}\left(\epsilon^{rx}-\epsilon^{r(b-x)}\right), \quad \ldots\ldots \quad (1)$$

$$Q'=\frac{\epsilon^{rb}-1}{(\mathrm{A}\epsilon^{rb}-1)(\sqrt{\mathrm{RR}'}+s')}\,\frac{\mathrm{M}}{r}-\frac{\mathrm{M}}{2\mathrm{R}}\,\frac{1-\mathrm{A}}{\mathrm{A}\epsilon^{rb}-1}\left(\epsilon^{rb}+1-\epsilon^{rx}-\epsilon^{r(b-x)}\right), \quad (2)$$

in which

$$r=\sqrt{\frac{\mathrm{R}}{\mathrm{R}'}},$$

and

$$\mathrm{A}=\frac{\sqrt{\mathrm{RR}'}+s'}{\sqrt{\mathrm{RR}'}-s'};$$

for near the centre of an infinitely long bar, where $x>0$ and

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* Formulæ giving the same distribution as this have been obtained by Biot and also by Green. See Biot's *Traité de Physique*, vol. iv. p. 669, and 'Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism,' by Green, 17th section.

$<b$, and $b=\infty$, we have

$$Q_e=0, \text{ and } Q'=\frac{M}{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

For a ring-magnet, $s'=0$;

$$\therefore Q_e=0, \text{ and } Q'=\frac{M}{R}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

And if a is the area of the bar or ring,

$$a\lambda=\frac{1}{R}=\frac{Q'}{M}, \text{ or } \lambda=\frac{Q'}{aM}, \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

in which λ is the same as in the equations previously given. These equations show that we may find the value of λ, and hence the permeability, by experimenting either on an infinitely long bar or on a ring-magnet. Equations (4) evidently apply to the case where the diameter of the ring is large as compared with its section. The fact given by these equations can be demonstrated in another and, to some persons, more satisfactory manner. If n is the number of coils per metre of helix and n' the number on a ring-magnet, i the strength of current, and ρ the distance from the axis of the ring to a given point in the interior of the ring-solenoid, the magnetic field at that point will, as is well known, be

$$2n'i\frac{1}{\rho},$$

and at a point within an infinitely long solenoid

$$4\pi in.$$

If the solenoid contain any magnetic material, the field will be for the ring

$$2n'i\frac{\mu}{\rho}$$

and for the infinite solenoid

$$4\pi\mu in.$$

Therefore the number of lines of force in the whole section of a ring-magnet of circular section will be, if a is the mean radius of the ring,

$$Q'=4n'i\mu\int_{-R}^{+R}\frac{\sqrt{R^2-x^2}}{a-x}dx=4\pi n'i\mu(a-\sqrt{a^2-R^2});$$

or, since $n'=2\pi an$ and $M=in$, we have, by developing,

$$Q'=4\pi M\mu(\pi R^2)\left(1+\frac{1}{4}\frac{R^2}{a^2}+\frac{1}{8}\frac{R^4}{a^4}+\&c.\right). \quad . \quad . \quad . \qquad (6)$$

For the infinite electromagnet we have in the same way for a circular section,

$$Q' = 4\pi M\mu(\pi R^2). \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When the section of the ring is thin, equation (6) becomes the same as equation (7), and either of them will give

$$\lambda = 4\pi\mu = \frac{Q'}{(\pi R^2)M}, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is the same as equation (5).

In all the rings used the last parenthesis of (6) is so nearly unity that the difference has in most cases been neglected, the slightest change in the quality of the iron producing many times more effect on the permeability than this. Whenever the difference amounted to more than $\frac{1}{200}$ it was not rejected.

The apparatus used to measure Q′ was based upon the fact discovered by Faraday, that the current induced in a closed circuit is proportional to the number of lines of force cut by the wire, and that the deflection of the galvanometer-needle is also, for small deflections, proportional to that number. In the experiments of 1870–71. an ordinary astatic galvanometer was used; but in those made this year a galvanometer was specially constructed for the purpose. It was on the principle of Thomson's reflecting instrument, but was modified to suit the case by increasing the size of the mirror to $\frac{5}{8}$ of an inch, by adding an astatic needle just above the coil *without* adding another coil, by loading the needle to make it vibrate slowly, and, lastly, by looking at the reflected image of the scale through a telescope instead of observing the reflection of a lamp on the scale. The galvanometer rested on a firm bracket attached to the wall of the laboratory near its foundation. In most of the experiments the needle made about five single vibrations per minute. The astatic needle was added to prevent any external magnetic force from deflecting the needle; and directive force was given by the magnet above. Each division of the scale was ·075 inch long; and the extremities of the scale were reached by a deflection of 7° in the needle from 0. The scale was bent to a radius of 4 feet, and was 3 feet from the instrument. At first a correction was made for the resistance of the air &c.; but it was afterwards found by experiment that the correction was very exactly proportional to the deflection, and hence could be dispensed with. This instrument gave almost perfect satisfaction; and its accuracy will be shown presently.

The tangent-galvanometer was also a very fine instrument, and was constructed expressly for this series of experiments. The needle was 1·1 inch long, of hardened steel; and its deflections were read on a circle graduated to half degrees, and 5 inches in diameter. The average diameter of the ring was 16½ inches nearly, and was wound with several coils; so that the sensibility could be increased or diminished at pleasure, and so give the instrument a very wide range. The value of each coil in producing deflection was experimentally determined to within *at least* ⅛ of 1 per cent. by a method which I shall soon publish. The numbers to multiply the tangent of the deflection by, in order to reduce the current to absolute measure, were as follows:—

Number of coils.	Multiplier.
1	·05377
3	·01800
9	·006007
27	·002018
48	·001143

By this instrument I had the means of measuring currents which varied in strength several hundred times with the same accuracy for a large as for a small current. For greater accuracy a correction was applied according to the formula of Blanchet and De la Prevostaye for the length of the needle, the position of the poles being estimated; this correction in the deflections used was always less than ·6 per cent. To eliminate any error in the position of the zero-point, two readings were always taken with the currents in opposite directions, each one being estimated with considerable accuracy to $\frac{1}{10}$ of a degree.

The experiments were carried on in the assay laboratory of the Institute, which was not being used at that time; and precautions were taken that the different parts of the apparatus should not interfere with each other. The disposition of the apparatus is represented in Plate II.

The current from the battery A, of from two to six large Chester's "electropoion" cells No. 2, joined according to circumstances, passed to the commutator B, thence to the tangent-galvanometer C, thence to another commutator D, thence around the magnet E (in this case a ring), and then back through the resistance-coils K to the battery. To measure the magnetism excited in E, a small coil of wire F was placed around it*, which connected with the galvanometer H, so that, when the magnetism was reversed by the commutator D, the current induced in the coil F, due to twice cutting the lines of force of the ring, produced a sudden swing of the needle of H. As the needle swung very freely and would not of itself come to rest in ten or fifteen minutes, the little apparatus I was added: this consisted of

* If a bar was used, this coil was placed at its centre.

a small horseshoe magnet, on one branch of which was a coil of wire; and by sliding this back and forth, induced currents could be sent through the wire, which, when properly timed, soon brought the needle to rest. This arrangement was very efficient; and without it this form of galvanometer could hardly have been used. To compare the magnetism of the ring with the known magnetism of the earth, and thus reduce it to absolute measure, a ring G supported upon a horizontal surface was included in the circuit; when this was suddenly turned over, it produced an induced current, due to twice cutting the lines of magnetic force which pass through the ring from the earth's magnetism. The induced current in the case of either coil, F or G, is proportional to the number of the lines of force cut by the coils * and to the number of wires in the coil, which latter is self evident, but may be deduced from the law of Gaugain †. It is evident, then, that if c is the deflection from coil G, and h that from helix F, the number of lines of force passing through the magnet E, expressed in the unit we have chosen, will be

$$Q'=2n'(6{\cdot}27 \sin 74^\circ\ 50')\pi R^2 \frac{1}{c}\frac{h}{2n'}, \quad . \quad . \quad . \quad (9)$$

where n' is the number of coils in the ring G, n the number in the helix F, R the radius of G, 6·27 the total magnetism of the earth, and 74° 50′ the dip. The quantity $2n'\ (6{\cdot}27 \sin 74^\circ\ 50')\pi R^2$ is constant for the coil, and had the value 14·51. This is the number of square metres of a unit field which, when cut once by a wire from the galvanometer, would produce the same deflection as the coil when turned over.

The experiments being made by reversing the magnetism of the bars, a rough experiment was made to see whether they had time to change in half a single vibration of the needle; it was found that this varied from sensibly 0 to nearly 1 second, so that there was ample time. It was also proved that the sudden impulse given to the needle by the change of current produced the same deflection as when the change was more gradual, which has also been remarked by Faraday, though he did not use such sudden induced currents. As a test of the method, the horizontal force of the earth's magnetism was determined by means of a vertical coil; it was found to be 1·634, while the true quantity is 1·641.

It is sometimes assumed that some of the action in a case like the present is due to the direct induction of the helix around the magnet on the coil F. I think that this is not correct; for when

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* Faraday's Experimental Researches, vol. iii. series 29.

† Daguin's *Traité de Physique*, vol. iii. p. 691.

the helix is of fine wire closely surrounding the bar or ring, all the lines of force which affect F must pass through the bar, and so no correction should be made. However, the correction is so small that it will hardly affect the result. If it were to be made, $\frac{Q'}{a}$ (equation 5) should be diminished by $4\pi M$; but, for the above reasons, it has not been subtracted. As a test of the whole arrangement, I have obtained the number of lines of force in a very long solenoid: the mean of two solenoids gave me

$$Q' = 12{\cdot}67M(\pi R^2);$$

while from theory we obtain, by equation (7) ($\mu = 1$),

$$Q' = 12{\cdot}57M(\pi R^2),$$

which is within the limits of error in measuring the diameter of the tubes &c.

All the rings and bars with which I have experimented have had a circular section. In selecting the iron, care must be used to obtain a homogeneous bar; in the case of a ring I believe it is better to have it welded than forged solid; it should then be well annealed, and *afterwards* have the outside taken off *all round* to about $\frac{1}{8}$ of an inch deep in a lathe. This is necessary, because the iron is "burnt" to a considerable depth by heating even for a moment to a red heat, and a sort of tail appears on the curve showing the permeability, as seen on plotting Table III. To get the normal curve of permeability, the ring must only be used *once*; and *then* no more current must be allowed to pass through the helix than that with which we are experimenting at the time. If by accident a stronger current passes, permanent magnetism is given to the ring, which entirely changes the first part of the curve, as seen on comparing Table I. with Table II. The areas of the bars and rings were always obtained by measuring their length or diameter across, and then calculating the area from the loss of weight in water. The following is a list of a few of the rings and bars used, the dimensions being given in metres and grammes. In the fourth column "annealed" means heated to a red heat and cooled in open air, "C annealed" means placed in a large crucible covered with sand, and placed in a furnace, where, after being heated to redness, the fire was allowed to die out; "natural" means that its temper was not altered from that it had when bought.

Results given in Table	Quality of substance.	How made.	Temper.	Spec. grav.	Weight.	Mean diam.	Area.	State.
I.	"Burden's best" iron.	Welded and turned.	An-nealed.	7·63	148·61	·0677	·0000 916	Normal.
II.	" "	" "	"	7·63	148·61	·0677	916	Magnetic.
III.	" "	" "	C an-nealed.	7·63	148·01	·0677	912	Burnt.
IV.	Bessemer steel.	Turnedfrom large bar.	Natural.	7·84	38·34	·0420	371	Normal.
V.	Norway iron.	Welded and turned.	C an-nealed.	7·83	39·78	·0656	7695	Magnetic.
VI.	Cast nickel*.	Turnedfrom button.	……	8·83	4·806	·0200	0869	Normal.
VII.	Stubb's steel.	Hard-drawn wire.	Natural.	7·73	……	……	0969	Normal.

The first three Tables are from the same ring.

Besides these I have used very many other bars and rings; but most of them were made before I had discovered the effect of burning upon the iron, and hence did not give a normal curve for high magnetizing-powers. However, I have collected in Table VIII. some of the results of these experiments; but I have many more which are not worked up yet.

In the following Tables $Q=\frac{Q'}{\pi R^2}$ has been measured as previously described. It is evident that if, instead of reversing the current, we simply break it, we shall obtain a deflection due to the temporary magnetism alone. In this manner the temporary magnetism has been measured; and on subtracting this from Q, we can obtain the permanent magnetism.

The following abbreviations are made use of in the Tables, the other quantities being the same as previously described.

C.T.G.	Number of coils of tangent-galvanometer used.
D.T.G.	Deflection of tangent-galvanometer.
D.C.	Deflection from coil G.
D.F.	Deflection from helix F on reversing the current.
Q	Magnetic field in interior of bar (total).
D.B.	Deflection from F on breaking current.
T	Magnetic field of bar due to temporary magnetism.
P	Magnetic field of bar due to permanent magnetism.
n	Number of coils in helix F.

$$Q=T+P.$$

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* Almost chemically pure before melting.

Each observation given is almost always the mean of several. D.T.G. is the mean of four readings, two before and two after the observations on the magnetism; D.C. is the mean of from four to ten readings; D.F. mean of three; D.B. mean of two, except in Table I., where the deflection was read only once. In all these Tables the column containing the temporary magnetism T can only be accepted as approximate, the experiments having been made more to determine Q than T.

The value of *n* was generally varied by coiling a wire more or less around the ring, but leaving its length the same.

The change in the value of D.C. is due to the change in the resistance of the galvanometer from change of temperature, copper wire increasing in resistance about 1 per cent. for every 2°·6 C. rise. In Table I. the temperature first increased slowly, and then, after remaining stationary for a while, fell very fast.

TABLE I.

"Burden's Best" Iron, Normal.

C.T.G.	D.T.G.	M.	D.C.	n.	D.F.	$\frac{\text{D.F.}}{2n}$.	$\frac{\text{D.B.}}{n}$.	Q.	λ.	λ. Calculated.	$\mu=\frac{\lambda}{4\pi}$.	T.
48	4·5	·1456	23·4	30	6·5	·1083	·08	715	4910	5845	390·7	528
	16·45	·5501	...	...	54·6	·910	·59	6005	10920	10885	868·7	3894
	20·2	·6815	...	...	87·9	1·465	·80	9667	14180	14074	1129	5280
	28·6	1·011	23·3	10	74·2	3·71	1·34	24600	24830	24000	1936	8882
	31·1	1·119	...	...	88·2	4·41	1·48	29230	26120	26050	2078	9811
	31·9	1·155	...	...	92·6	4·63	1·53	30820	26690	26660	2124	10180
	41·12	1·623	...	2	29·8	7·45	2·0	49590	30570	30740	2433	13310
27	28·35	1·766	23·1	...	32·8	8·20	2·5	54820	31030	31050	2470	16710
	29·6	1·861	...	...	34·6	8·65	2·65	57820	31070	31100	2472	17710
	33·4	2·162	23·1	...	39·8	9·95	2·85	66510	30770	30776	2448	19050
	37·45	2·512	...	...	44·7	11·18	3·05	74730	29750	29930	2367	20390
	44·45	3·223	...	...	53·5	13·38	3·85	89430	27750	27390	2208	25740
	52·1	4·225	...	...	60·3	15·08	4·85	100800	23860	24730	1899	32420
9	34·65	6·744	...	...	73·1	18·28	7·10	122700	18210	18410	1448	47680
	39·8	8·136	23·0	...	77·3	19·32	7·90	129700	15940	16130	1269	53040
	44·3	9·542	...	1	40·6	20·30	9·1	136300	14280	13920	1137	61100
	55·1	14·04	...	...	43·5	21·75	9·8	145400	10360	10760	824·1	65510
3	42·95	27·18	...	...	47·4	23·70	11·5	157700	5803	6350	461·8	76540
	51·3	36·60	...	...	49·1	24·55	12·7	162700	4445	4523	353·8	84180
	60·15	51·18	23·4	...	50·3	25·15	13·2	166000	3243	3310	258·0	87120
		∞						175000		0		

TABLE II.

"Burden's Best" Iron, Magnetic.

M.	Q.	λ.	μ.	M.	Q.	λ.	μ.
·1456	426	2920	232	2·930	82720	28240	2247
·5699	3346	5987	476	4·210	100900	23950	1906
·6962	5700	8189	652	6·769	122800	18140	1444
1·080	24350	22550	1795	7·273	124300	17090	1360
1·191	29280	24580	1956	7·626	127100	16670	1326
1·537	46150	30020	2389	11·10	139500	12570	1000
1·590	49070	30260	2408	13·61	144700	10630	846
1·933	59680	30860	2456	22·10	154600	6965	554
2·377	71660	30150	2399				

TABLE III.

"Burden's Best" Iron, Burnt.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·143	1001	7039	560	1020	3·810	116900	30730	2446	
·553	9395	16980	1351	5115	4·283	120200	28060	2233	
·682	16550	24240	1929	6835	4·722	123900	26240	2088	30830
·962	37330	38780	3086	9454	6·565	133100	20270	1613	
1·070	42920	40130	3194	10300	9·326	141200	15140	1200	39810
1·153	48830	42340	3369	10530	11·00	144400	13120	1045	
1·317	59490	45180	3595	11650	13·44	147500	10970	873	44070
1·340	59580	44450	3538	13700	23·41	155500	6642	529	51030
2·127	90180	42400	3374	18470	32·73	159400	4870	387	
2·501	98560	39400	3136	19920	32·56	158400	4864	387	
2·864	104000	36310	2890	24600	51·03	165800	3250	259	56100
3·151	108200	34330	2732	24610					

TABLE IV.

Bessemer Steel, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1356	327	2412	192	309	2·756	39960	14500	1154	13080
·2793	817	2995	238	727	3·219	50550	15700	1250	16350
·5287	1726	3264	260	1471	3·551	56310	15860	1262	15980
·9398	3833	4079	325	3106	4·469	71380	15970	1271	18340
1·421	7702	5421	431	5576	5·698	85530	15010	1195	23610
1·880	14080	7487	596	8972	11·44	119550	10450	832	28020
1·947	15420	7920	630	8938	20·69	138300	6685	532	41360
2·300	24830	10800	859	11320	38·99	153700	3942	314	52930

TABLE V.

Norway Iron, Magnetic.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1344	865	6439	512		2·290	105900	46240	3680	35240
·2673	2550	9910	759	1892	4·393	134100	30520	2429	54970
·5161	13000	25200	2005	5857	5·910	142100	24090	1917	62810
·5572	15310	27480	2187	8110	7·874	149100	18940	1507	68490
·6725	30140	44820	3567	8921	13·77	156800	11390	906	77060
·9305	53800	57820	4602	13970	26·84	165800	6038	480	84710
1·362	77700	57110	4545	21630	36·86	168500	4572	364	87860
1·788	93000	52020	4140	28200					

TABLE VI.

Cast Nickel, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
1·433	852	595	47·4		13·43	27100	2018	160·6	11260
2·904	2377	819	65·1		16·53	31050	1878	149·5	13530
3·527	3685	1070	85·1		21·02	34950	1663	132·3	16480
5·555	10080	1815	144·4		32·17	41980	1305	103·8	22300
6·783	13680	2017	160·5	5120	33·92	42650	1257	100·0	23360
7·401	15270	2063	164·2	5614	60·91	50860	855	66·4	29540
9·273	19600	2114	168·2	7644	82·36	53650	651	51·8	33460
11·78	24720	2098	167·0	9902	105·2	55230	525	41·8	35120

TABLE VII.

Stubb's Steel Wire, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1673	159	953	75·9		13·65	54300	3978	316·6	20900
·6237	678	1087	86·5	598	19·35	77770	4020	319·9	29480
1·084	1197	1104	87·9	1101	27·43	100800	3676	292·6	38590
2·043	2448	1199	95·4	2257	33·39	111300	3335	265·4	45110
2·714	3446	1270	101·0	3095	35·58	115000	3228	256·9	45950
4·221	6278	1487	118·4	5145	38·64	119400	3092	246·0	48060
10·26	33700	3286	261·5	16170					

The best method of studying these Tables is to plot them: one method of doing this is to take the value of the magnetizing-force as the abscissa, and that of the permeability as the ordinate; this is the method used by Dr. Stoletow; but, besides making the complete curve infinitely long, it forms a very irregular curve, and it is impossible to get the maximum of magnetism from it. Another method is to employ the same abscissas, but to use the magnetism of the bar as ordinates; this gives a regular curve, but has the other two disadvantages of the first method; however, it is often employed, and gives a pretty good idea of the action. In Plate II. I have given a plot of Table V. with the addition of the residual or permanent magnetism, which shows the general features of these curves as drawn from any of the Tables. It is observed that the total magnetism of the iron at first increases very fast as the magnetizing-force increases, but afterwards more and more slowly until near the maximum of magnetism, where the curve is parallel to the axis of Q. The concavity of the curve at its commencement, which indicates a rapid increase of permeability, has been noticed by several physicists, and was remarked by myself in my experiments of January 1871; it has now been brought most forcibly before the public by Dr. Stoletow, whose paper refers principally to this point*. M. Müller has given an equation of the form

$$I=220d^{\frac{3}{2}}\tan\frac{m}{\cdot 00005d^{2}}$$

to represent this curve; but it fails to give any concavity to the first part of the curve. A formula of the same form has been used by M. Casin†; but his experiments carry little weight with them, on account of the small variation of the current which he used, this being only about five times, while I have used a variation in many cases of more than three hundred times.

Weber has obtained, from the theory that the particles of the iron are always magnetic and merely turn round when the magnetizing-force is applied, an equation which would make the first part of the curve coincide with the dotted line in Plate II.‡; and Maxwell, by addition to the theory, has obtained an equation which replaces the first part of the curve by the broken line§. I believe that I have obtained at the least a *very* close approximation to the true equation of the curve, and will show further on that Q and M must satisfy the equation

$$\frac{Q}{M}=A\sin\left(\frac{Q+a\frac{Q}{M}+H}{D}\right). \quad . \quad . \quad . \quad . \qquad (10)$$

It is very probable that Weber's theory may be so modified as to give a similar equation.

Space will not permit me to discuss the curves of temporary and permanent magnetism; but I will call attention to the following facts which the Tables seem to establish.

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* On the Magnetizing-Function of Soft Iron, especially with the weaker decomposing powers. By Dr. A. Stoletow, of the University of Moscow. Translated in the Phil. Mag. January 1873. See particularly p. 43.

† *Annales de Chimie et de Physique*, February 1873, p. 182.

‡ This is according to Maxwell's integration of Weber's equation, Weber having made some mistake in the integration.

§ Treatise on Electricity and Magnetism, Maxwell, vol. ii. chap. vi.

1. *Nearly or quite all the magnetism of a bar is, with weak magnetizing-forces, temporary; and this is more apparent in steel than in soft iron.*

2. *The temporary magnetism increases continually with the current.*

3. *The permanent magnetism at first increases very fast with the current, but afterwards diminishes as the current increases, when the iron is near its maximum of magnetism.*

I have now described the methods of plotting the Tables hitherto used; and I will now describe the third, which is, I believe, new. This is by using the values of the magnetism of the bar as abscissas, and those of the permeability as ordinates. In this way we obtain a perfectly regular curve, which is of finite dimensions, and from which the maximum of magnetism can be readily obtained. Plate III. shows this method of plotting as applied to Table I. If we draw straight lines across the curve parallel to the axis of Q and mark their centres, we find that they always fall very exactly upon a straight line, which is therefore a diameter of the curve. The curve of nickel shown upon the same Plate has this property in common with iron. I have made several attempts to get a ring of cobalt; but the button has always been too porous to use. However, I hope soon to obtain one, and thus make the law general for all the magnetic metals. There are two equations which may be used to express the curve: one is the equation of an inclined parabola; but this fails for the two ends of the curve; the other is an equation of the general form

$$\lambda = A \sin\left(\frac{Q + a\lambda + H}{D}\right), \quad . \quad . \quad . \quad . \quad (11)$$

in which A, H, D, and a are constants depending upon the kind and quality of the metal used. A is the maximum value of λ, and gives the height of the curve E D, Plate III.; a establishes the inclination of the diameter; H is the line A O; and D depends upon the line A C. The following equation, adapted to degrees and fractions of a degree, is the equation from which the values of λ were found, as given in Table I.:

$$\lambda = 31{\cdot}100 \sin\left(\frac{Q + {\cdot}87\lambda + 5000}{1000}\right).$$

The large curve in Plate III. was also drawn from this, and the dots added to show the coincidence with observation; it is seen that this is almost perfect. As λ enters both sides of the equation, the calculation can only be made by successive approximations. We might indeed solve with reference to Q; but in this case some values of λ as obtained from experiment may be accidentally greater than A, and so give an imaginary value to Q.

By plotting any Table in this way and measuring the distance O C, we have the maximum of magnetism.

I have given in the same Plate the curve drawn from the observations on the nickel ring with Q on the same scale, but λ on a scale four times as large as the other. The curve of nickel satisfies the equation

$$\lambda = 2120 \sin \left(\frac{Q + 5\cdot35\lambda + 2000}{360} \right)$$

quite well, but not so exactly as in the case of iron. This ring, when closely examined, was found to be slightly porous, which must have changed the curve slightly, and perhaps made it depart from the equation.

In Table VIII. I have collected some of the values of the constants in the formula when it is applied to the different rings and bars, and have also given some columns showing the maximum of magnetism. When any blank occurs, it is caused by the fact that for some reason or other the observations were not sufficient to determine it. The values of a, H, D, and the value of λ, when $Q=0$, can in most cases only be considered approximate; for as they all vary so much, I did not think it necessary to calculate them *exactly*. For comparison, I have plotted Dr. Stoletow's curve and deduced the results given in the Table, of course reducing them to the same units as mine.

It will be observed that the columns headed "maximum of magnetism" contain, besides the maximum magnetic field, two columns giving the tension of the lines of force per square centimetre and square inch of section of the lines. These have been deduced from the formula given by Maxwell* for the tension per square metre, which is $\frac{Q^2}{4\pi}$ absolute units of force.

This becomes

$$\left. \begin{array}{l} \dfrac{Q^2}{2465500000} \text{ kilogrammes per square centim.,} \\ \dfrac{Q^2}{173240000} \text{ lbs. per square inch,} \end{array} \right\} , \quad (12)$$

from which the quantities in the Table were calculated.

It is seen that the maximum of magnetism of ordinary bar iron is about 175,000 times the unit field, or 177 lbs. on the

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[*] Treatise on Electricity and Magnetism, vol. ii. p. 256.

TABLE VIII.

Ring or bar.	Quality of substance.	Temper.	Maximum of magnetism.			atest permeability.		a.	H.	D.	Spec. grav.	Value of λ when Q=0.	State of bar.
			Magnetic field. Q.	Tension of lines in kil. per square centim.	Tension in lbs. on the square inch.	A.	μ.						
Ring.	Very fibrous wire ...	Annealed.	......	......	......	13900	1106	·60	......	......	7·51	1500	Burnt.
Bar.	Soft wire	”	......	......	......	31000	2467	—·47	......	......	7·88	2300	”
Ring.	“Burden's best” iron.	”	175000	12·42	176·9	31100	2475	·87	5000	1000	7·63	4400	Normal.
Ring.	” ”	”	175000	12·42	176·9	30900	2459	·87	3000	990	7·63	2700	Magnetic.
Ring.	” ”	Carefully annealed.	177000	12·71	180·8	45500	3621	·23	7000	*950	7·63	6800	Burnt.
Ring.	Norway	”	181000	13·29	189·1	69300	5515	......	4000	......	7·87	9000	Normal.
Ring.	Norway	”	174000	12·28	174·7	58500	4656	·344	3000	980	7·83	5500	Magnetic.
Ring.	Bessemer steel	Natural.	172000	12·00	170·8	16100	1281	1·10	6300	990	7·84	2500	Normal.
Bar.	Stubb's steel	”	......	......	......	4160	331	2·45	10000	1010	7·73	950	”
Bar.	“Burden's best” ...	”	......	......	......	24800	1974	—·42	5000	......	7·65	3500	Burnt.
Bar.	” ”	Annealed.	......	......	......	27000	2149	—·56	6000	......	7·65	4800	”
Ring.	Nickel	......	63000	1·61	22·9	2120	169	5·35	2000	360	8·83	470	Normal.
Ring.	Nickel	Carefully annealed.	64000	1·66	23·6	3830	305	......	1400	......	......	560	Burnt.
Ring.	Dr. Stoletow's ring.	Carefully annealed.	177000	12·71	176·9	27510	2190	·294	3300	......	......	2500	Magnetic

* This satisfies all except the last few observations, which constitute the “tail” before referred to.

square inch, and for nickel 63,000 times, or 22·9 lbs. on the square inch. For pure iron, however, I think it may reach 180,000, or go even above that. It is seen that one of the Norway rings gave a very high result; this is explained by the following considerations. All the iron rings were welded except this one, which was forged solid from a bar 2 inches wide and then turned. Even the purest bar iron is somewhat fibrous; and between the fibres we often find streaks of scale lying lengthwise in the bar and so diminishing the section somewhat if the ring be welded from the bar; when, however, it is forged solid, these streaks are thoroughly disintegrated; and hence we find a higher maximum of magnetism for a ring of this kind, and one approaching to that of pure iron. But a ring made in this way has to be exposed to so much heating and pounding that the iron is rendered unhomogeneous, and a tail appears to the curve like that in Table III. It is evident that this tail must always show itself whenever the section of the ring is not homogeneous throughout.

Hence we may conclude that the greatest weight which can be sustained by an electromagnet with an infinite current is, for good but not pure iron, 354 lbs. per square inch of section, and for nickel 46 lbs.

Joule* has made many experiments on the maximum sustaining-power of magnets, and has collected the following Table, which I give complete, except that I have replaced the result with his large magnet by one obtained later.

Table IX.

Magnet belonging to	Least area of section, square inch.	Weight sustained.	Weight sustained ÷ least area	Q.
Mr. Joule. 1.	10	2775	277	154700
Mr. Joule. 2.	·196	49	250	147000
Mr. Joule. 3.	·0436	12	275	154100
Mr. Joule. 4.	·0012	·202	162	118300
Mr. Nesbit......	4·5	1428	317	165500
Prof. Henry ...	3·94	750	190	128200
Mr. Sturgeon...	·196	50	255	148500

It is seen that these are all below my estimate, as they should be. For comparison, I have added a column giving the values of Q which would give the sustaining-power observed; some of these are as high as any I have *actually* obtained, thus giving an experimental proof that my estimate of 354 lbs. cannot be far from correct, and illustrating the beauty of the absolute system of electrical measurement by which, from the simple deflection of a galvanometer-needle, we are able to predict how much an electromagnet will sustain without actually trying the experiment.

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* Phil. Mag. 1851.

In looking over the columns of Table VIII. which contain the values of the constants in the formula, we see how futile it is to attempt to give any fixed value to the permeability of iron or nickel; and we also see of how little value experiments on any one kind of iron are. Iron differs as much in magnetic permeability as copper does in electric conductivity.

It is seen that in the three cases when iron bars have been used, the value of *a* is negative; we might consider this to be a general law, if I did not possess a ring which also gives this negative. All these bars had a length of *at least* 120 times their diameter.

The mathematical theory of magnetism has always been considered one of the most difficult of subjects, even when, as heretofore, μ is considered to be a constant; but *now*, when it must be taken as a function of the magnetism, the difficulty is increased many fold. There are certain cases, however, where the magnetism of the body is uniform, which will not be affected.

Troy, June 2, 1873.

XV. *On Objections recently made to the received principles of Hydrodynamics.* *By* Professor Challis, *M.A., LL.D., F.R.S., F.R.A.S.*[*]

A LARGE proportion of my scientific labours having been devoted to researches respecting the principles and applications of Hydrodynamics, I have naturally taken much interest in the discussion carried on in this Magazine between Mr. Moon and the Hon. J. W. Strutt (Lord Rayleigh) relative to points affecting the very foundations of that department of science. After considering the arguments on both sides, it appears to me that the questions raised demand additional elucidation, to supply which is the purpose of this communication.

Mr. Moon's equation (1) in page 117 of vol. xxxvi. of the Philosophical Magazine, viz.

$$0=\frac{d^2y}{dt^2}+\frac{1}{D}\frac{dp}{dx},$$

includes the principle of constancy of mass, as is shown in the Note in p. 101 of vol. xlv., and for motion in one dimension is perfectly general. Respecting that equation Mr. Moon remarks that "it holds without reference to any hypothesis as to the law of pressure." I should rather say that it involves no specification of a particular fluid, and is therefore applicable to all perfect fluids.

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[*] Communicated by the Author.

I see nothing to object to in the argument which follows (in p. 117), from which Mr. Moon comes to the general conclusion that the pressure (p) is a function of the density (ρ) and velocity (v). This theorem, which, as far as I know, Mr. Moon has the merit of first demonstrating, is in the same sense general as the equation (1); that is, it contains no specification of the fluid. It shows in fact that, in defining the particular fluid, we are limited to the hypothesis that the pressure is some function of the density and the velocity. Now evidently this general theorem includes the more particular case in which the pressure is a function of the density only. Therefore, by Mr. Moon's own showing, it is legitimate to make the *hypothesis* that the pressure of the fluid is always in exact proportion to its density, and by this supposition to *define* the fluid. This argument proves, since it wholly depends on the fluid being in a state of motion, that the equation $p=a^2\rho$ may be assumed relatively to *fluid in motion*, and that it is not necessary, if it were possible, to establish its truth for that case by direct experiment.

Mr. Moon is evidently not aware that such an inference is deducible from his argument, inasmuch as he proceeds to employ the equation (1) and the theorem $p =$ funct. (ρ and v) in all their generality, as if no other course of reasoning were legitimate; and in this manner he obtains expressions for the pressure, density, and velocity which satisfy the general equation (1) (see Phil. Mag. vol. xxxvi. p. 124). According to the view I have indicated, these expressions embrace the values of the pressure, density, and velocity for every particular fluid comprehended under the general definition $p=$ funct. (ρ, v). I have found, in fact, that they are inclusive of the values obtained for air of given temperature in the ordinary way from the equation $p=a^2\rho$.

But after proving analytically that it is allowable to assume that $p=a^2\rho$ for fluid in motion, the question as to whether this equation is true for a particular fluid (as the air) is, as Lord Rayleigh rightly urges, a *physical* one, and can only be settled by the aid of experiment. The proper process for this purpose is to introduce into the general differential equations applicable to the motion of a fluid the relation $p=a^2\rho$ (already shown to be, *pro hac vice*, legitimate), and then, after applying the integrals of the equations in solving various hydrodynamical problems, to compare numerically the results of the solutions with experiment. Such evidence of the exactness of the equation $p=a^2\rho$ is *accumulative*, increasing with the number and variety of the comparisons that admit of being made satisfactorily. One contradictory experiment would be fatal to its truth. As far as my acquaintance with the solutions of hydrodynamical problems extends, I have seen no reason to doubt the exactness of that equation for air of given temperature in motion, within a wide range of density.

The legitimacy of the hypothesis that $p=a^2\rho$ having been proved by an argument for which Mr. Moon is himself responsible, it must be under some misapprehension that he contends by other arguments that that equation is not admissible. The fact is, these arguments are vitiated by the circumstance that inferences, drawn from an investigation which embraces perfect fluids of all kinds, are treated as applicable to a particular fluid without previously introducing any condition defining the fluid.

To illustrate the foregoing views, I propose to discuss an instance of the application of the equation $p=a^2\rho$ which is considered by Mr. Moon to lead to absurd conclusions, viz. that in which "a vertical cylinder closed at its lower end has an air-tight piston, which is capable of working freely in the upper part of it, and is exactly supported by the air beneath; and at a given time a given weight is placed upon the piston." The question is, to determine what motion of the piston is thereby produced. From what has been already said, it will be assumed that the equation $p=$ funct. (ρ and v) justifies making the hypothesis that $p=a^2\rho$ for *air in motion*; and the solution of the problem will accordingly proceed as follows.

Let the weight of the piston be Mg, and the additional weight mg; and let the distance of the under surface from the bottom of the cylinder be a when the weight is added, and z_1 after the time t. Also let k^2p_0 be the whole pressure of the fluid upon the piston before the weight is put on, and k^2p_1 that at the time t. In general, if p be the pressure at any point of the fluid at the height z above the bottom of the cylinder at any time t, we shall have $p=$ funct. (z, t), and therefore

$$\left(\frac{dp}{dt}\right)=\frac{dp}{dt}+\frac{dp}{dz}\frac{dz}{dt}.$$

The factor $\frac{dp}{dz}$, expressing the variation of the pressure from point to point at a given time, depends partly on the circumstance that the impulse immediately given by the bottom of the piston to the contiguous fluid is *propagated*, so that for any other position the consequent pressure is a function of z and t, and partly on the variation of the pressure at the given time due to the force of gravity. As the total variation of pressure referable to these two causes will always be comparatively very small for a cylinder of moderate dimensions, for the sake of simplicity it will be neglected, and the density of the fluid will be assumed to be at each instant the same throughout, and to vary only with the time. Accordingly for the acceleration of the piston we have

$$\frac{d^2z_1}{dt^2}=\frac{k^2p_1}{\mathrm{M}+m}-g;$$

and since by hypothesis $k^2p_0=\mathrm{M}g$, and $\frac{k^2p_1}{k^2p_0}=\frac{a}{z_1}$, it follows that

$$\frac{d^2z_1}{dt^2}=\frac{\mathrm{M}ag}{(\mathrm{M}+m)z_1}-g.$$

We may now suppose, in order to make the problem more general, that the mass m impinges on the mass M of the piston with a given velocity V, and that the two masses subsequently move on together. Hence their initial common velocity is $\frac{m\mathrm{V}}{\mathrm{M}+m}$. In this expression so much of the momentum of the fluid as is generated by the impact *at the first instant* is left out of account as being indefinitely small. The same is the case with respect to the impact of one *solid* body on another, if impact be regarded as a short and violent pressure; for the momentum of the impinging body is altered *by degrees*, beginning with zero, by the reaction of the other, and it is after an interval, however short, that the permanent alteration of its momentum is effected. In the problem before us, the reaction of the fluid is taken account of with sufficient approximation by supposing the density of the air and its pressure on the piston to be *at all times* inversely proportional to the space which the air occupies. This being admitted, the integration of the above equation gives

$$\frac{dz_1^2}{dt^2}=\frac{m^2\mathrm{V}^2}{(\mathrm{M}+m)^2}+2g(a-z_1)+\frac{2\mathrm{M}ga}{\mathrm{M}+m}\log\frac{z_1}{a}.$$

This equation includes the effect of the impact of the momentum mV, and is applicable whether V is positive or negative. The case of V negative might be practically exemplified by suddenly attaching the mass m to M by a string passing over a pulley. In that case z_1 is always greater than a.

If $\mathrm{M}=0$, the formula gives for the square of the velocity of m, $\mathrm{V}^2+2g(a-z_1)$, which is the same as that for motion in free space, as it ought to be. For since $k^2p_0=\mathrm{M}g$, if $\mathrm{M}=0$, we should have $p_0=0$, and there would be no pressure to impede the descent of m.

If $\mathrm{V}=0$, or the mass m be added to M without impact, the expression for the square of the velocity of the piston becomes

$$2g(a-z_1)+\frac{2\mathrm{M}ga}{\mathrm{M}+m}\log\frac{z_1}{a}.$$

This expression is still applicable if m be a negative quantity—that is, if the mass M of the piston be supposed to be suddenly diminished by the quantity m. In that case z_1 is greater than a, and the velocity becomes zero for the value of z_1 which satisfies the equation

$$2g(z_1-a)=\frac{2\mathrm{M}ga}{\mathrm{M}-m}\log\frac{z_1}{a}.$$

If we suppose that $m=-\mathrm{M}$, which is to suppose that the surface of the fluid is exposed to vacuum by a sudden abstraction of the piston, the above expression for the square of the velocity fails to give any definite result, because the factor $\frac{2\mathrm{M}ga}{\mathrm{M}+m}$ becomes infinite. This case is referred to by Mr. Moon at the end of his article in the Philosophical Magazine for February 1873; and a similar one is adduced in p. 27, vol. xxxvi., where a finite density D is assumed to be immediately contiguous to a density 2D. Such instances of changes of the density, and consequently of the pressure, *per saltum*, are not embraced by the ordinary principles of analytical hydrodynamics; if susceptible of treatment, they would require the application of new principles. I do not profess to be able to indicate what would be the appropriate process; but I regard it as certain that no argument against the validity in general of the equation $p=a^2\rho$ can be drawn from exceptional cases in which the density and pressure vary *per saltum*.

The general expression for the acceleration of the piston *downwards* being

$$g-\frac{\mathrm{M}ag}{(\mathrm{M}+m)z_1},$$

at the first instant, when $z_1=a$, this becomes $g-\frac{\mathrm{M}g}{\mathrm{M}+m}$, or $\frac{mg}{\mathrm{M}+m}$. The piston begins to descend by the action of this force, for the same reason that a free body begins to descend by the action of gravity; and, as Lord Rayleigh justly maintains, any argument adduced to prove that the descent cannot commence in the former case must equally apply in the other. I think I have sufficiently shown that Mr. Moon's dissent from this view is attributable to his misunderstanding the character of the equation $p=$ funct. (ρ and v).

I propose to conclude this communication by making some remarks on the parts respectively performed by *reasoning* and by *experiment* or *observation,* in establishing the truth of the results obtained in applied mathematics, not having seen in modern scientific productions sufficiently precise information as to the relation between these two parts.

(1) The experimental results obtained by Galileo and Atwood respecting the vertical descent of bodies acted upon by gravity, and the experimental proof that the vertical acceleration of a projectile is independent of the motion in a curvilinear path, and and that its horizontal motion is uniform, establish the laws which govern *constant* accelerating forces. It was absolutely necessary to ascertain these laws by *experiment* before any *abstract reasoning* respecting accelerating force was possible. But the laws relating to constant accelerating forces having thus become known, it can be proved by means of Taylor's theorem, or some equivalent process, that an accelerating force, whether variable or constant, estimated in a given direction, is quantitatively expressed by *the second differential coefficient* of the function which gives the distance at any time of the accelerated particle from a fixed plane perpendicular to that direction. Taylor's theorem is legitimately employed for this purpose on the general principle that abstract calculation is comprehensive of all concrete physical relations. This, in fact, is the proper *à priori* argument for the truth of that general symbolic expression for accelerative force.

On the same expression the whole of Physical Astronomy depends, as well as every department of applied science which requires the calculation of motions produced by accelerative forces. The satisfactory comparison of the results of calculation so made with observed facts constitutes the argument *à posteriori* for the truth of the analytical symbol for accelerative force, and with so much the greater evidence as the number of such comparisons is greater. The analytical proof of Kepler's laws is a very important part of such evidence, inasmuch as it involves as a corollary the demonstration of the law of gravity, which could not be given by observation alone, nor by calculation alone; but requires that the two processes be combined. In short, the discovery by geometrical or analytical *reasoning* that a variable force is expressible by the second differential coefficient of space with respect to time, specially characterizes the Newtonian epoch of physical science.

(2) A perfect fluid being defined by the properties of mutual pressure, and of easy separability, of contiguous parts, it is possible to demonstrate, for fluid at rest, the law of equal pressure in all directions from a given point, exclusively by *reasoning* founded on these properties. (See 'Principles of Mathematics

and Physics,' pp. 104–107.) In certain cases the law admits of being verified directly by *experiment*; but generally the verification is effected by comparisons of results obtained mathematically, on the hypothesis of the truth of the law, with observed facts. Also for the case of fluid in motion an *à priori* demonstration of the same law may be given on principles which are indicated in pp. 172 and 173 of the work just cited. But in this case experiment cannot be directly employed for its verification; and on this account some mathematicians have thought that the law was not as certainly established for fluid in motion as for fluid in equilibrium. It may, however, be asserted that, although for fluid in motion the demonstration can only be effected by the combination of reasoning with the results of experiment, both the *à priori* proof of the law, and its verification *à posteriori* by comparison of calculated results with observation, are equally valid whether the fluid is in motion or at rest.

(3) Assuming that Boyle's law has been proved by experiment to be true for air of given temperature at rest, what evidence is there that it is true for air in motion? This question, to which, as far as I am aware, a satisfactory answer has not hitherto been given, I propose to answer as follows:—Since it does not appear practicable to verify the law for fluid in motion by direct experiment, the only course that can be adopted is to justify the assumption of the law for that case by *à priori* reasoning. This having been done, the truth of the assumption has to be confirmed, as in all like instances, by comparison of results mathematically deduced from it with experiment. The chain of the reasoning is therefore incomplete unless it is shown by an *à priori* argument that the equation $p=a^2\rho$ is not excluded by the state of motion. Now this link is supplied by Mr. Moon's equation $p=$ funct. (ρ and v), the investigation of which rests on the general principle that all physical relations expressible by functions of space and time are comprehended by the rules of abstract calculation. Thus that equation is quite general, including every supposable relation between ρ and v; and accordingly we may assume that v vanishes from the equation, or that v is a function of ρ. In either case p becomes a function of ρ only. Now, since in the original investigation ρ was assumed to be a function of space and time, this relation between p and ρ, including as a particular case $p=a^2\rho$, must apply to fluid in motion. Hence Boyle's law may be assumed as a basis of reasoning in hydrodynamics.

XVI. *On the Nodal Lines of a Square Plate.*

By Lord Rayleigh, *F.R.S.* (Hon. J. W. Strutt)*.

In a recent Number of Poggendorff's *Annalen*† there is a short paper by Strehlke, complaining of the inaccuracy with which the nodal lines of vibrating square plates are depicted in certain elementary works, and drawing attention to the results of his own careful measurements. His remarks relate principally to that mode of vibration which has for nodal line a closed curve not greatly differing from a circle, but which has been erroneously regarded by some as a slightly modified form of the inscribed square.

On the experimental determination of nodal lines there is no greater authority than Strehlke; and if there were no light from theory, his results might be considered to exhaust the question. Strehlke, indeed, quotes Professor Kirchhoff as expressing the opinion that there is at present no prospect at all of a theoretical solution of the problem—an opinion which may be correct if understood to refer to a *perfectly general* theory, but which is certainly erroneous if intended to apply to the particular mode of vibration under discussion. So long ago as 1833 Wheatstone pointed out the right path ‡, though he did not follow it correctly. He considers the vibration as resulting from the superposition of two independent but equal and synchronous vibrations, in each of which the plate vibrates according to the same law as a simple bar, each line of the plate parallel to one pair of edges being affected by the same motion. On account of the symmetry, the period is the same, whichever pair of edges be taken; and thus the two vibrations compound into one having the same period, whatever may be the ratio of amplitudes. In the present case the two vibrations must be considered to have the same phase and equal amplitudes, so that the motion at the centre of the square is simply doubled. The nodal line is then the locus of points at which the component vibrations neutralize each other.

This view is perfectly correct; but Wheatstone went wrong in his application of it, from not sufficiently considering what the law of vibration of a bar really is. His conclusion that the nodal line coincides with the inscribed square involves the suppositions that when a bar vibrates freely (in its gravest mode) the amplitudes at its centre and ends are numerically equal, and that the nodes lie midway between these points. As a matter of fact neither of these suppositions is correct; the amplitude of vibration at the ends is decidedly (more than in the ratio of 3 : 2) greater than at the centre, from which it follows that the nodal line does not pass through the centres of the sides, and the nodes of a vibrating bar deviate sensibly from the positions assigned to them. It will be observed that the four points in which the required locus intersects the diagonals of the square are determined by a knowledge of the nodes of the component vibrations; for a point which lies on both these systems necessarily satisfies the condition for the nodal line of the resultant vibration. Further it is obvious that no part of the nodal line can lie in those compartments of the plate where the component vibrations have the same sign, whether positive or negative; that is to say, the curve passes through the rectangles, and not through the squares, into which the disk is divided by the two primary systems of nodes. The reproduction of Wheatstone's argument in a work so well known as Tyndall's 'Lectures on Sound' relieves me from the necessity of stating it at greater length here.

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* Communicated by the Author.

† Pogg. *Ann.* vol. cxlvi. p. 319.

‡ Phil. Trans. 1833.

The algebraical calculation of the form of a bar vibrating freely was given originally by Euler; but I do not find that the result has been reduced to numbers further than was necessary for calculating the position of the nodes. If the distance of any point from the end, expressed as a fraction of the total length, be x, and z denote the transverse displacement, we have the following expression, giving the relative displacements of the different points of the bar*:

$$z = -\sqrt{2}\sin\left(\frac{180mx}{\pi} - \frac{180\beta}{2\pi} - 45\right)^{0}$$

$$+\epsilon^{mx}\sin\left(\frac{180\beta}{2\pi}\right)^{0} + \epsilon^{-mx}\cos\left(\frac{180\beta}{2\pi}\right)^{0}; \quad . \quad . \quad . \quad (1)$$

where, approximately,

$$m = 4{\cdot}7300, \quad \beta = {\cdot}0176. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For numerical computation put $x = \frac{r}{20}$; then on reduction we have z expressed as the sum of three terms, the first of which is $-\sqrt{2}\sin\,(r \times 13^{\circ}\,33' - 45^{\circ}\,30'\,15'')$, and the logarithms of the two others $r \times {\cdot}10271 + \bar{3}{\cdot}9444$ and $-r \times {\cdot}10271 + \bar{1}{\cdot}99998$ respectively.

From this formula z was calculated for integral values of r from 0 to 10, and the results reduced in such a proportion as to make the last term (that corresponding to the middle of the rod) equal to unity. The intervals were then bisected by means of the appropriate interpolation-formulæ involving the use of four terms. If p, q, r, s be four consecutive values of z, the interval between q and r is bisected by

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* Donkin's 'Acoustics,' p. 190. More exact values are $m = 4{\cdot}73004$, $\beta = {\cdot}01765$.

$$z=\frac{q+r}{2}+\frac{q+r-(p+s)}{16}. \quad . \quad . \quad . \quad . \quad (3)$$

At the ends of the Table a modification is required. For the term next before that corresponding to the middle of the rod we have $s=q$, as we know that every thing is symmetrical about the centre. For the term corresponding to $x=0{\cdot}025$, I have contented myself with a simple interpolation by first differences, a course justified by the fact that in the neighbourhood of the ends the curvature is exceedingly small. The Table stands as follows:—

x.	z.	Diff.	x.	z.	Diff.
·000	−1·6448		·250	+·1632	·1577
·025	1·4538	·1910	·275	·3110	·1478
·050	1·2628	·1910	·300	·4475	·1365
·075	1·0724	·1904	·325	·5713	·1238
·100	·8835	·1889	·350	·6814	·1101
·125	·6966	·1869	·375	·7765	·0951
·150	·5131	·1835	·400	·8559	·0794
·175	·3340	·1791	·425	·9183	·0624
·200	− ·1607	·1733	·450	·9636	·0453
·225	+ ·0055	·1662	·475	·9908	·0272
			·500	1·0000	·0092

It appears that the displacement at the ends of the rod is by no means of the same numerical magnitude as at the middle. It will be found by interpolation that $z=-1$ when $x={\cdot}0846$, which last number therefore gives the nearest approach made by the nodal line of the resultant vibration to the sides of the square. The nodal lines of the original systems correspond to $x={\cdot}2242$, differing sensibly from $\frac{1}{4}$.

If we take two adjacent sides of the square as axes of Cartesian coordinates, the lines of constant displacement are represented by

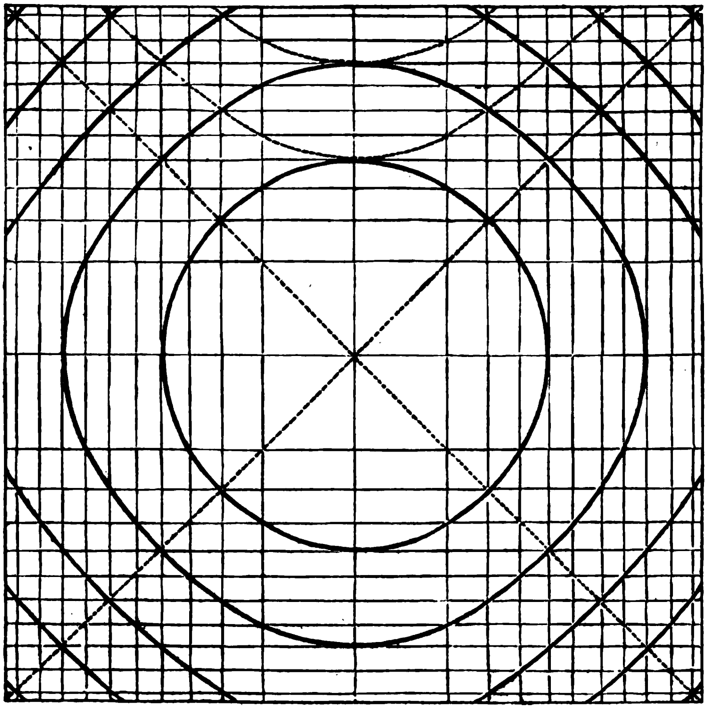
$$z_x+z_y=\text{const.}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

z being the function already investigated and tabulated. For the nodal line the constant in (4) is to be put equal to zero.

In order to construct (4) graphically, the most convenient method is that adopted by Maxwell in similar cases. The systems of curves (in this case straight lines) represented by $z_x=$ const. and $z_y=$ const. respectively are first laid down, the values of the constants forming an arithmetical progression with the same common difference in the two cases. In this way a network is obtained which the required curves cross diagonally. The execution of this plan requires an inversion of the Table connecting z and x, of which the result is as follows:—

z.	x.	Diff.
+1·00	·5000	
·75	·3680	·1320
·50	·3106	·0574
·25	·2647	·0459
·00	·2242	·0405
− ·25	·1871	·0371
·50	·1518	·0353
·75	·1179	·0339
1·00	·0846	·0333
1·25	·0517	·0329
−1·50	·0190	·0327

The system of lines represented by the above values of x (completed symmetrically on the further side of the central line) and the corresponding system for y are laid down in the figure.



From these the curves of equal displacement are deduced. At the centre of the square we have z a maximum, and equal to 2 (on the scale adopted). The first curve proceeding outwards is the locus of points at which $z=1$. The next is the nodal line, separating the regions of opposite displacement. The remaining curves taken in order give the displacements -1, -2, -3. The numerically greatest negative displacement does not occur until the corners of the square, where it amounts to $2\times 1{\cdot}6448 = 3{\cdot}2896$.

The same calculations and constructions give the theoretical solution for another and even better-known mode of vibration of a square plate. We have hitherto supposed that the two component vibrations were in the same phase. If we take them in opposite phases, the curves of equal displacement become

$$z_x - z_y = \text{const.}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

which are to be found by crossing the same network of straight lines along the other diagonals. In this mode of vibration the nodal lines are the diagonals of the square; and the hyperbolic curves in the figure (when completed on the opposite side) are the loci of points where the displacement amounts respectively to 1 and 2. The curves similarly situated in the other two portions cut off from the square by the diagonals would correspond to displacements -1 and -2. The maxima of vibration occur at the middle points of the sides of the square, and amount numerically to $1+1{\cdot}6448=2{\cdot}6448$.

Other modes of vibration of a square plate may be obtained by starting from the higher fundamental modes of a bar with three or more nodes. Some of these I hope to examine in my work on Acoustics, now in preparation.

Strehlke expresses the form of the nodal line as given by his observations by means of polar equations, the origin being taken at the centre of the square. This was a natural course to take in view of the approximate circular form, but does not commend itself to the theorist. The equations representing the results obtained with three different plates are

$$r = {\cdot}40143 + {\cdot}0171 \cos 4t + {\cdot}00127 \cos 8t,$$

$$r = {\cdot}40143 + {\cdot}0172 \cos 4t + {\cdot}00127 \cos 8t,$$

$$r = {\cdot}4019 \ \ + {\cdot}0168 \cos 4t + {\cdot}0013 \ \ \cos 8t.$$

From these we obtain for the radius vector parallel to the sides of the square ·41980, ·41981, ·4200, while the theoretical result is ·4154. The radius vector measured along a diagonal is ·3856, ·3855, ·3864, but from theory ·3900.

The agreement between theory and observation is not so good as might have been expected from the case of a circular plate, for which the theoretical results were calculated by Poisson and

Kirchhoff, and tested experimentally by Strehlke; and the latter may therefore be disposed to reject the theory *in toto*. But it is necessary here to distinguish two distinct questions: it is one thing to deduce the logical consequences of premises generally admitted, and another to inquire how far those premises are really applicable to the case in hand. With respect to the first point there can, I apprehend, be no doubt that the theory developed in the present paper stands on as high a level as that of Kirchhoff relating to a circular plate. It satisfies the same differential equation over the area, and the same boundary-conditions, and is indeed in one respect* even less open to doubt. But it may be that the fundamental equations are for some reason less applicable to a square than a circular plate, though I do not see why this should be the case. On the other hand it is possible that, in spite of all Dr. Strehlke's care, some error may have crept into his measurements. If this be so, the cause of error must be of a systematic character, not eliminated by repetition or by changing the plates. One suggestion I may be allowed to make: it appears to me that experimenters have not been sufficiently careful to touch the vibrating plates only at the nodes. In the present case it is, I believe, not unusual to damp the plate by touching the middle point of one of the sides—a method which would be legitimate on the old view that the node passes, or ought to pass, through the points referred to, but which certainly cannot be justified for purposes of measurement without an experimental or theoretical proof that no error is thereby introduced. One would suppose that the action of the finger or other damper must in part resemble the effect of a load applied at the point touched; and there can be no question that such a load would produce a disturbance.

XVII. *Notices respecting New Books.*

Astronomical and Meteorological Observations made during the year 1870, *at the United-States Naval Observatory.* Washington: 1873.

THIS volume, which commences with the report of the Superintendent of the Observatory, Rear-Admiral Sands, contains observations with the transit-circle, the meridian transit-instrument, the mural circle, and the equatoreal. From the report we learn that the equatoreal was under the charge of Professors Newcomb and Hall, who observed with it the minor planets, comets, and occultations. The transit-circle was under the charge of Professor Harkness, who also had charge of the telegraphic apparatus and connexions by which correct time is furnished to the Navy department, the city of Washington (by striking the fire bells three times daily), and the principal railways in the Southern States. The transit-instrument and mural circle were under the charge of Professor Yarnall; and Professor Eastman had charge of the Meteorological Observations.

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* The relation of the constants and rigidity and compressibility.

The attention of Professor Newcomb has been given chiefly to the theory of the moon, for the purpose of revising both the theory and the tables, in the course of which a new and more exact method of computing the effect of the attraction of the planets has been worked out. The results of the Professor's work indicate that the positions of the moon at the total eclipse of 1715 and at the epochs of two occultations of Aldebaran observed at Greenwich and London in 1680, are better represented by the old tables of Burckhardt than by those of Hansen.

The introductory part of the volume contains very full descriptions of each instrument, its various adjustments, method of observing adopted, determination of corrections, &c. These notices embody various important practical details which it is next to impossible to find in any popular treatise, but which are essential for the training of an efficient amateur astronomer. We need not enlarge on the high value of the observations recorded.

The volume contains four appendices. Appendix I. embodies the operations for determining the difference of longitude between Washington and St. Louis. The work was effected by the aid of the Western Union Telegraph Company in exchanging signals—the observations and reductions at St. Louis being made by the officers of the United-States Coast Survey, and those at Washington by the officers of the Observatory.

Appendix II., "Reports on Observations of Encke's Comet during its return in 1871," has already been noticed in our pages.

Appendix III., "On the Right Ascensions of the Equatoreal Fundamental Stars," by Professor Newcomb, contains much valuable information. The object the Professor had in view was to reduce the right ascensions of different catalogues to a mean homogeneous system. We may possibly return to this Appendix on some future occasion.

Appendix IV. contains the zones of stars observed at the Naval Observatory with the meridian transit-instrument in the years 1846 to 1849—a portion of the great work originally contemplated, but discontinued on account of the difficulty experienced by the computing staff in endeavouring to keep pace with the observing, a difficulty which threatens the extinction of our own system of meteorological observations.

We cannot close this notice without directing attention to the progress of Astronomy in America. Our own National Observatory, under the able management of the Astronomer Royal, continues to maintain its position as the most important astronomical establishment in the United Kingdom. There is, however, much useful work to be effected by amateurs, who may consult the volume before us with advantage.

XVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xlv. p. 462.]

December 18, 1872.—Warington W. Smyth, Esq., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "Further Notes on the Punfield Section." By C. J. A. Meyer, Esq., F.G.S.

This paper was supplementary to one read before the Society by the author in March of the present year (see Quart. Journ. Geol. Soc. xxviii. p. 245), and contained the results of a fresh examination of the section at Punfield, and of the Wealden and Neocomian strata of the Isle of Wight. He described the section exposed at his visit to Punfield as presenting:—1. True Wealden beds; 2. a grit-bed with limestone and paper-shales, containing fish-bones and Cyprides; 3. apparently argillaceous beds; 4. a thin band of hard ferruginous sandstone with Atherfield fossils; 5. a clay-bed, the upper part regarded as representing the "lobster-clay" of Atherfield, the lower sandy portion containing an abundance of marine fossils belonging to common Atherfield species; 6. the so-called "marine band;" and 7. laminated clays and sands with lignite. The author indicated the accordance of this arrangement with what is observed elsewhere, and maintained that the grit-bed (No. 2), with its limestone and paper-shales, containing *Cypris* and *Cyrena*, was really to be regarded as the passage-bed between the Wealden and the Neocomian.

2. "On the Coprolites of the Upper Greensand Formation, and on Flints." By W. Johnson Sollas, Esq.

The first part of this paper was principally occupied in an endeavour to explain the perfect fossilization of sponges and other soft-bodied animals. It was shown that the hypothesis which considered that sponges had become silicified by an attraction of their spicules for silica was altogether untenable. Mr. Hawkins Johnson's supposititious reaction, according to which the carbon of animal matter is directly replaced by silicon, was shown to be inconsistent with the known facts of chemistry. The author's explanation was not intended to be final. The first fact pointed out was the very remarkable way in which the silica or calcic phosphate of the fossils under consideration followed the former extension of organic matter. This was explained for silica by the fact that, when silicic acid is added to such animal matters as albumen or gelatin, it forms with them a definite chemical compound; and it was assumed that in process of time this highly complex organic substance would decompose, its organic constituents would be evolved, and its silica would remain behind. In such a way flints might be produced; and dialysis would lend its aid. The same explanation was applied to account for the connexion between calcic phosphate and animal matter in the case of the "Coprolites."

The Blackdown silicified shells were next explained; and it was reasoned that the state of their silica offered arguments tending to prove a passage of silica from the colloidal to the crystalline state.

The second part of the paper discussed the Coprolites specially. Their exterior appearance is extremely sponge-like, almost exactly resembling some species of modern sponges. They are marked by oscules of peculiar characters.

The so-called "pores" of palæontologists are well marked. Spicules, triradiate, sexradiate, sinuous, defensive and connecting, have been observed. They are siliceous in composition. On dissolving the coprolites in acid, the spicules are set free, associated with *Polycystina* (*Haliomma hexacantha* &c.) and *Xanthidia* (*X. furcatum*). The genera and species of coprolites described were as follows:—*Rhabdospongia communis, Bonneyia bacilliformis, B. cylindricus, B. Jessoni, B. scrobiculatus, B. verrongiformis, Acanthophora Hartogii, Polyacantha Etheridgii, Retia simplex, R. costata, Ulospongia patera, U. calyx, U. Brunii.* The external appearance of these forms, which constitute a vast number of the coprolites, their curious oscules and siliceous spicules, were said to leave no doubt as to their spongious origin.

XIX. *Intelligence and Miscellaneous Articles.*

A NEW METHOD FOR EXAMINING THE DIVISIONS OF GRADUATED CIRCLES. BY G. QUINCKE.

THE task of examining a graduated circle by which the position of two telescopes, with microscopes for reading off, was to be determined accurately to a few seconds, has led me to a method of examination combining convenience and accuracy; and, as far as I am aware, it has not yet been described.

The telescopes are provided with a Gauss's ocular (*Astron. Nachr.* 579. 31. 10, 1846), in which the cross-threads can be illuminated by a plane glass placed between them and the lense of the ocular and inclined at an angle of 45° to the axis of the telescope.

The axis of the telescope is placed normal to a plane-parallel glass plate, if the cross-wires coincide with the reflected image projected by the rays reflected from the plate.

The glass plate can be set up normal to the graduated circle by being fixed with wax upon a little turntable in its centre.

The axis of the telescope and its axis of rotation are exactly perpendicular to each other, if the cross-wires and their reflected image coincide also after the telescope has been turned through 180°. With different positions of the plane glass firmly united to the circle, we thus obtain also the excentricity of the axis of rotation (if any exist) in relation to the centre of the circle. For the sake of greater intensity of light I make use of Steinheil's plane-parallel glasses, one side of which is silvered and polished.

Two plane-parallel mirrors are fastened with wax upon the turntable, perpendicular to the axis of the telescope. They are exactly perpendicular to each other, if the angular mirror which they constitute makes the two images (produced by double reflection) of the cross-wires of one of the telescopes coincide with the cross-wires themselves.

If the axes of the telescopes 1 and 2 are, through reflection of the illuminated cross-wires, set normal to the two faces of the mirror, they make exactly an angle of 90° with each other. Different positions of the angular mirror then determine every four points of the circle-division which are 90° distant from each other.

Two plane glasses make exactly an angle of 120° or 60° with one another, if two telescopes, placed each normal to one of them, show simultaneously, through double reflection, the cross-wires of telescope 1 in the cross-wires of telescope 2, and *vice versâ*.

By putting, with different positions of the angular mirror of 120° or 60°, the two telescopes normal to the respective faces of the mirror, we obtain, by reading-off the circle, points exactly 60° or 120° distant from one another.

If the two plane glasses make an angle of $180°-2\phi$, and the telescope-axes, normal to them, an angle 2ϕ with each other, a third plane glass can be fastened with wax upon the turntable in the centre of the graduated circle in such manner as to reflect the rays proceeding from the cross-wires of telescope 1 upon the cross-wires of telescope 2. Plane glass 3 is then inclined at the angle ϕ to plane glass 1 or 2; and the angular mirror formed of 1 and 3 or 2 and 3 can be used to set the telescope-axes perpendicular to the mirror-faces, and to determine points in the circle whose distance from one another is measured by the angle ϕ.

From the angles 90° and 60° we thus obtain with this third plane mirror angles of 45° and 30°—and from these, angles of $22\frac{1}{2}°$ and 15°, &c.

Should there be any difficulty in bringing the plane glasses into correct position with wax and the free hand, it can be readily obviated by a simple arrangement with screw and pressure-spring.

The method of reflection of the cross-wires permits us also to compare the angles of glass prisms with those of the angular mirrors, and, with the unalterable angle of a glass prism of exactly 90°, 60°, 30°, 20°, 10°, &c., to measure and calibrate the divisions of the circle. The latter I have not been able to carry out, as the prisms long since ordered for the purpose are not yet in my possession.

The method above described is most convenient, and accurate to the extent of the magnifying-power of the telescopes and as far as the distinguishing of the ocular-threads is possible—that is, as nearly as one can in general see with the apparatus in question. As, also, the perfectness of the plane glasses can be easily controlled with the telescope, it is probably applicable to the production of a new and accurately graduated circle.

Würzburg, June 1, 1873.

ON A CONVENIENT EYEPIECE-MICROMETER FOR THE SPECTROSCOPE. BY PROFESSOR O. N. ROOD.

I have recently contrived a very simple eyepiece-micrometer for the study of spectra produced by prisms and "gratings," which, while quite inexpensive, is capable of yielding results that are not easily surpassed except by the use of an eyepiece provided with a micrometer-screw. A thin semicircular plate of silver is made quite smooth, and rendered black by holding it over the flame of a lamp; it is afterward flowed with a drop of weak spirit-varnish to cause the lampblack to adhere. Crossing the straight edge of this dead-black surface, lines ¼ millim. &c. are ruled with a dividing-engine, and the necessary figures added with the help of a lens. The opaque semicircular plate is then introduced into the interior of a negative, or preferably in front of a positive eyepiece, so that it is in focus and does not occupy quite half of the field of view. Opposite it and somewhat nearer the eye an opening is made in the side of the eyepiece, whereby the lines are brightly illuminated —as a general thing, merely by the diffused light of the room; but if this is quite dark, the small flame of a distant lamp easily accomplishes the same end. This arrangement, it will be seen, furnishes a set of bright lines on an almost perfectly black ground with the least possible outlay of expense or trouble in manipulation; and the *degree* of their brightness, it will be found, can readily be regulated merely by shading the opening more or less with the hand. The distance of the lines apart should not be too small, as is often the case in the photographic scales of ordinary spectroscopes, but such as will facilitate the estimation of tenths of a division. Two such eyepieces have been constructed and employed by me with much satisfaction in the mapping of a large number of spectra furnished by prisms and gratings, particularly in those cases where the spectral lines were quite faint.—Silliman's *American Journal*, July 1873.

JAMIN'S COMPOUND MAGNETS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Is not the method of making powerful compound magnets published by M. Jamin substantially identical with that employed by Scoresby, and fully set forth in his 'Magnetical Investigations' so long since as 1839? He shows that thin *hard* steel plates may be superposed on the other with great advantage if separated by thin slips of wood or cardboard, and states that the power of such plates accumulated efficiently "to the amount of 192, and might have been carried much further."

I am, &c.,
R. S. CULLEY.

General Post Office,
June 25, 1873.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1873.

XX. *On the Effects of Magnetization in changing the Dimensions of Iron and Steel bars, and in increasing the Interior Capacity of Hollow Iron Cylinders. By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.*

[Continued from vol. xlv. p. 359.]

Part II. *On the Elongations and Retractions of Rods of Iron and Steel on their Magnetization and Demagnetization.*

TO study and measure with precision the minute elongations and retractions which rods of iron and steel undergo on their magnetization and demagnetization, it is necessary that the motions of the part of the measuring-apparatus which records these changes in length should not be in the least affected by outside vibrations transmitted to the apparatus, but should be controlled alone by the molecular motions in the rods which take place on changes in their magnetic condition; also the motions of this indicating part of the apparatus should be synchronous with the motions in the rods, so that we may be able to study the character as well as the amount of these elongations and retractions.

Several instruments have been devised by me which fulfil these essential conditions; but they were all abandoned (except one, to be described in detail in Part IV. of this memoir) and preference given to "the reflecting comparator and pyrometer" of our esteemed colleague Mr. Joseph Saxton. This simple and precise instrument is well known to American physicists as the apparatus which has greatly aided in giving to the geodetic work of our national Coast Survey its renowned precision, and in rendering accurate the comparisons and constructions of our Office of Weights and Measures. A detailed description, with drawings, of this instrument will be found in the "Report on the Construction and Distribution of Weights and Measures, Washington, 1857," written by Dr. A. D. Bache, the late illustrious President of our National Academy*.

The measuring-instrument.

I will now describe the actual adaptation of this instrument to our research. The drawings (figs. 1 and 2) give respectively an elevation and a plan of the apparatus. A beam of Georgia pine, well seasoned, dried and then soaked with shellac varnish, formed the base on which the instrument was lined and firmly attached. This beam is 7 feet long, 8¼ inches deep, and 5¾ inches wide. It rested on slips of hard wood at *i*, *i*, placed at distances from the ends of the beam equal to one fourth of its length. At *a* and *b* are two Vs of brass which supported the terminal brass caps of the rods experimented on. These rods were all 60·1 inches long, ·5 inch in diameter; and each rod weighed on the average 1520 grammes. While the ends of a rod rested on the Vs, 1100 grms. of its weight was supported by the two springs *s*, *s*, which took hold of the rod at distances from its ends equal to one fourth of its length. The flexure of the rod was thus in great part avoided; and it could therefore

Fig. 1.

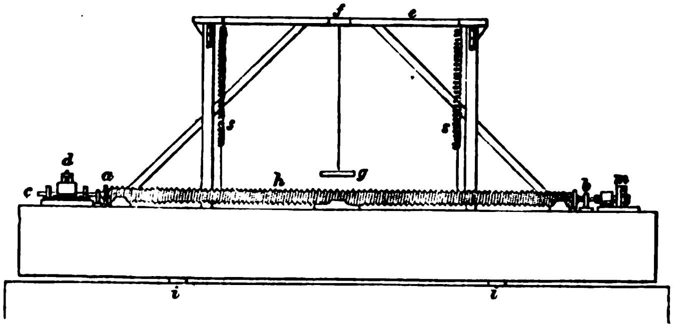
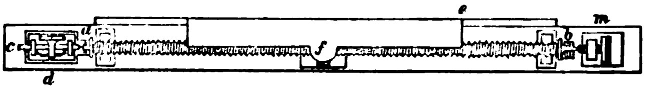


Fig. 2.



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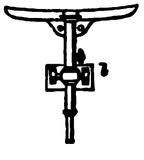
* To my friend and colleague Dr. J. E. Hilgard, of the United-States Coast Survey Office, I am indebted for the loan of the comparator used in these researches.

be accurately centred in the helix *h*. This helix is 60·25 inches long, and has an outside diameter of 1·75 inch. It is wrapped on a tube of brass of ·8 inch internal diameter, slit longitudinally throughout its whole length. At *m* is the micrometer abutting-screw, against which the end of the rod is firmly pressed by two heliacal springs, which are stretched between hooks on the brass mounting of the screw and a rod which passed through the terminal brass cap of the rod. These brass caps at both ends of the rod are terminated by pieces of agate. The other end of the rod is in contact with a slide *c*, with triangular section, which accurately moves, between guides, in a direction which is the axis of the rod prolonged. To this slide is attached a delicate fusee-chain, which is coiled once round the vertical axis of the mirror *d*. This chain is prevented from slipping by a steel pin, which securely attaches it to the axis. The slide carrying the chain is firmly pressed against the terminal agate of the rod by means of an heliacal brass spring. Thus the rod is at one end firmly pressed against the micrometer-screw, while against the other end presses the slide, which is connected with the mirror by the intervention of the fusee-chain, which latter is also tightly stretched. The well-joined framework *e* supports the springs *s*,*s*, and also a divided circle, *f*, from whose centre depended either a fibre of silk or a filament of glass, which supported a magnet of very hard steel, *g*. From the oscillations of this magnet, or from its deflections, by means of the divided circle and glass filament were determined the intensities of the residual magnetism of the rods. The deflections of the mirror, caused by the elongations or retractions of the rods, were determined by means of the telescope and scale represented at *l* (fig. 3). The telescope and scale were placed between 5 and 6 metres from the mirror; and the scale was divided into millimetres. Each unit of division given in the experiments corresponds to one centimetre of the scale.

Fig. 3.



The above apparatus was placed in a room without windows and entirely underground; and the room was always entered by descending from a door whose bottom was on a level with the ceiling of the room. During the course of the experiments the range of variation of temperature of this room did not exceed 0°·1 C. in twenty-four hours.

Examination of the stability and degree of precision of the apparatus.

A rod was accurately centred in the helix, and the micrometer-screw and the slide of the mirror were brought into contact with its ends. After the heat imparted to the rod and apparatus by handling had been dissipated, and the scale-reading had remained constant for an hour, I pushed the mirror-slide away from the rod and then again allowed it to come against its agate terminal. This operation was performed as expeditiously as was consistent with the careful avoidance of shocks to the apparatus. The thermometers, which were placed near various parts of the apparatus, were now read; and it was ascertained that they gave the same readings as before the apparatus was touched. The scale was now read in the telescope; and it was ascertained that the cross-threads bisected the same division observed before the slide was moved, thus showing that the mirror returned to the position it had before its rotation. This important fact was repeatedly confirmed with all the rods and at various stages of the research. Indeed the measures of hundreds of experiments made on the elongations and retractions of rods also confirmed the confidence I obtained in the indications of the apparatus arrived at by the preceding experiments.

Jumping on the floor of the room and the passage of carriages and carts in the streets had not the slightest effect in disturbing the scale-readings.

To ascertain if the mirror accurately followed the changes in length of the rod, I repeatedly made the following observations. The readings of the screw-head and the scale were noted; then the screw was rotated by an assistant so as to push before it the rod. The scale-readings ran up steadily with the rotation; and when the screw was rotated backward the scale-readings smoothly ran down; and when the screw-head had reached the same position it had before it was touched, I found that the scale-reading corresponded to that noted when the screw previously had this position. This observation, repeatedly made, gave me the means of testing the precision of the instrument during the progress of the investigations.

It will be seen below that one division of the scale, or 1 centimetre, corresponded to a change of ·00011 inch in the length of the rod; but $\frac{1}{2}$ millimetre, or $\frac{1}{20}$ of a division, could be precisely read in the telescope; I am therefore justified in believing that the measures I shall give in this memoir can be relied on to the $\frac{1}{200000}$ of an inch.

Determination of the value of one division of the scale in changes of length, in inch-measure, of the rods.

Pasted on the inside of the box in which the mirror, telescope, and micrometer-screw came to me was the following:—"Abutting-screw of field pyrometer. By seven comparisons of five turns with 0·1 inch on Troughton-scale, in June 1857, by Mr. Saxton,

1 turn = 0·01912 inch."

As the result of numerous determinations made on various parts of the screw, I found that $\frac{1}{100}$ of a rotation of the screw equalled 1·737 division of the telescope-scale, which gives 0·00011 inch as the value of one division of the scale. This value, however, applies only to the experiments on the rods of iron Nos. 1 to 6 inclusive. Before commencing the experiments on the rods of steel, the distance of the scale from the mirror was changed; and in this new position I found that one division of the scale corresponded to 0·000146 of an inch.

Description of the helix, and measures of the resistances of its wires.

The helix was a compound one, formed of four layers of copper wire. The two inner layers formed 1069 turns of one length of 303 feet of wire ·087 inch in diameter. The two outer layers were formed of another length of 330 feet of ·112-inch wire wrapped in 850 turns. These two helices could be used separately, or joined into one helix of 633 feet having 1919 turns.

The resistance of the inner helix was ·44 ohm; the outer had a resistance of ·31 ohm, together giving a resistance of ·75 ohm. The latter resistance, added to that of the wires leading from the battery through a Gaugain galvanometer to the helix and back to the battery, brought up the resistance to nearly one ohm.

A *battery* of 25 cells of Bunsen was used in the determination of the coefficients of elongation and retraction; and the above interpolar resistance showed that the maximum effect of magnetization would be given by connecting the 25 cells, 5 in couple and 5 in series. Whenever in this research we speak of the effect of 25 cells, it is to be understood that they are connected as just described.

The iron and steel rods used in the experiments were prepared for me with the well-known skill and fidelity of Mr. Wallace, of Ansonia, Ct. He carefully selected the materials, and annealed the iron rods by packing them, with iron scales from a rolling-mill, in a wrought-iron covered box, and exposing the box to a red heat for three days; the box was then allowed to cool very slowly. The steel rods were tempered as uniformly as possible throughout their lengths.

Arrangement of the apparatus, and general description of the phenomena which take place on the magnetization and demagnetization of the rods of iron.

The beam supporting the apparatus was so placed that the axis of the helix was in the magnetic meridian. Each rod before it was introduced into the helix was tested as to its magnetic condition by placing its length at right angles to the magnetic meridian and pointing it towards the centre of a magnetic needle. When the rod thus directed gave indications of polarity, its S. end was placed downwards, with the axis of the rod in the line of the dip, and its upper end was struck with a light mallet. The rod was tested until, after one or more operations of this kind, it gave no indications of polarity. But on placing the rod in the helix it, of course, was again magnetized, but feebly, by the earth's induction. This fact serves to determine the distance at which the magnet, which determined the residual polarity, had to be suspended above the rod. If this magnet is placed too near the rod, then an interaction between it and the soft iron of the rod takes place by the inductive action of the magnet, and the vibrations of the latter are more frequent than when it is alone acted on by the earth; but if it be removed to a certain distance above the rod, then the magnetism of the rod acts as a "damper" on the magnet, and its vibrations are slower than when it is only under the earth's influence. There is therefore an intermediate position, at which the magnet vibrates the same, whether the rod remains under it or is taken away. This distance, of course, varies with the rod used; but on the average it was about 3 inches.

Thus arranged the rod was allowed to remain until its temperature had become constant and the scale-reading in the telescope was stationary.

The interpolar connexions with the battery were made so that the helix, on closing the circuit, would magnetize the rod with the same direction of polarity it already had from the earth's action. The current was now passed from the 25 cells by plunging the amalgamated wire of the open part of the circuit into a cup of mercury; then the scale-reading was immediately noted; the circuit was at once broken and another reading obtained. The thermometers, which were placed on various parts of the apparatus, and which had been read just before closing the circuit, were now again observed and the room vacated and closed. At intervals of a half hour during the three to six subsequent hours the room was entered and readings of scale and thermometers obtained.

It may here be well to give a general account of the phenomena which the rods exhibit when the voltaic circuit is successively closed and opened. When the rod has for the first time the heliacal current passed round it, a sudden elongation takes place; and this elongation remains steadily of the same amount as long as the circuit is closed and the temperature of the rod remains constant. Now on breaking the circuit the rod retracts, with a less velocity than that with which it first elongated; but the retraction does not equal the elongation. The temperature of the rod remaining constant, it has been found that the rod retains the length it attained on the breaking of the circuit; that is, the rod has received with its permanent magnetic charge a permanent elongation. On passing the current a second time, the rod again elongates; but the elongation is now less than that which took place when the current was first passed round it. On now breaking the circuit the rod retracts to the length it had before the current was passed for the second time; that is, after the first magnetization and demagnetization of the rod the successive elongations and retractions are equal. These conditions exist until four or five subsequent make- and break-circuits have been made: but now a change takes place in the phenomena; for on making the circuit the rod elongates about the same as in the preceding experiments, *but* on breaking the circuit the retraction does not equal the elongation; so that after each experiment the rod is slightly longer than before the preceding experiment was made. Continuing the experiments, the scale gradually passes over the cross-threads; and I have thus repeatedly caused the entire scale to traverse the field of the telescope. On now allowing the rod to remain at the temperature it had when the current was for the first time passed round it, the rod slowly retracts until, after several hours have elapsed, it has the length which was observed after the first experiment made upon it.

Heat developed in the rod at the instant of its demagnetization.

The above described experiments show conclusively that the minute elongations which take place on breaking the circuit are due to the heat developed in the rod at the moment of its demagnetization; for in the preceding experiments the current did not heat the helix sufficiently to cause radiations from it to elongate the rod; therefore, to obtain the results described above, it is important to ascertain beforehand, when the current has traversed the helix for a time equal to that occupied in the experiments, that the rod during this time does not elongate.

If the current is sufficiently intense to heat directly the helix and rod, the above phenomenon of heating on demagnetization nevertheless manifests itself, and can readily be disentangled from the combined effects, as will be seen further on.

These interesting results, proving the development of heat on demagnetization, were obtained a year ago without any knowledge of the recent work of Jamin and Roger*; and these measures, made directly on the changes in length of the rods, tend to confirm the result arrived at by those experimenters. Recently Cazin (*Comptes Rendus*, vol. lxxv. p. 1265) has shown that "the heat thus produced is proportional to the square of the intensity of the magnetism and to the polar distance;" and Moutier (*Comptes Rendus*, vol. lxxv. p. 1620)* deduces this result from a thermodynamic theorem established by Clausius, and thus concludes his paper:—"The increase of *vis viva* which the bar experiences from the effect of magnetization is therefore proportional to the square of the intensity of the magnetism and to the polar distance. The effect of the demagnetizing corresponds to an equal loss of *vis viva*, which is the measure of the thermal effect produced; and this effect is the only one which accompanies the demagnetization."

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* I have not been able to find the paper containing Jamin and Roger's experiments either in the *Comptes Rendus* or in the *Ann. de Chim. et de Phys.* I have obtained the information of their results only from the fol-

The fact that an iron bar is heated by successive magnetizations and demagnetizations has been known for a long time; but only recently have experiments been made which indicate that this heat is produced at the moment of demagnetization. In a paper "On the Calorific Effects of Magneto-electricity, and on the Mechanical Value of Heat" (Phil. Mag. S. 3. vol. xxiii. 1843), Dr. Joule first showed that heat was developed in an iron bar *when* it was rotated between the poles of a powerful magnet, and also determined that the heat thus produced was proportional to the square of the inductive force. These experiments will ever be regarded with interest; for they led Joule to the first experimental determination ever made of "the mechanical value of heat." It may here be of interest to present the following account of the experiments made by Van Breda and Grove, taken from Daguin's *Traité de Physique*, 1861, vol. iii. p. 621:—"M. Van Breda having enveloped a tube of iron with a helix through which he passed an intermittent current, found a heating of the iron, due to the alternative displacement of the molecules†, the heat being shown by the dilatation of the air contained in the tube, which formed the reservoir of an air-thermometer. Grove subsequently determined the heating of an armature of soft iron, on passing an intermittent current in the wire of an electro-magnet on which the armature was placed, or in turning near it

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lowing passage in the paper of M. Cazin (*Comptes Rendus*, vol. lxxv. p. 1266):—"When we pass an intermittent current in the wire of an electromagnet, the recent experiments of MM. Jamin and Roger have demonstrated in a definite manner that the core is heated." The method by which they discovered this fact is not stated by Cazin. See *Ann. de Chim. et de Phys.* vol. xvii. (1869).

* Phil. Mag. Feb. 1873, p. 157.

† The heat observed, however, may not be entirely due to these motions; for the thermal effects may in part be due to the currents induced in the iron on magnetization and demagnetization.

the poles of a strong electromagnet. The heating effects were indicated by a thermo-electrical couple. Cobalt and nickel gave similar results, but less marked; whilst non-magnetic metals were not heated in the same circumstances." I have made many experiments on a tube of iron, weighing two hundredweight, which confirm these results; the experiments will be given in Part III. of this memoir.

I here present two Tables of experiments on rod No. 2, of Ulster iron. The successive discussion of these two Tables will give to the reader a clear physical conception of the phenomena, and serve to elucidate the account I have above given of the heat developed on demagnetization.

TABLE I.

No. of experiment.	Scale, circuit open.	Scale, on closing circuit.	Scale, on breaking circuit.	Elongation.	Retraction.
1.	37·6	39·2	38·0	1·6	1·2
2.	38·0	39·2	38·0	1·2	1·2
3.	38·0	39·2	38·0	1·2	1·2
4.	38·0	39·2	38·0	1·2	1·2
5.	38·0	39·2	38·0	1·2	1·2
6.	38·0	39·2	38·1	1·2	1·1
7.	38·1	39·3	38·1	1·2	1·2
8.	38·2	39·4	38·3	1·2	1·1
9.	38·3	39·5	38·4	1·2	1·1
10.	38·4	39·55	38·6	1·15	0·95
11.	38·6	39·6	38·6	1·0	1·0
12.	38·6	40·0	38·85	1·4	1·15
13.	38·85	40·1	39·0	1·25	1·1
14.	39·0	40·2	39·1	1·2	1·1
15.	39·1	40·2	39·2	1·1	1·0
16.	39·2	40·3	39·2	1·1	1·1
17.	39·2	40·4	39·3	1·2	1·1

In exp. No. 1 we passed the current for the first time round the unmagnetized rod, and observed an elongation of 1·6 of a division of the telescope-scale; immediately after the observation we broke the circuit, which had remained closed about five seconds, and observed a retraction of 1·2 division; the rod now remained at a constant temperature for three hours, and the scale-reading remained steady at 38·0—thus showing that the rod had received a permanent elongation of ·4 of a division on receiving its charge of residual magnetism.

On repeating the experiment we find an elongation and retraction of 1·2 division, which is the quantity the rod retracted on the first break-circuit. Experiments 2 to 5 inclusive give the same result; but on the 6th and subsequent break-circuits we observe a retraction less than 1·2; and this effect we attribute to the heat produced in the rod at the instant of its demagnetization. It is also noteworthy that, from the moment of breaking the circuit in an experiment until the forming of it in the succeeding one, the scale remained immovable. Taking 1·2 division as the amount of elongation and retraction due alone to magnetization and demagnetization, we can determine the mean amount of elongation at the moment of demagnetization as follows. The mean elongation in experiments 6 to 17 is 1·18 division. This is only ·02 of a division less than 1·2, and can candidly be attributed to the errors of observation; but the mean retraction of the same experiments is 1·08 division, which is ·12 of a division less than 1·2, and gives us the measure of the effect due to the heating of the rod at the moment of its demagnetization; for on keeping the rod at the temperature it had during experiments 1 to 5, we found that it gradually retracted until the scale again remained steady at 38·0.

Table II. is here given to show that nearly the same effects of elongation and retraction are observed when the rod is gradually elongating under the effects of heat radiated from the helix, when the latter has a powerful current passed through it.

Table II.

No. of experiment.	Scale, circuit open.	Scale, on closing circuit.	Scale, on breaking circuit.	Elongation	Retraction.
18.	51·4	52·8	51·8	1·4	1·0
19.	51·8	53·2	52·2	1·4	1·0
20.	52·2	53·4	52·4	1·2	1·0
21.	52·5	53·8	52·7	1·3	1·1
22.	52·8	54·0	52·9	1·2	1·1
23.	53·0	54·3	53·2	1·3	1·1
24.	53·2	54·5	53·5	1·3	1·0
25.	53·5	54·7	53·6	1·2	1·1
26.	53·8	55·2	54·2	1·4	1·0
27.	54·2	55·4	54·4	1·2	1·0
28.	54·4	55·6	54·5	1·2	1·1
29.	54·5	55·7	54·7	1·2	1·0
30.	54·8	55·8	54·75	1·0	1·05
31.	54·8	56·0	55·0	1·2	1·0
32.	55·0	56·2	55·2	1·2	1·0
33.	55·2	56·3	55·25	1·1	1·05
34.	55·25	56·4	55·4	1·15	1·0
35.	55·4	56·45	55·5	1·05	0·95

The experiments in the above Table were made on the same rod used in the experiments in Table I.; but before this new series was commenced I passed round the helix a stronger current than previously used, so that the rod was elongated by the heated helix from 39·2 divisions of the scale to 51·4 divisions; and while the scale was advancing to this reading, I determined its rate of progress and found it to be 3·6 divisions in ten minutes. Therefore these experiments were made on the rod while it had a slow motion of elongation. The mean of the elongations is 1·22 division; the mean of the retractions is 1·04 division, which subtracted from 1·20 gives ·16 of a division for the effect of the heat on one demagnetization. The reduction of Table I. gave ·12 of a division for this effect. The difference in the two results I thus account for:—While the bar was slowly expanding from the heat radiated by the helix, the circuit was made and the elongation was immediately observed; but about five seconds elapsed before the reading could be obtained and the circuit broken; and during these five seconds the rod was expanding, but so slowly that its amount could not be read, but was often visible. That this minute expansion could not be determined was to be expected; for if the rod elongated from heat 3·6 divisions in ten minutes, it elongated only ·03 of a division in five seconds, and ·03 of a division was a quantity too small to be measured on the scale; but it nevertheless existed there, and during the continuance of 18 make-circuits would amount to ·03 × 18 = ·54 of a division—quite an appreciable quantity when we come to calculate the mean with this fraction contained in the sum of the retractions given in the last column of the Table. Therefore, to obtain the effect of the heat developed at the moment of demagnetization, we should *subtract* ·03 from ·16, the heating effect of demagnetization determined without this correction. This gives ·16 − ·03 = ·13 of a division, while from Table I. we deduced ·12 for the value of the same effect. The difference of only ·01 division in the two results is not, however, to be taken without some reserve; for in the calculations I assumed that the rod had the same rate of expansion under a closed circuit as under an intermittent one; and this I did because I have no means of determining the difference, if any exists.

Experiments similar to those just given were made on all the iron rods; and similar results were obtained.

Relations existing between the number of break-circuits, the heating of the rod, and its elongation.

At this stage of the investigation it became of interest to determine the above relation. For that purpose I drilled a hole 6 inches deep in the direction of the axis of rod No. 3, of Norway iron, and inserted into this hole a thermo-electric couple formed of two wires (one of copper, the other of iron). This compound wire was wrapped first with two layers of waxed silk, then with twelve layers of floss-silk; and over these layers I coiled two more layers of waxed floss-silk, leaving, however, the point of junction of the wires exposed. This apparatus was introduced into the rod so that the uncovered point of the wire was about one millimetre from the bottom of the hole; and the space included between the point of the wire and the bottom of the hole was filled in some experiments with fine iron filings, in others with mercury. The terminals of the thermo-electric couple were connected with a delicate galvanometer. With the apparatus thus arranged I successively made 50, 100, 200, 300, and 400 break-circuits, taking care that the closed circuits preceding the break-circuits should all be of the same duration. After each series of break-circuits the elongation produced in the rod and the permanent deflection on the galvanometer-needles were noted; and the observations showed that the elongations and the increments of temperature in the rod were proportional to the number of break-circuits.

On the elongations and retractions observed in the iron rods as the strength of the magnetizing-current is gradually increased and diminished; and on the equality in the elongations produced by a definite current when it is gradually and when it is suddenly brought up to its maximum strength.

The observed sudden elongations taking place in an iron rod at the moment of its magnetization naturally led me to inquire if the quantity of this elongation was in any way due to the suddenness of the magnetizing action, and whether the elongation produced by a certain current which is gradually brought up to its maximum strength would equal that produced by the same current suddenly passed with the same maximum strength. This problem was also connected with a proposed simple and accurate means of measuring the changes in dimensions of bodies subjected to magnetization; and therefore I have examined it with care, in the following manner. I cut the thick copper wire leading from the battery to the helix, and firmly attached one of its loose ends to a support. Between this copper wire and the opposite wall I stretched a fine wire of German silver. The other loose end of the battery wire was bent into a sharp angle, and the vertex of this angle was well amalgamated. Now, by sliding this bent copper wire along the fine wire of German silver towards the other copper wire, I could gradually diminish the resistance; and on its touching the other end of the thick battery wire this interposed resistance vanished and the current gained its maximum strength. On slowly retracing our steps the resistance was gradually increased until the whole length of the fine wire was interposed; and then the resistance was at its maximum and the strength of the current at its minimum. But if we brought the two amalgamated ends of the copper wire into contact either with or without the intervention of a mercury cup, we at once could suddenly send the current with its maximum intensity through the helix.

Mean results of first series of experiments. One cell in circuit. Resistance of fine wire =·6 ohm.

On gradually diminishing the resistance,

Fraction of length of fine interposed wire . .	1.	¾.	½.	¼.	⅛.	0.
Scale-readings .	went from 54·8 to 54·85	54·9	55	55·2	55·6	56·1

On gradually increasing the resistance,

Scale-readings	went from 55·5 to 54·8	55·6	55·8	55·9	55·95	56·1
Tangent-galvanometer .	4¾°	..	..	.	..	29½°

Mean results of second series of experiments. One cell in circuit. Resistance of fine wire =·9 ohm.

On gradually diminishing the resistance,

Scale-readings	54·8	54·8	54·85	55	55·4	56·1

On gradually increasing the resistance,

Scale-readings	went from 55·25 to 54·8	55·4	55·65	55·8	55·9	56·1
Tangent-galvanometer . .	3°	..	..	..	..	29½°

Examining the results in the two series of experiments, we see that when the current was passed with all of the interposed resistance in the circuit, the scale went from 54·8 to 54·85, or moved ·05 of a division in the first series of experiments; but in the second series the current was too feeble to effect a measurable elongation, and it was not until one fourth of the fine wire was out of the circuit that the scale-readings began to increase. In both series of experiments the rapid increase in the rate of elongation is noticeable after three fourths of the fine wire was out of the circuit. After all the interposed resistance had been traversed and was out of the circuit, the elongation in both series of experiments amounted to 1·3 division of the scale. The same amount of elongation always occurred when the ends of the copper wires were brought together, or when the circuit was as suddenly formed by plunging the wires into a cup containing mercury. Therefore it is well established that a current of a definite strength will produce the same amount of elongation, whether that strength is suddenly or gradually attained. Indeed in some of the experiments over three minutes were occupied in gradually decreasing the interposed resistance until it was entirely out of the circuit; yet during this very slow increase of the current-strength the scale slowly and smoothly moved upwards in its readings, and when all the interposed resistance had been passed over the elongation again equalled 1·3 division.

The establishment of the above fact was of considerable importance; for it rendered applicable the following simple and precise method of measuring the change in dimensions of bodies on their magnetization. Two iron, steel, or bismuth bars are placed parallel to each other, in Vs, with their similar ends strongly pressed against a firm support, so that, if the rods change in length on magnetization, their free ends will move. Now imagine a lever so arranged that one end of it carries a plano-convex lens and the other end a micrometer-screw. The convex side of the lens is opposite a plane glass which terminates the end of one of the rods, while the point of the micrometer-screw touches the end of the other rod, against which it is pressed by a spring. A piece of plane glass, inclined, placed in front of the lens sends the light from a sodium flame down to the lens and plane glass behind it; and by means of a microscope we can look through the inclined glass on to the lens, and thus accurately view and measure the Newton's rings which we shall now observe. If round the rods we now pass a voltaic current of gradually increasing intensity, we shall see the rings gradually displaced; and from the amount and direction of this *displacement*, together with the knowledge of the wave-lengths of the rays of sodium-light, we can accurately determine the amount and direction of the motion of the ends of the rods. If, however, the current had passed at once with its full intensity, then would have followed a sudden displacement of the rings, but the amount and direction of this displacement it would have been impossible to determine. By making the arm of the lever which carries the convex lens longer than the arm which carries the screw, we can increase the delicacy of the apparatus; for it is understood that, as the rods move in the same direction, the rod carrying the plane glass moves towards the lens, while at the same time the other rod, through the intervention of the lever, pushes the lens towards the plane glass.

The examination of the experiments of the first and second series, contained under the heading "on gradually increasing the resistance," makes known a remarkable phenomenon. In these experiments the current with its maximum strength was first passed through the helix, and then it was gradually brought down to its minimum strength by sliding the copper battery-wire over the fine wire of German silver until the whole length of the latter was brought into the circuit. At the moment of sending the current with its maximum strength, the rod elongated 1·3 division of the scale; but if we now keep the circuit closed but gradually diminish the strength of the current, we observe that the scale-readings do not correspond to those given when the corresponding strengths of current were reached by going from their minimum to their maximum, as the following Tables, giving the differences of scale-readings in the two cases, clearly show.

First Series of Experiments.

Fraction of length of fine interpolar wire. . .	1.	¾.	½.	¼.	⅛.	0.
On gradually diminishing the current. .	55·5	55·6	55·8	55·9	55·95	56·1
On gradually increasing the current . .	54·85	54·9	55·0	55·2	55·6	56·1
Differences . . .	·65	·7	·8	·7	·35	0

Second Series of Experiments.

Fraction of length of fine interpolar wire . .	1.	¾.	½.	¼.	⅛.	0.
On gradually diminishing the current .	55·25	55·4	55·65	55·8	55·9	56·1
On gradually increasing the current . .	54·8	54·8	54·85	55·0	55·4	56·1
Differences . . .	·45	·6	·8	·8	·5	0

We thus see that the rod tends to persist in the elongation it acquired on first passing the maximum current; for it does not retract in proportion to the diminished strength of this current; and the experiments show that even when the current is so far diminished in strength that it would, if suddenly thrown through the helix, be unable to elongate the rod sufficiently to be measurable, yet this feeble current holds the rod elongated ·45 of a division in the second series of experiments; but on breaking the circuit the rod instantly retracts ·45 of a division in the second series of experiments and ·65 of a division in the first series, and regains the length it had before the current was passed round it.

On passing the current with the whole of the fine wire in the circuit, we have in the first series of experiments an elongation of ·05 of a division; but on making the circuit without the interposed fine wire we have an elongation of 1·3 division; and if we now do not break the circuit, but gradually diminish its strength by increasing the interpolar resistance, we find that, when the whole of the fine wire is again in the circuit, the elongation is yet ·65 of a division, whereas when the circuit is at once formed with this same interposed resistance the rod was elongated only ·05 of a division.

The discovery of this most remarkable phenomenon was contained in the above experiments; but to be sure that my experiments should not mislead me I repeated them several times, using every precaution to ensure their accuracy, and obtained results almost identical with those formerly observed. I am therefore confident that I have discovered a phenomenon worthy of minute study; and I purpose to make it the subject of a special investigation.

Unfortunately, during the above experiments I did not make a parallel series of determinations of the magnetic intensities of the rod during the successive stages of passing a current of increasing and of decreasing strength. Yet I can hardly believe that the magnetic intensity will be kept up with the persistent elongation of the rod when it is slowly demagnetized; I think it will be found that the magnetic intensity of the rod depends alone on the strength of the current traversing the helix. The phenomenon indeed shows that, the molecules of the rod on its elongation by magnetization having been forced into new positions, the molecules, either by what might be well called *a "magnetic set"*[*] or from molecular friction, retain these new positions with such persistence that it requires the sudden shock of the induced current produced on breaking the circuit, to cause them to rush to their positions of stable equilibrium.

Effects observed on making and breaking separate currents in the two component helices of the compound helix.

In these experiments two batteries were used. In the outer helix I made and broke a current from sixteen cells, arranged four coupled and four in series. In connexion with the inner helix I used a battery of 25 cells, connected five in a row and five in series. The experiments are interesting, as showing the effects of the induced currents formed on making and breaking the circuits in the various manners given in the following:—

(1)	Made	circuit in	inner	helix; rod	elongated	1·4	division.
	„	„	outer	„	„	·25	„
	Broke	„	„	„	retracted	·25	„
	„	„	inner	„	„	1·4	„

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[*] The term "magnetic set," as applied above, is, by analogy, an appropriate name for the phenomenon; but it cannot well be so applied, because Dr. Joule has already appropriated "magnetic set" as designating the residual magnetism an iron rod retains after its electromagnetization.

(2) Made circuit in outer helix; rod elongated 1·5 division.
„ „ inner „ rod suddenly retracted ·4 and then suddenly elongated ·4.
Broke „ „ „ rod suddenly retracted ·4 and then suddenly elongated ·4.
„ „ outer „ rod retracted 1·5 division.

(3) Made circuit in inner helix; rod elongated 1·4 division.
„ „ outer „ „ „ ·25 „
Broke „ inner „ rod suddenly retracted ·35, then suddenly elongated ·35.
„ „ outer rod retracted 1·65.

(4) Made circuit in outer helix; rod elongated 1·5 division.
„ „ inner „ rod suddenly retracted ·5, then suddenly elongated ·5.
Broke „ outer „ rod retracted ·1 division.
„ „ inner „ „ 1·4 „

On the times occupied in the elongations and retractions of a rod when the two component helices are joined as one helix and placed in the circuit of one battery.

The determinations I here give were made with the eye and a chronograph, and although not as accurate as the interest of the research demands, yet are near enough to the truth to show that the subject is worthy of a careful investigation. The experiments given under the above heading and the succeeding one give an insight into the *velocities* of the molecular motions; and therefore these determinations, taken in connexion with the measures of the corresponding elongations and retractions, will be of considerable theoretic interest when they have been determined with the precision which the following proposed apparatus will in all probability afford.

I thus propose to attack this problem. The mirror of the apparatus will be made of the minimum weight consistent with stability. The mirror will reflect a pencil of light from an electric lamp to a revolving glass disk coated with sensitized collodion. This converging pencil will form a dot of light on the disk, and when the latter is stationary will, on the elongation of the rod, describe a portion of one of its radii, which will appear on developing the sensitized plate. If, however, the disk have a uniform and known rate of rotation, the dot will, on the elongation of the rod, describe a curved line, which, referred to the appropriate ordinates, will give not only the time of the motion of elongation, but also the mode or law of this motion. Of course the motion of retraction can be studied in like manner.

The following experiments were made on rod No. 3, of English refined iron; and each result is the mean of fifty experiments.

	Time of elongation.	Time of retraction.
(1) 25 cells. . .	$\frac{1}{20}$ of a second.	$\frac{3}{10}$ of a second.
(2) 1 cell . . .	$\frac{3}{10}$ "	$\frac{1}{10}$ "

It is thus seen that with twenty-five cells the duration of retraction is six times as long as the duration of the elongation; but with a current from one cell the phenomena are *reversed*, and the duration of the elongation is three times that of the retraction.

Determinations of the times occupied in the elongation and the retraction of a rod when the inner or the outer helix forms in itself a closed circuit, while the current is passed in the respective cases in the outer and in the inner helix.

(1) Terminals of inner helix *not* joined. Current passed through the outer helix from twenty-five cells. Elongation of the rod 1·5 division. Time of elongation $\frac{1}{120}$ of a second. Time of retraction $\frac{1}{4}$ of a second.

(2) Same results as above when the outer helix was open and the current was passed through the inner helix.

(3) The terminals of inner helix united, so that this helix formed a closed circuit in itself. Current from twenty-five cells passed through outer helix. Elongation 1·5 division.

Time of elongation $\frac{2}{10}$ of a second. *Time of retraction* $1\frac{1}{10}$ of a second.

(4) Same results as above when the terminals of the outer helix were united and the current passed through the inner helix.

(5) One cell used. When the terminals of outer or inner helix were *not* united, and the current passed respectively through inner or outer helix, the elongation was 1·1 division, the time of elongation $\frac{3}{10}$ of a second, the time of retraction $\frac{2}{10}$ of a second.

(6) One cell used. The terminals of inner helix united. The elongation was 1·1 division. Time of elongation $\frac{6}{10}$ of a second. *Time of retraction* $1\frac{3}{10}$ of a second.

(7) Same results as experiment (6) when the terminals of outer helix were joined and the current from one cell passed through the inner helix.

To observe a rod slowly retracting during 1·3 of a second was a most remarkable sight, and suggests many thoughts as to the interaction of the induced currents passing in the helices and rod. I may here venture to suggest that the study of these extraordinary phenomena (which I believe I have here first made known) will eventually be of some service in the investigation of induced currents. For the present I am content with merely presenting *the facts*; for I have not yet been able to command the time which their investigation will require.

In experiments (3) and (6) the times of retraction were respectively $1\frac{1}{10}$ of a second and $1\frac{3}{10}$ of a second; and the slowness of these motions allowed me to obtain an insight into their *character*. In each of these experiments the rod retracted with a gradually diminishing velocity, and the motion reminded one forcibly of that pertaining to a body projected vertically upwards.

The coefficients of elongation and of retraction of seven rods of different species of iron, and of three steel rods of various degrees of hardness.

It remains to give the determinations I have made of the coefficients of elongation and of retraction. These measures were made on rods of circular section, 60·1 inches long and ·5 inch in diameter. As previously stated, the iron rods were thoroughly annealed, and the steel rods were carefully tempered. On the ends of the rods numbers were stamped; and these marks corresponded to the rods as follows:—

1 . . .	Scrap iron.
2 . . .	Ulster iron.
3 . . .	Norway iron.
4 . . .	English refined iron.
5 . . .	Low-Moor iron.
6 . . .	Fall-River iron.
000 . . .	Steel, soft.
00 . . .	„ hardened and drawn to blue.
0 . . .	„ „ „ yellow.

The method of determining these coefficients was as follows:—When the rod had attained a fixed temperature, so that the scale-reading remained constant for an hour, I recorded this scale-reading. I then passed the current from the 25-cell battery; and as soon as the new scale-reading thus produced was read, I broke the circuit and obtained the corresponding scale-reading. These readings were now written in the note-book; and immediately after recording them I again made and broke the circuit, and noted the two corresponding readings of the telescope-scale. I then continued making and breaking the circuit and recording the scale-divisions until the rod began to elongate from the heat produced on demagnetization.

The Tables following (see p. 200) consist of six columns, A, B, C, D, E, and F. Under A are designated the rods. B contains the elongations or retractions produced on first passing the current; C the retractions or elongations observed after the first-made circuit had been broken; D the permanent elongations or retractions observed in the rod after the first current passed had been broken; E the elongations or retractions produced on making the second and subsequent circuits; F the elongations or retractions produced on breaking the second and subsequently formed circuits.

After the quantities given in the columns I have written *e* to designate the *elongation* of the rod, and *r* to indicate its *retraction*.

I have given the measures in three Tables. Table I. contains the elongations and retractions in the actual scale units. It is here to be remembered that one division of the scale equals 0·00011 of an inch for the experiments on rods Nos. 1 to 6 inclusive; while for the remaining rods, 000, 00, and 0, one division of the scale equals 0·000146 of an inch. Table II. gives the elongations and retractions of Table I. expressed in fractions of the inch of "Troughton's scale"*. Table III. contains the coefficients calculated from the numbers given in Table II.

Certain numbers in the Tables are followed by * or by †; * indicates the maximum effect observed in the iron or in the steel rods corresponding to the phase of experiment given in the heading of the column in Table I., or as subsequently designated by A, B, C, &c.

An examination of the Tables shows that the maxima and minima effects in the case of the iron rods are very irregularly distributed. Thus, corresponding to the "first make-circuit," we find that rod No. 4 gives the maximum, while rod No. 1 the minimum. On the "first break-circuit" rod No. 2 is the maximum, while rod No. 5 is the minimum. For the "permanent elongation" rod No. 4 is the maximum, and rods Nos. 1 and 2 are the minima. In the two columns corresponding to "second make-circuit" and "second break-circuit," we see that rod No. 8 gives the maximum effect observed, while rod No. 1 gives the minimum.

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* "Two copies of the new British standard, viz. a bronze standard, No. 11, and a malleable-iron standard, No. 57, have been presented by the British Government to the United States. A series of careful comparisons (made in 1856 by Mr. Saxton, under the direction of Dr. Bache) of the British bronze standard No. 11, with the Troughton scale of 82 inches, showed that *the British bronze standard yard is shorter than the American yard by* 0·00087 *inch*. So that, in very exact measures with the yard-unit, it is necessary to state whether the standard is of England or of the United States, as 10,000 American feet = 10,000·5803 English feet."—Lecture-Notes on Physics, by A. M. Mayer, p. 12 (Van Nostrand, New York, 1868).

The phenomena of elongation and retraction observed in rods of steel.

The phenomena observed on the magnetization and demagnetization of the rods of steel have not been referred to. Here we have presented to us remarkable results. On first passing the current round rod 000, of soft steel, it elongated ·8 of a scale-division, behaving like a rod of soft iron; *but* on breaking the circuit, to my astonishment it again *elongated* ·6 of a division, thus leaving this rod with a *permanent* elongation of 1·4 of a division; and this elongation exceeds the permanent elongation given to any of the soft-iron rods when similarly experimented on. On passing the current round the rod *for the second time* the soft-steel rod again did not act like a rod of iron; for it *retracted* ·25 of a division, instead of *elongating* as did the rods of iron in like circumstances; and on breaking this circuit the rod *elongated* ·25 of a division instead of *retracting*, again exhibiting a phenomenon the reverse of those observed in the rods of iron. And it is here important to remark that *all* the steel rods behaved in the same manner on the making and breaking of the second and subsequently formed circuits.

The results just described differ from those obtained by Dr. Joule. Referring to his memoir (Phil. Mag. vol. xxx. p. 85), we find that experiments on a rod of soft steel, 1 yard long and ¼ of an inch in diameter, showed that the rod elongated on first passing the current; but on breaking this circuit the rod *retracted*, while in my experiments the rod again *elongated* on breaking this circuit. Indeed the experiments of Dr. Joule indicate that a rod of soft steel behaves like one of iron, except that the elongations and retractions are of less extent than in the case of an iron rod. It is important, however, to observe that Dr. Joule did not, in his first experiment on this rod, pass round it a current sufficient to "saturate" it, but gradually increased the intensity of the current in successive experiments; and it is to be remarked that, as the intensity of the current increased, the retractions and elongations came nearer and nearer to equality; but in no instance did he observe a *retraction* on passing a current and an *elongation* on its cessation.

In his subsequent experiments Dr. Joule worked on a steel rod of the same dimensions as that used in his former experiment; but it was "hardened to a certain extent throughout its whole length, but not to such a degree as entirely to resist the action of the file." On *first* passing the current, and also on subsequently passing the current with successively increased intensities, he obtained results similar to those I observed in the rod of soft steel; but with this rod also he never observed a less, according to the nature of the body; the other portion is free, and its density equal in all bodies. Of course, however, this does not prevent the æther from experiencing resistance in its passage from the one place to the other. This conclusion was partially verified by Fizeau's well-known investigations on the passage of light through a fluid in motion; for he was led by his investigation to this result—that one portion of the æther adheres to the molecules of the fluid, while the other portion must be considered free and independent of that motion*.

According to our view, the galvanic current consists of nothing but the transport of the free æther in the direction of the length of the conductor; and, in a previous memoir†, we have endeavoured to adduce proofs for the correctness of this view. The quantity of æther which the circuit contained when the æther was still at rest, is neither augmented nor diminished by the formation of a current; it is merely put into translatory motion by the electromotive forces. In ordinary galvanic currents these forces expend heat in producing this motion; so that cooling must ensue at the place where they are in action—perhaps in the same way as a gas compressed within a vessel is cooled when it gets an opportunity of issuing through an opening, in which operation heat-vibrations are expended in order to effect a translatory motion of the gas-particles. The electromotive forces act directly upon the adjacent strata of æther only, and set them in motion; and, through the pressure hence arising, these occasion the motion of the rest of the mass of æther. As is well known, the æther has very great elasticity. It may therefore be assumed that the pressure producing this motion cannot very much alter the density of the moving æther.

According to the theory we present of electrical phenomena, the distribution of the electroscopic tensions upon the surface of the conductor which unites the poles of the electromotor is an immediate effect of the current itself. In the hitherto received electrical theory, on the contrary, the electroscopic distribution has been taken for a basis, from which it has been attempted to deduce both the dependence of the current-intensity on the electromotive force and on the resistance (Ohm's law), and also the law of the development of heat by the current. But since, according to the æther theory, the electroscopic distribution is an effect of the current, and a phenomenon has not to be deduced from its effects, but from its causes, we have held it expedient to theoretically establish these two laws independently of the electroscopic distribution.

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* *Comptes Rendus*, vol. xxxiii. p. 349. Pogg. *Ann.* Ergb. iii. p. 457.

† *Archives des Sciences Phys. et Nat. de Genève*, 1872. Pogg. *Ann.* Ergb. vi. Hefte 1 & 2.

stand by the expression "galvanic resistance."

We picture to ourselves a tube, the cross section of one half of which is I, and that of the other half nI, filled with a fluid which is in translatory motion in consequence of forces acting upon it at one end of the tube. If, now, at one place in the tube we wish to lessen the motion or its velocity by a counterpressure (*e. g.* a piston or the like), we must apply n times as much pressure in the wider half as in the narrower, in order to produce the same effect. The diminution of the velocity or the strength of the current does not depend on the absolute quantity of this counterpressure, but on its quantity on the unit of surface of the cross section. If the pressure on the unit of surface is equally great in the wider and in the narrower tube, the lessening of the strength of the current is the same in both cases. This will be the relation, whatever may be the nature and constitution of the resistance; only the particles of the fluid must be sufficiently mobile to propagate the pressure in all directions. What has just been said is directly applicable to a galvanic current. Whatever view one may entertain on the nature of electricity, all are perhaps agreed in this—that it is a fluid the particles of which are readily movable, and that it must therefore possess the property of communicating pressure in all directions. Galvanic resistance obstructs electric motion. It thus produces a counterpressure; and this is equally distributed over all points of the cross section of the conductor. When, for example, two wires of different metals and of unequal thickness produce an equal diminution of a given current-intensity, these resistances are said to be equal; and we have then, in accordance with the foregoing, to assume that, on the unit of surface of the cross section, the counterpressure which each of them opposes to the propagation of the current is also equal. Consequently it is only the counterpressure on the unit of the cross section that can come into question in the determination of resistances. This is a consequence of hydrodynamical laws, and cannot be conceived in any other way, inasmuch as electricity is a fluid.

That galvanic resistance depends on the physical and chemical constitution of the conductor is readily understood; but the possibility can also be foreseen that it may be dependent on other conditions also. The resistance might be regarded as arising from the friction experienced by the æther molecules in pressing through between the material molecules of the conductor. We have already remarked that the density of the free æther in all bodies is equal. Therefore in equal volumes there are equal quantities of free æther. If, then, a current comes from a wire with the cross section I, and passes into another resistance is proportional to the intensity: as we perceive from what has been before adduced, in that case the division must take place in the way experiment teaches that it does.

But if galvanic resistance actually is, as we have endeavoured to demonstrate, proportional to the intensity, it may possibly be asked how this circumstance could so long have escaped observation in the determination of the resistances of conductors. The reason for it can readily be discerned. When the resistance of a conductor is to be investigated, either the conductor is inserted in the undivided circuit of a galvanic series, and the diminution of intensity thereby produced is compared with the diminution of the same current-intensity occasioned by another conductor of known resistance, or else a division of the current is made use of by employing a differential galvanometer or a Wheatstone's bridge. In the former case Ohm's formula is used for the calculation, and in the latter the formulæ which give the division of the current among several conductors. But in all these formulæ no other resistances occur than those belonging to the unit of intensity. Therefore, in the methods employed, only resistances with equal intensity, namely intensity 1, have to be compared one with another; and from such a comparison it is impossible to draw the conclusion that the resistance increases with the intensity. Consequently the question of the dependence of the resistance on the intensity can only be solved in a theoretical way.

XXII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* By James D. Dana.—Part II. *The Condition of the Earth's Interior, and the connexion of the facts with Mountain-making.*—Part III. *Metamorphism*[*].

[Continued from p. 140.]

II. *The Condition of the Earth's Interior.*

THE condition of the earth's interior is not among the geological results of contraction from cooling; but these results offer an argument of great weight respecting the earth's interior condition, and make it desirable that the subject should be discussed in this connexion. Moreover the facts throw additional light on the preceding topic—the origin of mountains.

It seems now to be demonstrated by astronomical and physical arguments (arguments that are independent, it should be noted, of direct geological observation), that the interior of our globe is essentially solid. But the great oscillations of the earth's surface, which have seemed to demand for explanation a liquid interior, still remain facts, and present apparently a greater difficulty than ever to the geologist. Professor LeConte's views (Am. Journ. Sc. vol. iv.) were offered by him as a method of meeting this difficulty; yet (as he admits in his concluding remarks) the oscillations over the interior of a continent, and the fact of the greater movements on the borders of the larger ocean, were left by him unexplained. Yet these oscillations are not more real than the changes of level or greater oscillations which occurred along the sea-border, where mountains were the final result; and this being a demonstrated truth, no less than the general solidity of the earth's interior, the question comes up, how are the two truths compatible?

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[*] For Part I., on the Orig'n of Mountains, see p. 41.

The geological argument on the subject (the only one within our present purpose) has often been presented. But it derives new force and gives clearer revelations when the facts are viewed in the light of the principles that have been explained in the preceding part of this memoir.

The Appalachian subsidence in the Alleghany region of 35,000 to 40,000 feet, going on through all the Palæozoic era, was due, as has been shown, to an actual sinking of the earth's crust through lateral pressure, and not to local contraction in the strata themselves or the terrains underneath. But such a subsidence is not possible, unless seven miles (that is, seven miles in maximum depth, and over a hundred in total breadth) of *something* were removed in its progress from the region beneath. If this *something* was only vapour or gas, then seven miles of open space must have existed there; and this could not have been, except through seven miles of local contraction along the region; but such an open space, if of possible formation, would have been obliterated by catastrophic subsidence, instead of the slow movement that actually took place. And moreover, such open spaces, of no less extent, must have existed, in one or more ranges, underneath all continental borders. This is proved, and at the same time the extreme improbability of their existence demonstrated, by the facts reviewed beyond.

If the matter beneath was not aërial, then liquid or viscous rock was pushed aside. This being a fact, it would follow that there existed, underneath a crust of unascertained thickness, a sea or lake of mobile (viscous or plastic) rock as large as the sinking region, and also that this great viscous sea continued in existence through the whole period of subsidence, or, in the case of the Alleghany region, through all Palæozoic time—an era estimated on a previous page to cover at least thirty-five millions of years, if time since the Silurian age began embraced fifty millions of years.

The under-Appalachian fire-sea, if a reality, must hence have had a long continuance.

But on the above condition it could not have begun its existence later than the period of disturbance closing pre-Silurian time. Earlier great subsidences were involved in the deposition of the material of which the Blue Ridge, Highland Ridge, Adirondacks, and the Archæan heights further north were made; and the undercrust sea would have been through all a necessity. In fact it is difficult to find a reason for doubting its having dated back to the era of general fluidity.

Directly following the Palæozoic, or as its closing event, as explained on a preceding page, occurred the plicating of the Alleghany rocks to their depths miles below, and the crystallization of part of them; and this epoch ended in the making of the mountains (a synclinorium) and the annexation of the central and western part of the region to the essentially stable area of the continent; and if motion in the rocks was ever transformed into heat, the under-Appalachian sea should have had its temperature, or its extent, or both, increased. Then, after the Appalachian region had thus become essentially stable, the locus of the region of yielding was moved to the eastward. The long range of the Triassico-Jurassic beds, on the Atlantic border from Nova Scotia to southern North Carolina, show the positions of the new troughs, as stated at page 133. These subsidences, amounting to 4000 feet in some parts, ended in a tilting of the beds and in fissure eruptions through all these sandstone regions from the most northern to the most southern. Now the question arises whether the great under-Appalachian fire-sea of the Palæozoic continued on through the Triassic and Jurassic periods of the Mesozoic, and thus favoured the subsidences and eruptions that then took place—or whether the old sea of viscous rock, after being increased in extent or temperature by the profound plicating and faulting of the Appalachian revolution, then ceased to exist (in some way difficult to understand), and others were made further east by the later movements. Such a ceasing with a subsequent renewal is seemingly improbable; and if it did not occur, then the under-Appalachian fire-sea continued from the Palæozoic far into the Mesozoic era.

When the material of the under-Appalachian sea was pushed aside by the subsiding Palæozoic deposits of the Appalachian region, what became of it? Some of it may have moved off southward. The chief part would pass either to the *west* or to the *east*. That it did not go *west* is evident from the ascertained fact that the oscillations in that direction during Palæozoic time were small; for the region was then the larger part of the time a mediterranean salt-water basin or sea, nurturing crinoids, corals, and mollusks, and making limestones. If not westward, then it passed *eastward*; and if driven eastward, a geanticlinal elevation of a sea-border region parallel with the area of subsidence must have been in progress from lateral pressure. The height of this geanticlinal, or swell of the overlying crust (anticlinorium), would depend on the distance to which escape eastward was possible—that is, on the eastward extent of the subterranean region of mobile rock. Its elevation was probably small and of varying extent during the Silurian and Devonian; for Devonian fossils show that the sea-border south of New York had some way an open connexion with the Atlantic Ocean; but there is no evidence in the Appalachian rocks of the Carboniferous era to prove that off New Jersey it was not at that time almost or quite a complete barrier: the marine fossils in the more eastern of the Pennsylvania Coal-measures are rare; and those in the western Pennsylvania beds would be from the waters of the Mediterranean Sea over the Mississippi basin, which reached northward from Alabama and, east of the Cincinnati uplift, bathed all the western part of the Appalachian region, and probably its whole breadth.

When, at the beginning of the making of the Alleghanies, the strata commenced to yield before the pressure and to become pushed up into great folds, the geanticlinal of the sea-border would subside in part in consequence of the removal of resistance in front of it; and this tendency to subside by gravity may have been part of the means by which the plication was made to go forward, its action adding to that of the pressure. But the subsidence did not continue to the obliteration of the geanticlinal; for it was still above the ocean's level during the following era—the Triassico-Jurassic. The absence of all remains of distinctively marine fossils from these rocks, and from any rocks of the Triassic and Jurassic eras in view over the Atlantic border, demonstrate (as I have long held) that an emerged area then existed outside of the present coast-line. Moreover, inasmuch as these Triassico-Jurassic areas (situated on the Atlantic slope parallel with the Appalachians) were subsiding while their rocks were in progress, the sea-border anticlinorium should at the time have taken another turn upward as a counterpart to this subsidence.

With the close of the Triassico-Jurassic era, if not before, the great *anticlinorian* barrier began actually to disappear; and by the time the Cretaceous period opened it had so far sunk that the Atlantic coast-region south of New York was again exposed to the ocean and flourished with abundant marine life, the Cretaceous fossils of the coast giving full evidence on this point. Thus the absence from the present Atlantic border of all Triassic and Jurassic marine fossils and the presence of Cretaceous species in great numbers are well accounted for.

Professor Hunt has recognized the existence, on the Atlantic border of the continent and outside of it, during the Palæozoic era and earlier, of an emerged region, and has appealed to various bare Archæan areas in New England and to the northeast, and to the Archæan character of the Blue Ridge, &c., as proof. He has designated the region, badly, as an "eastern continent," and finds in it, with reason, a source for much of the sedimentary material that was used in making the Appalachian and other rocks. Professor LeConte also brings into his views such an elevation, and remarks upon its final disappearance. But neither of these authors states that he regards it as part of a system of oscillations set in motion by the lateral pressure resulting from the earth's contraction, and a direct counterpart to the geosynclinal of the Appalachian region. Their views are adverse to such an idea, the subsidence with them being not due to contraction.

The facts thus sustain the statement that lateral pressure produced not only the subsidence of the Appalachian region through the Palæozoic, but also contemporaneously, and as its essential prerequisite, the rising of a sea-border elevation, or geanticlinal, parallel with it, and that both movements demanded the existence beneath of a great sea of mobile rock.

The movement and mountain-making over other parts of the Atlantic border (p. 132), and also the grand double series of events on the Pacific or Rocky-Mountain border (p. 133), sustain and illustrate the same views. The undercrust fire-sea on the Pacific border must have had great length from north-west to south-east—and also great breadth, for the border region is at least 1000 miles wide; and great breadth and great length seem plainly its characteristics even till Tertiary times. And did it continue on through the Tertiary and afford the floods of rock that were poured out from the deep fissures of this long era? And was it still in existence when the great floods were poured forth over the drift-gravel beds?

It is further to be noted that, in the course of past time, the whole continent has had its surface from one side to the other criss-crossed with oscillations and lines of disturbance, from the lateral pressure acting against its opposite sides, whence it is clear that the continental subterranean seas were *once* continuous. An appeal to the other continents for further testimony is hardly necessary.

The facts from the ocean seem to demand a vastly greater range for the undercrust mobile layer. The coral-island subsidence during the Quaternary and part or all of the Tertiary could hardly have been due simply to radial contraction from cooling; for this would make the cooling over the tropical part of the ocean in this small part of geological time sufficient to produce there a sixth of the oceanic depression. Is it not proof that even then the plastic layer had enough of extent beneath the tropical part of the oceanic crust to permit of such a sinking under the irresistible lateral pressure at work? However this may be, it stands as a fact to be explained.

In view of the conclusions here reached with regard to the earth's interior, I present the following statements:—

1. That this restriction of the interior liquidity of the earth to an undercrust layer does not require in itself any modification of the views I presented more than twenty-five years since on the results of the earth's contraction, since there is still a flexible crust and mobile rock beneath it.

2. The condition of the earth's interior here recognized is, as many readers will have observed, that suggested long ago by Professor W. Hopkins, the author who first offered a mathematical argument in favour of the earth's either having a very thick crust or being solid throughout*. In a paper "On Theories of Elevation and Earthquakes" in 1847†, Professor Hopkins argues that the central mass of the earth became solid in consequence of the pressure whenever the temperature within reached a limit that permitted of it, that crusting at the surface from cooling commenced afterwards, and that between the regions of interior and exterior solidification there long remained a viscous layer, which in the progress of time was gradually contracted by the union of the solid nucleus to the thickening shell.

3. The possibility of solidification at centre from pressure in the face of a temperature too high for consolidation from cooling has not been experimentally demonstrated; yet a number of facts favour the principle. It has been urged that since the solidification of rocks is attended by contraction (that is, by increase of density), and since pressure tends to produce this

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* Trans. Roy. Soc. 1839, 1840, 1842.

† Report Brit. Assoc. for 1847, p. 33. The theory of elevation advocated in this paper attributes elevation, not to lateral pressure from contraction, but to evolved vapours underneath the elevated region. The array of facts which have been presented respecting the positions of mountain-ranges, their relations to the great areas of depression, their successive formation on sea-borders in parallel ranges, and the natural evolution of the whole from the universal action of the one great cause (contraction), has appeared to me to afford the most complete demonstration that the vapour theory is not necessary, at least as regards mountain-ranges. The fact also that mountains so raised could not hold themselves up has seemed to be an insuperable difficulty to the success of the method.

greater density, therefore pressure may bring about the condition of the solid. The fact that ice, which has less density than water, changes to water under pressure, has been appealed to in support of the conclusion. The pressure to which the material within the earth is subjected is so great that experiment can never imitate it or directly test its effects. Beneath only one hundred and fifty miles of liquid rock it would be not less than one million of pounds to the square inch. Less than this may have been sufficient to produce crystallization, and so give rigidity to the viscous rock-material, or, at least, after the cooling the earth has undergone. The rigidity of slowly solidified rock is beyond that of glass or steel, or the degree which, according to Sir William Thomson, must exist in order that the earth should be as completely free as it is from tidal movements in its mass.

4. According to the above, the solid part of the globe consists, as regards origin, of three parts:—

(*a*) The central mass, consolidated by pressure; the solidification *centrifugal*, or from the centre outward.

(*b*) The crust proper consolidated by cooling; the solidification *centripetal*, or from the surface inwards.

(*c*) The outer crust or superficial coatings (the supercrust), made chiefly by the working over and elaborating of the material of the surface through external agencies, aided by the ever-acting lateral force from contraction, and including all terrains from the Archæan upward.

5. As to the thickness of the viscous layer and the overlying crust, or the depth of the later undercrust seas, I have nothing to offer. The Appalachian subsidence might have been accomplished with but seven or eight miles of depth underneath.

The undercrust fire-seas would have their heat from time to time supplemented through the movements of the crust. But the ordinary oscillations of the crust were so extremely slow and so ineffectual in producing heat, and the greater mountain-making movements occurred at so very long intervals (many millions of years), and then were so very limited in area compared with the earth's surface, that this cause could not have prevented a gradual narrowing of their limits with the progressing refrigeration. But even after the general union of the crust and nucleus, giving the earth *trap-like* "rigidity," had taken place, leaving only local fire-seas, the connexion may not have been so complete that it would not sometimes yield enough to the slow working of lateral pressure to permit oscillations of nearly continental extent, like those of the post-Tertiary.

A final word on Mountain-making.—From the above we learn that in the work of mountain-making in eastern North America there was first the commencing and progressing geanticlinal on the sea-border, and, as a concomitant effect of the lateral pressure, a parallel geosynclinal further west along the border of the continent. Concurrently the deepening trough of the geosynclinal was kept filled to the water-level, or nearly, by sedimentary accumulatious until these had become seven miles in thickness; and, as a consequence, the lines of equal temperature (isogeotherms) in the crust beneath gradually rose upward seven miles; and further, the geosynclinal crust, owing to this rising of heat from below, lost part of its thickness by a melting-off of an under portion, and also part of its strength up to a higher level by the softening action of the heat, while it received, as the only compensation for the loss of thickness, the addition of half-consolidated sediments above. Finally, the geosynclinal region, owing to its position against the more stable continental mass beyond it, and to the weakness produced in its crust in the manner explained, became, under the continued lateral pressure and the gravity of the geanticlinal, a scene of catastrophe and mountain-making after the manner described.

The principle here brought in, that the weakening of the crust through the rise of the isogeotherms was one occasion of the catastrophe, is made of prominent importance by Professor LeConte (Am. Journ. Sci. vol. iv. p. 468), though by a somewhat different method.

The geological facts thus far gathered have not yet proved that there was a geanticlinal on the Pacific border (like that of the Atlantic), as a counterpart to the geosynclinals in progress; but the evidence may be looked for with confidence.

III. *Metamorphism.*

The fact that all metamorphic or crystalline rocks are upturned or plicated rocks has led many to believe that disturbance and plications were essential features of an epoch of metamorphism, and that Herschel's theory, which attributes metamorphism to the heat that rises into the strata owing to accumulation above (a rise of the isogeotherms), is insufficient. This conclusion is certainly confirmed by finding no evidence of metamorphism in the lowest beds of the Carboniferous series of Nova Scotia, where, since the series has a thickness of nearly 16,000 feet, according to two of the best geologists in the world, Logan and Dawson, the bottom temperature must have been, when the series was completed, at least 330° F. It is still better sustained by the observation that the lower of the forty thousand feet of strata in the Appalachian region were, where measured by the Professors Rogers, *not metamorphic*, the Chazy and Trenton limestones being ordinary uncrystalline limestones. And yet the temperature in these inferior beds, marked by the ascending isogeotherms, must have been before their disturbance, as calculated by Professor LeConte, not less than 800° F., and much above this if more heat escaped from the earth then than now. Thus seven miles of accumulations were not sufficient to bring about metamorphism or crystallization even in the lowest stratum, or any change beyond that of ordinary solidification*.

It seems certain, therefore, that this method of obtaining the heat, by blanketing the surface with strata, is not sufficient.

Neither, as Mallet has observed (p. 303 of the last volume of Silliman's American Journal), can heat be derived from simple pressure or "mechanical compression," as the language of Vose suggests†. But with *movements* in the strata, or progressive plications, such as the metamorphic rocks themselves show they have experienced, then, according to the principle of the transformation of motion into heat, first suggested with reference to metamorphism by Professor Henry Wurtz of New York in 1866‡, and recently applied to volcanoes and demonstrated by Robert Mallet, Esq., the conditions for metamorphism might be complete even with comparatively little help from a rise in the isogeotherms. This result would certainly follow if the heat from motion is great enough, as Mallet appears to show, to produce fusion. Such a cause is capable, as others have urged, of producing the heat throughout the strata just where it is needed for work. Under it accumulations of strata of like thickness and composition would be differently acted upon according to the three conditions:—(1) the amount of motion, one principal source of heat; (2) the thickness of the series of beds undergoing movement, another source of heat beneath; (3) the amount of moisture present in the beds. Thus widely diverse metamorphic rocks might be made of the same material; and if a region of feebly metamorphic rocks is found to lie side by side with one of thoroughly metamorphic, the strata of the two may have originally been similar and of one and the same geological horizon.

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* The arguments here presented are the same that I urged in 1866 (Silliman's American Journal, vol. xlii. p. 252).

† Vose could hardly have intended to say in place of pressure the *motion* produced by pressure; for in one of his paragraphs he attributes the changes distinctly to "the enormous pressure generated in the folding of masses of rock the depth of which is measured by miles;" and this pressure was that of gravitating sediments alone, while the additional heat required came from a rise in the isogeotherms in consequence of surface accumulations. The truth is, that instead of folding generating pressure, the pressure generated the folding; and the movement attending folding was essential to the existence of the heat requisite for metamorphic changes. Thus the views of Vose and Hunt are set aside by Mallet, instead of being, as Professor Hunt says (the last volume of Silliman's Journal, p. 270), "confirmed" by him. In a letter of May 10th to the writer, Mr. Mallet refers to his opposition to Herschel's theory, and adds that he was "rather amused" at finding himself brought forward by Professor Hunt in support of it. Mr. Vose's views are contained in his work on Orographic Geology, published in Boston in 1866 (136 pp. 8vo).

‡ Silliman's Journal, vol. v. p. 385. Professor Wurtz's opinion was first published in a paper on "Gold Genetic Metamorphism," in the 'American Journal of Mining' (New York), Jan. 25, 1868. The paper was read at the Meeting of the American Association at Buffalo in August 1866.

Metamorphism over large areas is thus one of the direct results of the earth's contraction. Solidification is often only a lower stage in the same process; and the reddening of sandstones, as already explained*, is frequently involved with it.

XXIII. *On the Measure of Work in the Theory of Energy.* By ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

PROFESSOR MAXWELL gives the following definition and measure of work:—"Work is done when resistance is overcome; and the quantity of work done is measured by the product of the resisting force and the distance throughout which that force is overcome" (Theory of Heat, 1871, p. 87).

1. It is to be presumed that when the uniform force F acts throughout the time T in a given direction upon a body which is free to move in that direction, the resistance overcome by the force will be that arising from the *inertia* of the body—in other words, the resistance which the body offers to any change being effected in its state of rest or motion for the time being, and which is always proportional to the force employed in overcoming it. It follows from this, that, under the circumstances referred to, the resistance which is being overcome at each epoch of time, and therefore the work done in equal intervals of time, will be the same throughout the motion. But if the body is at rest to start with, and T is divided into n equal intervals, the work done at the end of the first interval $\frac{T}{n}$, according to the above measure, will be $\frac{1}{2}F^2\frac{T^2}{n^2}$; that done at the end of the second interval will

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* Phil. Mag. for July, pp. 49 & 50.

† Communicated by the Author.

be $\frac{1}{2}F^2\left(\frac{2T}{n}\right)^2$; that at the end of the third interval will be $\frac{1}{2}F^2\left(\frac{3T}{n}\right)^2$, &c.: and therefore the work done in the first interval $\frac{T}{n}$ will be $\frac{1}{2}F^2\frac{T^2}{n^2}$; that *during* the second interval will be $\frac{3}{2}F^2\frac{T^2}{n^2}$; that *during* the third interval will be $\frac{5}{2}F^2\frac{T^2}{n^2}$, &c. It has already been pointed out, however, that the foregoing definition of work implies that the amount of work done in equal intervals of time will be the same. It follows, therefore, that the definition and measure of work above propounded contradict each other in the case we have been considering.

2. Suppose a body whose mass is M to be moving in a certain direction with a velocity V_1, and that the force F is applied to the body in the direction of its motion. Professor Maxwell proves that, if during the small time T the body moves through the space s, and has acquired at the end of T the velocity V, we shall have

$$Fs = \tfrac{1}{2}(MV^2 - MV_1^2),$$

an equation, be it remembered, which holds independently of the magnitude of T, provided F be uniform.

If we put $V = V_1 + v$, we shall have

$$\text{work} = Fs = \tfrac{1}{2}M(v^2 + 2vV_1). \quad . \quad . \quad . \quad . \quad (1)$$

Now v is the pure product of the force F acting on the body M during the time T; whence it appears that, adopting the measure of work above proposed, the work done by the force F on the body M in the time T involves the variable quantity V_1, which is entirely independent alike of F, of M, and of T.

3. The right side of the expression (1) will always be positive so long as V_1 and v have the same sign, *i. e.* so long as the directions of the force and the initial velocity conspire. But if the force and initial velocity have opposite directions, and T and V_1 are both finite, the right-hand side of (1) will first be negative; as the motion proceeds it will become zero; and it will finally become and continue positive. It results, therefore, from the above measure of work, that the work done in a finite time by a finite force acting upon a body of finite magnitude which is free to move, may be zero.

4. The proper work of force is to generate or destroy momentum*; and the work done by the force in a given time will

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* No doubt force has another effect—that, namely, of causing a body to describe, or of preventing its describing, space: but of these two effects, viz. the description of space and the generation of momentum in any in-

be properly measured by the momentum created or destroyed in that time*.

The measure thus proposed, in fact, differs from the received measure enunciated by Professor Maxwell less than might at first sight be supposed; for when T is very small, (1) becomes

$$\text{work}=Fs=Mv\,.\,V_1.$$

Thus, while I contend that the work done in a short interval of time under the above circumstances is properly measured by the momentum generated during the time, according to the views upon the subject which are generally received it is measured by the product of the momentum generated and the initial velocity V_1—a position the reasonableness of which, I apprehend, it will be found difficult to establish.

5. If, instead of expressing the work done in terms of the force acting and the time during which it acts, we wish to express it in terms of the force and of the space described under its influence, we have only to replace T in the expression FT by its equivalent in terms of the other variables. This, where the body moves from rest, would give us $\sqrt{2MFs}$ as the measure of the work done by the force F on the body M while moving through the space *s*.

6 New Square, Lincoln's Inn,
July 29, 1873.

XXIV. *Experiments on the Directive Power of large Steel Magnets, of Bars of magnetized Soft Iron, and of Galvanic Coils, in their Action on external small Magnets. By* GEORGE BIDDELL AIRY, *Astronomer Royal, C.B., P.R.S.—With Appendix, containing an Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass. By* JAMES STUART, *Esq., M.A., Fellow of Trinity College, Cambridge*†.

THE only experiments with which I am acquainted tending to throw light upon the distribution of magnetic power in the different parts of a steel magnet are some very imperfect

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definitely small interval of time, the former will be of a lower order of magnitude than the latter; while, of the small space actually described in the interval, all but an indefinitely small portion will have been described under the influence of the velocity from time to time generated during the interval; the residuum immediately due to the direct action of the force, and in no degeee resulting from acquired velocity, being of the *third* order of small quantities at most.

* From this it follows that the work done by the force F acting during the time T on a body which is free to move will be measured by FT.

† From the Philosophical Transactions for 1872, Part II., having read February 8.

(*α*) Wood-like in structure, of a dull brownish-black colour, cuts readily, and does not soil the fingers; particles do not cake when heated; sp. gravity 1·32 to 1·45; contains 17 to 18 per cent. of hygroscopic water. This lignite makes a very good fuel, but is not as rich in carbon as most of the lignites about Lough Neagh, which contain 51 to 58 per cent. (Sir R. Kane). Compared with other lignites its percentage of ash is high; but though it also contains a large quantity of hygroscopic water, yet in this respect it does not contrast so unfavourably with them.

(*β*) Moderately brittle, of a brilliant black lustre, sp. gravity 1·29 to 1·305, hygroscopic water 7 to 8 per cent., burns rapidly, caking but little, and leaving a light porous coke; a slight admixture of iron pyrites in thin scales; ash light, and of a dull reddish colour. From the large amount of carbon present, and the small amount of ash, this makes a very good burning coal. It is as rich in carbon as many of the English and most of the Scotch coals.

(*γ*) Hard, compact, difficult to pulverize, of a dull black colour, powder brownish black, burns brightly, tumesces little, cakes but slightly, ash greyish white; sp. gravity 1·275 to 1·37, hygroscopic water 1·5 to 1·9 per cent., coke about 50 per cent. There is a considerable admixture of iron pyrites, which occurs in thin layers. This coal yields a large quantity of gas of high illuminating quality; and were it not for the large percentage of ash it contains, it would be equal to any of the English Cannel-coals.

XXVII. *On the Nodal Lines of a Square Plate.*

By Lord RAYLEIGH, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I REGRET to find that the investigation of one of the nodal lines of a square plate, published in your August Number, is to a great extent vitiated by the erroneous assumption that it is possible for a square plate to vibrate after the manner of a simple bar. The function representing such a motion satisfies the general differential equation, as well as the two necessary boundary conditions along the pair of edges for which the motion is constant; but on the other pair of edges one of the conditions is in general violated. This corresponds to the fact that if a plate is bent by couples acting along one pair of opposite edges, it will in general take a contrary curvature in the perpendicular direction.

Several too confident assertions in the paper referred to must now be retracted; and the deviation of Dr. Strehlke's measurements from my calculations are no greater than are fairly attributable to the imperfections of the latter. It appears that the dependent of the relation between the two elastic constants.

I may, however, be permitted to remark that my calculation, though not strictly applicable to a glass or metal plate, belongs to an extreme case of the true solution; for it would be correct if the nature of the material composing the plate were such that the extension produced by a longitudinal force acting along a bar of it were unaccompanied by lateral contraction. This condition of things, though probably not realized in nature, is approximated to in the case of substances such as cork (Thomson and Tait's 'Natural Philosophy,' § 685).

I remain, Gentlemen, your obedient Servant,

RAYLEIGH.

XXVIII. *Reply to some Remarks by* Professor Challis*, "*On Objections recently made to the received principles of Hydrodynamics.*" *By* ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

I AM glad to find that my argument in proof of the equation $p=$ funct. (ρ, v), applicable to fluid motion in one direction, has received the sanction of Professor Challis. When personal friends have concurred with opponents in regarding such a relation between the pressure, velocity, and density as a wild suggestion, unworthy of a moment's consideration, it will be readily understood that I am not insensible to the value of support.

Professor Challis labours under a misapprehension, however, when he asserts that the "general theorem includes the more particular case in which the pressure is a function of the density only," and that, therefore, by my "own showing, it is legitimate to make the hypothesis that the pressure is always in exact proportion to the density."

For motion in one direction it is axiomatic that we must have in *every* fluid, in *every* case of motion,

$$p=f_1(xt), \qquad \rho=f_2(xt), \qquad v=f_3(xt);$$

whence it follows that, for every value of x and t for which each of the foregoing equations represents a substantive relation between the variables, we shall have

$$p=\text{funct.}\,(\rho, v).$$

It is not axiomatic, however, though it happens to be true, that in *every* fluid for *particular cases* of motion we may assume

$$p=f(xt), \qquad \rho=a^2f(xt);$$

where a^2 is a constant whose value or values depend on the nature of the particular fluid dealt with.

And it is neither axiomatic nor true, that in *any particular*

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* See Phil. Mag. for August.

† Communicated by the Author.

§ 31. *Experiment.*—The 8-cell battery which I used was not sufficient to heat twelve inches of the wire when in a single loop even to dull redness. Such a non-luminous hot loop discharged an electroscope of — electricity, when at a distance of one inch, in seven or eight seconds. A + charge is not perceptibly affected.

§ 32. It appears, on comparing the experiments of §§ 1–21 with those of §§ 28–31, that the behaviour of red- and white-hot earth-connected iron balls is precisely analogous to the behaviour of red- and white-hot platinum wires heated by galvanism. In experiments §§ 28–31 the battery was not insulated from the ground. I may mention that repeated and varied attempts by insulating the battery to detect a difference in the discharging-power between an earth-connected and insulated galvanically heated wire (similar to the difference between the discharging-powers of earth-connected and insulated hot iron balls) failed, presumably on account of the considerable amount of neutral matter in the battery, which must be electrically connected with the wire.

§ 33. *Experiment.*—When the black bottom of a pan of boiling water is brought within ¼ inch of the top of an electroscope charged with + or — electricity, ordinary temporary inductive discharge takes place: the leaves recover their divergence completely on the removal of the pan. Yet the actual quantity of heat, as measured by air-expanding power radiated by the bottom of the pan upon the electroscope at the distance of ¼ inch, is far greater than that radiated upon it at a distance of 6 inches by the 4-inch long white-hot platinum wire. This is clearly enough shown by employing the differential air-thermometer with conical copper air-chambers blackened above. The pan has a base about 700 times as great as the orthographic projection of the outline of the spiral, and indefinitely greater than the projection of the wire itself. Immediately the spiral is heated white-hot above one cone, the pan of boiling water (containing an immersed "heater") is brought so near to the other that the liquid in the bent stem remains for a time sensibly at rest. The wire is 6 inches from its cone. The pan has to be placed at a distance of 8 inches to produce equilibrium; and therefore, if the mere quantity of radiant heat received by the electrified body were at all proportional to the amount of discharge, the pan at 8 inches should discharge the electroscope as rapidly and completely as the white-hot spiral at 6 inches. But, as before stated, its permanent discharging-power is practically nothing, even at ¼ inch. Hence it is clear that the discharging-power depends far more upon the *quality* of the heat radiated from the *discharger,* than upon the quantity of heat (as measured in heat-units) received by the electrified body.

§ 34. *Experiment.*—If a series of dielectric non-conductors, such as a series of plates of varnished glass, be placed on an electroscope which is then charged with — electricity, a dull red-hot ball or platinum wire a few inches above the electroscope causes the leaves to collapse, but does not discharge the system. On removing the plates in succession (after the removal of the heated discharger), the leaves diverge more and more, till the withdrawal of the last leaves them nearly fully charged. The bottom of each plate is found to be charged with + electricity. A similar effect is produced, changing the changeables, by a white-hot ball or wire above the upper of a series of varnished glass plates on the top of a positively charged electroscope.

§ 35. The experiments with electroscopes may be repeated almost one for one with the condensed electricity of a Leyden jar. Owing, I presume, to the closeness of the two condensed electricities to one another on the metallic coatings, and to that penetration of the two kinds into the glass which furnishes the potential residual charge, the discharge of a jar is never so rapid or complete under the same circumstances as that of an electroscope.

§ 36. *Experiment.*—A Leyden jar of about 1½ square foot outer foil surface is fully charged with + electricity. A white-hot earth-connected iron ball, at a distance of 3 inches above the electroscope, discharges the jar, but by no means completely, in thirty seconds (when the ball had become dull red). A jar charged with — electricity is discharged under like conditions almost completely in thirty seconds. The remaining charge is not greater than the first residual charge under ordinary circumstances (the glass being about ⅛ inch thick).

§ 37. A dull red-hot ball earth-connected at a distance of 2 inches from a +-electrified jar discharges scarcely any of the charge in a minute, while a — charge in the same time is considerably diminished.

§ 38. *Experiments.*—A platinum spiral of wire 4 inches long and ·01 inch thick is fixed 4 inches above a charged jar. The latter loses little of its charge in one minute. When the current passes through the wire from eight platin-zinc cells the wire becomes white-hot, and the charge, whether + or —, is lost in one minute. For the success of this experiment, especially when + electricity is dealt with, the wire must be almost at the point of fusion.

§ 39. *Experiment.*—A 9-inch single-loop wire, one inch from the jar, heated to incipient redness, discharges about half of the electricity of a +-charged jar and almost the whole of a —-charged one.

§ 40. Two electroscopes, charged respectively with + and —

connected with the earth. Above the knob of the jar is the incandescible platinum wire. As the electricity of the inner coating (+ or —) is discharged, that of the outer coating (— or +) is released, and, traversing the wire of the galvanometer, deflects its needle. The effect may be increased by using a battery of jars as above, or by placing the discharging-wire between the knobs of oppositely electrified jars whose outer coatings are connected with the opposite terminals of the galvanometer. By such means more striking effects can be produced than by the uncondensed electricity of the conductor of the machine.

§ 46. The assertion that glass is a good conductor of electricity when red-hot at once suggests a possibility of connecting such a statement with the above experiments of discharge.

§ 47. *Experiment.*—Nevertheless this statement is true; for if the end of a glass tube be made red-hot, it may indeed be placed upon the top of a + or — charged electroscope (being held by the cool end) without discharging it; but if, when so resting, it be touched by a wire earth-connected, the charge is immediately and completely lost.

§ 48. *Experiment.*—The end of a stout glass tube is heated red-hot; on touching with the hot part either conductor of an electric machine, it is found that both + and — electricities may be abundantly conveyed from the conductor to the electroscope. This faculty continues with both electricities after the glass has ceased to be incandescent at all. It ceases with + before it ceases with —; indeed with the latter it is retained almost to a temperature as low as 100° C.

§ 49. *Experiment.*—In examining the discharging-power of red-hot glass some noteworthy facts are observed. When the red-hot glass is insulated, as when it forms the extremity of a long glass rod, there is a difference in the behaviour of + and — electricities. With + the hot glass merely acts as an inductive discharger; the leaves temporarily collapse, and return to their divergence when the glass is withdrawn. When the electroscope is charged with — electricity, the first approach of the hot glass causes partial collapse, as by simple induction. On withdrawing the glass, further collapse ensues; on bringing the glass again near, the same amount of divergence is produced which existed before withdrawal.

§ 50. On the 4th of January, 1873, at 3–4 P.M., I observed that the difference between the discharging-power for + and — electricity was greatly diminished and sometimes extinguished. There was at the time a severe storm over London, unaccompanied however, as far as I know, by electric atmospheric disturbance. On inquiry at the Observatories of Kew and Greenwich, Mr. Whipple informs me that, for a temporary reason, there is no record of the atmospheric electricity on that day. The Astronomer Royal informs me that the atmosphere was + on the morning of that day, but that of the afternoon there is no record.

§ 51. The following attempts were made to measure approximately the temperature (1) at which a hot insulated iron ball commenced or ceased to discharge — electricity when at a distance of 2 inches from an electroscope, (2) at which an insulated iron ball begins or ceases to discharge + electricity at the same distance of 2 inches, (3) at which there is no sensible difference between the discharge of + and —. These experiments were checked by examining the temperatures at which an insulated iron ball on cooling (1) commences to be able to receive —, (2) commences to be able to receive +, and (3) freely and equally receives both.

§ 52. *Experiments.*—The heats of balls at the critical temperatures were taken by plunging them into weighed quantities of cold water, weighing them both together. Taking into account the weight and specific heat of glass, and calling the sum of the heat-units in one gramme of iron from 0° C. to the temperature t° C.,

$$Fe\Sigma_0^t w,$$

it was found that

$$\begin{array}{rl} Fe\Sigma_0^t w = & \left.\begin{array}{r} 84\cdot7 \\ = 83\cdot7 \end{array}\right\} \quad \ldots\ldots\ldots \quad (1) \\ & \left.\begin{array}{r} =115\cdot9 \\ =116\cdot3 \end{array}\right\} \quad \ldots\ldots\ldots \quad (2) \\ & \left.\begin{array}{r} =145\cdot1 \\ =137\cdot0 \end{array}\right\} \quad \ldots\ldots\ldots \quad (3) \end{array}$$

At (1) both electricities cease to be discharged, and the insulated iron can serve as a proof plane for both kinds. Between (1) and (2) — electricity is discharged only, and the insulated iron may serve as a carrier for — electricity, but not for +. At (3) and above, both electricities are discharged equally, and the iron refuses to receive a charge of either kind.

August 28, 1873.

XXXII. *On a new Mechanical Theorem relative to Stationary Motions.* *By* R. Clausius.

[Concluded from p. 244.]

7. THE above-illustrated notion of the phase, which refers to periodic changes in the motion, can be employed in the consideration of motions which take place simultaneously in closed paths. But when we have a system of points which, though moving in a stationary manner, do not describe closed

paths, and with which also the individual variables by which the positions of the points are determined do not simply change their values periodically, a somewhat more general conception must be employed, which can be comprehended as the phase in a wider signification of the term.

Employing again the quantities $q_1, q_2, \ldots q_n$ for the determination of the positions of the points, without presupposing that every quantity repeats its variations regularly in periods of determined length, we shall yet introduce for each quantity a certain interval of time, which may be denoted by $i_1, i_2, \ldots i_n$. With the aid of these, we will define the phases belonging to the different quantities, and which may be called $\phi_1, \phi_2, \ldots \phi_n$, by the following equations:—

$$t = i_1\phi_1 = i_2\phi_2 \ldots = i_n\phi_n. \quad . \quad . \quad . \quad . \quad (12)$$

Now let $q_1, q_2, \ldots q_n$ be variated, and with each variable the phase belonging to it be regarded as the measuring quantity, which in the variation remains constant, while the time-interval may undergo an alteration. The variations so formed are, according to the above, to be represented by the symbols

$$\delta_{\phi_1}q_1, \delta_{\phi_2}q_2, \ldots \delta_{\phi_n}q_n.$$

Employing such a variation, we will form for the variable q_ν, the fraction

$$\frac{p_\nu\delta_{\phi_\nu}q_\nu - h\delta k}{t}.$$

If the quantity q_ν accomplished its variations in a periodical manner, and i_ν were the duration of its period, the variation $\delta_{\phi_\nu}q_\nu$ would also alter only periodically, and accordingly the fraction, which has t in its denominator, would make continually smaller fluctuations and so approximate to zero. The same would hold for all the n variables if they changed in a periodical way, in which each might have its special period-duration. But now we will not make the definite assumption that the variations of the quantities $q_1, q_2, \ldots q_n$ are periodic, but only fix the condition that the mean value of the sum

$$\Sigma\frac{p\delta_\phi q - h\delta k}{t}$$

for great times shall become very little—a condition which, according to the preceding, is at all events satisfied by periodic variations, but can also be fulfilled by other variations if they take place in a stationary manner.

After these preliminary remarks the following theorem can now be stated:—

If the variations, in the formation of which the quantities which exhibits a peculiar relation between the time-intervals i_1 and i_2 and the mean values of the ergal and *vis viva*.

If the mass μ be taken as very great in comparison with m, so that the fraction $\frac{m\mu}{m+\mu}$ may be supposed equal to m, the preceding equation changes into that which holds for the motion of one material point about a fixed centre. This equation I derived separately in a memoir published a short time since[*]; and I then said that for two points moving about each other the corresponding equation could in like manner be derived. Here, however, the same equation has come out as a special case of a much more general one.

To equation (II.) various other forms can be given, which are both theoretically interesting and convenient for use, wherein it can at the same time be brought into connexion with my theorem of the virial. These transformations and especially the application of the equation to the theory of heat I reserve for a subsequent memoir.

XXXIII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* By James D. Dana.

[Continued from p. 219.]

IV. *Igneous Ejections, Volcanoes*[†].

THE direct connexion of igneous ejections with the movements resulting from the earth's contraction has been briefly illustrated in the course of the remarks on mountain-making[‡]. It is apparent, without further discussion, that regions of great disturbances embrace the regions of great igneous ejections—that the oceanic slopes of the border mountains, especially around the Pacific, the greater ocean, have been preeminently subject to such eruptions—and that in Tertiary times, when the earth's crust was becoming too stiff to bend much before the lateral pressure, profound fractures instead of flexures were a common result of its action, igneous outflows were most extensive, and volcanoes were of increased size and numbers.

The questions remaining are:—

(1) Does the mobile rock owe its mobility in all cases, or any, to the movements of the rocks under lateral pressure—motion

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[*] *Nachr. der K. Gesellsch. der Wiss. zu Göttingen*, Dec. 25, 1872; Phil. Mag. July 1873.

[†] For Part I. see p. 41; Part II. p. 210; Part III. p. 217.

[‡] *Suprà*, pp. 52, 137, 212, 214.

transformed into heat being its source, in conformity with the views ably set forth by Mallet?

(2) Is the ejecting force in eruptions a result of the same lateral pressure?

1. *Source of igneous fusion**.—The suggestion of Professor Hopkins with regard to the earth's interior, which has been sustained on a preceding page†, offers for the source of igneous ejections the old one of viscous rock underneath the earth's crust; but viscous rock at first as a layer, and later as remnants of the same layer, left after the long cooling. This author suggests that these isolated or detached portions may be the source of modern volcanic eruptions; and this view is favoured by Scrope, the eminent vulcanologist‡.

The theory does not require that the isolated fire-lakes should exist now at the depth of the original viscous layer; for in the movements and flexures of the crust, and the opening of fissures for the escape of the mobile rock, great regions of it may have been pressed up and become isolated fire-lakes at a higher level. Or, as Professor Hopkins suggests, the subterranean sources of volcanic action may have been in shape like inverted cones extending far up into the crust, and may have finally been divided off into "smaller ones by a process similar to that by which the larger cavities were themselves formed from the general fluid mass, the position of each minor cavity being determined by the points in the roof of the larger one, where previous fractures had left the most perfect volcanic vents." This last explanation holds if regions of volcanic vents date back to early geological time, so that modern volcanoes in an area are successors to indefinitely older ones, but is less applicable where the vents of a region are of Cænozoic origin (as appears to be true of most of them) and a consequence of the later movements of lateral pressure.

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* I say nothing here on the nature or the degree of igneous fusion in plastic rock, as this is foreign to the subject under discussion.

† Professor Hopkins's argument from the amount of precession and nutation, in the Transactions of the Royal Society, 1839, 1840, and 1842, led him to the conclusion that "the thickness of the solid shell could not be less than about one fourth or one fifth of the radius of its external surface." In his paper in the Report of the British Association for 1847 he repeats his conclusion, and then considers the possible steps in the process of refrigeration. Among the different conditions discussed, he supposes as one (the one he favours) the temperature of fusion to be increased much by the pressure—at a rate much more rapid than "1° F. for an increase of pressure equivalent to about five atmospheres," and adds that then "we should probably have the condition under which solidification would commence at the centre. In this case, after incrustation had begun at the surface, the earth would consist of a solid central nucleus and a solid shell with fluid matter between them, as already explained, till the solidification should be complete." He then remarks, with regard to the present state of our globe, that "under the condition here assumed, with the observed rate of increase of terrestrial temperature in descending, there could be no reasonable doubt of the earth's entire solidity." He next treats more in detail of the process of solidification, and says, having in view the several conditions discussed, that "the incrustation may have constituted the very first step in the solidification, or it may have taken place after the formation of a solid nucleus surrounded by a superficial fluid envelope, according as the solidification began at the surface or centre; but in either case, as I have already intimated, the superficial crust when first formed must necessarily have reposed on a fluid mass beneath." He next explains his views as to the formation of the crust, and the final obliteration of the viscous layer; and states, with reference to volcanoes:—"By a continuation of this process it is obvious that the superficial crust of our globe must at length arrive at what I conceive, for reasons above assigned [in a paragraph on p. 33, speaking of the isolated sources of volcanoes], to be its present condition,—that of a solid mass containing numerous cavities filled with fluid incandescent matter, and either entirely insulated or perhaps communicating in some cases by obstructed channels." These cavities are made the sources of the material of volcanic eruptions.

‡ Volcanoes, 2nd edit. 8vo, pp. 264, 268.

The inference that igneous eruptions have generally been derived from a deep-seated source is sustained by the great lithological uniformity of ejections over widely distant ranges of surface. The eruptive rocks (or trap) of the Triassico-Jurassic areas of the Atlantic border from Nova Scotia to the Carolinas, already many times referred to*, all which belong to one epoch, are solely varieties of dolerite—rocks made up essentially of Labradorite and pyroxene, with more or less of magnetic iron-ore in disseminated grains or crystals. Analyses have shown that the trap of this region in Connecticut and New Jersey is essentially identical in constitution; and an examination and determination of the density of specimens of the same rock from North Carolina and Nova Scotia leaves no doubt that it is fundamentally alike throughout. The aspect of the ordinary specimens from these widely distant regions is closely the same; and the density varies little from 3. The dolerite of East Rock, New Haven, Conn., gave Professor G. J. Brush for the specific gravity 3·09–3·085; from West Rock, ib., 3·045–3·07; from the vicinity of Raleigh, North Carolina (a specimen received by the writer from Professor Kerr, of Raleigh, and not distinguishable by the eye from an East or West Rock specimen), 3·16. Professor Cook, of New Jersey, obtained for the dolerite of the Palisades (Geol. Rep. N. J. p. 215) 2·94. Professor H. How gives the writer, for a similar variety from Nova Scotia, 2·94. Complete analyses have not been made of many of these rocks. The following are analyses of the West-Rock (New Haven) dolerite and that of the Palisades, New Jersey (seventy miles west of West Rock)—the former by W. G. Mixter, Assistant Chemist in the Sheffield Scientific School, and the latter by Professor G. H. Cook, of the New Jersey Geological Survey:—

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* These hills and dikes of "trap" (as they are ordinarily called) have great length and breadth in Nova Scotia. In the Connecticut valley they are very numerous, and in many lines—as laid down, especially for Connecticut, by Percival in his 'Geological Report.' A copy of a part of his very accurate detailed map of the trap-region is reproduced in the writer's 'Manual of Geology,' page 20. At the east and west bend in the dikes, south of the middle of the map, are the Hanging Hills of Meriden, 900 to 1000 feet high, situated about 20 miles north of New Haven (and 12 miles from the southern margin of the map); and the range which extends from these hills north continues to Mount Tom, near Northampton, in Massachusetts. The range to the east, south of this bend, continues southward to Lake Saltonstall, east of New Haven. Other lines cut through the metamorphic rocks: one of these lines of trap-ejections in the eastern metamorphic region of Connecticut, as mapped by Percival, extends from Long-Island Sound, 6 miles east of New Haven, nearly to the southern boundary of Massachusetts, over 70 miles, with a nearly east-north-east trend, consisting of an interrupted series of dikes, intersecting metamorphic rocks of various kinds. A second independent Triassico-Jurassic trough exists in Connecticut, 15 to 20 miles west of the main one, over part of the towns of Southbury and Woodbury; and this also has its numerous trap dikes. The Palisades of New Jersey, bordering the Hudson River, are the northern part of a complex series that continues southward and westward through New Jersey and Pennsylvania into Virginia, following the course of the Triassico-Jurassic sandstone areas. There is also a series in the North-Carolina area.

	SiO^2.	$2AlO^3$.	FeO.	MgO.	CaO.	KO, NaO.	Ign.	
1. **West Rock.**	52·37	12·31	13·13	6·03	10·74	2·62	0·94	= 98·14
2. **Palisades** .	53·9	17·5	8·0	10·3	8·0	[2·3]	..	= 100

There are variations in the dolerite of the regions above referred to depending on the proportion of the felspar and also on the presence of water; but they occur in various parts of the several regions instead of characterizing any one of them. The dolerite of dikes in East Haven, Conn. (the town adjoining New Haven on the east), diminishes in lustre as the dike is more remote from the New-Haven line; and in the ridges near Saltonstall Lake, two miles distant, it is faintly glistening and of somewhat less hardness; and although generally solid throughout, it is in some parts vesicular (amygdaloidal). A precisely similar rock in all its characters occurs among the trap-hills (dolerite) of Meriden, called the Hanging Hills; and part of it is amygdaloidal. The same is found also in Nova Scotia. An examination of a specimen from the vicinity of Saltonstall Lake, by Professor O. D. Allen, shows the presence of 4·53 per cent. of matter driven off on ignition, after deducting the hygroscopic moisture (0·60). It indicates the presence of some hydrous mineral (chlorite apparently) in place of part of the augite or felspar; but the percentage of silica is 49·88, 50·10, according to two determinations by S. T. Tyson, of the Sheffield Scientific-line of the surface terminated at two given points not on the axis; but for the sake of simplicity I shall take the problem as above enunciated.

The history of the various attempts that have been made to solve this problem is very instructive relatively to the principles of the Calculus of Variations, especially as regards the distinction between continuous and discontinuous solutions. With respect to the principle of discontinuity, it may first be remarked that the integral of the equation designated as $M=0$ in Mr. Todhunter's work (the same that I have usually called $A=O$) may either give one line, or two or more lines. But in such cases there will be, according to Algebraic Geometry, a factor corresponding to each line, and the answer to the question may be given by parts of lines terminating at points where two lines cross each at *finite* angles of inclination. This is the case in the solution I gave of the Problem of the Brachistochronous course of a Ship, which is considered by Mr. Todhunter (Chap. I., art. 12) to be the first instance of a solution exhibiting this kind of discontinuity. The problem before us presents another instance of the same kind, inasmuch as we know beforehand that the spherical form is that for which the volume for a given surface is a maximum, and that consequently one solution of the problem should be given by a straight line coincident with the axis of revolution and a semicircle terminating at both ends in that line. It is found in fact that this solution is deducible from the equation (δ) when b^2 is supposed to vanish, in which case $y=0$, which is the equation of the straight line, and $y=\frac{a}{\sqrt{1+p^2}}$, which is the differential equation of the circle. Moreover the differential equation $pM=0$, which requires the factor p for effecting the integration, is satisfied by $y=0$, the equation of the straight line, which causes p to vanish, and by $x^2+y^2=4a^2$, the equation of the circle, which causes M to vanish. This discontinuous solution was given by the Astronomer Royal in the Number of the Philosophical Magazine for July 1861.

Again, as has been already stated, the integral of the equation (δ) gives the curve which is described by the focus of an hyperbola rolling on a straight line. But, contrary to what might have been expected, this curve by no means gives the solution of the problem. It is wholly inapplicable if the positions of the extremities of the curve, whether on or off the axis, be given; but if the abscissæ only of the extreme ordinates be given and the surface generated by the revolution of these ordinates and the curve joining their extremities be also given, the solution shows that the form of this curve is that traced by the focus of a rolling hyperbola, that the extreme ordinates are equal, and

The discovery of this discontinuous solution, which differs in kind from that discussed above, is due to Mr. Todhunter.

We have not, however, as yet arrived at a solution which gives a single continuous curve joining the given points—although, from considerations exactly like those adduced in the case of the preceding problem, it is certain that such a solution exists. The difficulty, as in that case, is overcome by employing the proposed new method of integration, which in the present instance consists in first deducing from the equation (δ) the differential equation of the first order of any curve parallel to the curve traced by the focus of the rolling hyperbola, and then showing that the equation (η) resulting from the elimination of the constants from that differential equation is verified if $F=0$ and $\frac{dF}{dx}=0$, the function F being $y-\frac{a}{\sqrt{1+p^2}}+\frac{ayq}{(1+p^2)^{\frac{3}{2}}}$. The parallel curve, as I have already argued, is under these circumstances an integral of $F=0$; and since its equation contains one more arbitrary quantity than the equation of the curve itself, it is capable of satisfying the given conditions of the problem. I have ascertained, for instance, that if the given surface be of small amount for a considerable distance between the given points on the axis, the form of the curve is like the arc of a bow; but if the amount of surface be large for a small interval between the points, the curve approaches the circular form. Under the supposed circumstances such forms might be antecedently expected to be given by the results of the analytical calculation.

The foregoing solutions of the two problems do not essentially differ from those I proposed in an article contained in the Philosophical Magazine for July 1871. But the latter solutions were effected by actually finding the equations of the evolutes corresponding to the two series of involutes; whereas the present research has shown that this process was not necessary, and that the new integration is virtually an extension of the recognized methods of integrating by factors and by differentiation. The principle of the application of this new method in these two instances may be briefly stated as follows. A differential equation $F=0$ of the second order may have an integral containing *three* arbitrary constants, but only on the condition that the differential equation of the third order obtained by eliminating these constants from the integral is verified by the *two* equations $F=0$ and $\frac{dF}{dx}=0$, and not by either singly. The reason is, that as the equation $\frac{dF}{dx}=0$ is a necessary consequence of $F=0$, the latter equation, in case the above condition can be fulfilled, is really of the third order.

I have now reached the conclusion of my researches in the Calculus of Variations, having succeeded at length in removing from its analysis the reproach of failing to solve a class of problems which unquestionably should thereby be capable of solution. For instance, it is obvious that there must be a continuous surface of revolution which for a given amount of surface and a given length of the axis encloses a maximum volume. But so far was this problem from being solved, that, on account of its peculiar difficulties, those who from their reputation as analysts might be expected to furnish a solution, have only endeavoured to show that it is insolvable. Probably, therefore, it will be held to be excusable that in my first attempts to solve it I adopted views and processes of reasoning which I had afterwards to abandon as untenable. I persevered, however, in my efforts to arrive at the solution, not being able to admit the possibility of the failure of analysis; and the results now communicated I consider to be a justification of this course.

Cambridge, October 8, 1873.

L. *On Statical and Dynamical Ideas in Chemistry.*—Part IV. (*conclusion*). *On the Idea of Motion.* *By* Edmund J. Mills, *D.Sc.*[*]

Contents.

A criterion of scientific progress is needed. This criterion is an idea common to all the sciences—namely, the idea of pure motion. It was first announced by Herakleitos of Ephesus; we afterwards trace it through the Platonic Sokrates, the Sophists, Aristotle, the Epicureans, and the Skeptics. It reappears in Hobbes and Hegel. Ferrier and Spencer compared with reference to this idea. All conscious knowledge is but knowledge of motion. The science of the naturalist, mathematics, and geology are illustrations of the directive force of the idea. In chemistry—which has not, like those sciences, assumed a "new" phase—a theory of absolute limits prevails; but its great epochs are intelligible only by the light of this criterion: its imaginary pursuits; study of the chemical process neglected. The extinction of ethics. Questions. Summary. Practical tendency of the discussion.

Ποταμοῦ ῥοῇ ἀπεικάζων τὰ ὄντα.

If a chemist, in the course of his researches, were to suspend for a moment his more immediate investigation and ask himself *Is this Progress?* he would have a difficult question to answer. I do not think it advisable that this question should often be asked, or that every chemist should ask it. But it does arise in the most legitimate manner; and, as it has a transcendant importance in the economy of intellectual effort, an answer is imperatively required. Moreover, as in every science the same demand springs up, how vast must be the interests concerned, how great the value of a right response!

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[*] Communicated by the Author.

The question stated presupposes that a deficiency or a requirement does actually exist—in other words, that the science from which the question comes in some respect differs from the general scientific level. Now in Part I. of these papers[*], I pointed out what is the universal criterion of progress—namely, "the most general idea existing at a given time as a factor in every branch of science." My position may be illustrated by an experimental usage. Suppose a phenomenon has to be observed by reading off a number; this is done, not once, but many times, and the average result is calculated. The average is certainly more valuable than either of the observations taken singly; and, if these are regarded as combining towards an end, it is contained to a greater or less extent in each of them. The idea of pure motion is asserted to be the mean or general idea sought. I now propose to explain more fully in what sense this assertion is made, and to discuss more at large than was possible in Parts II. and III. the nature of the criterion itself.

The general nature of the criterion establishes its universal applicability. But I address myself more especially to chemists; and their science is at the present time, and at this instant to the judicial reader, in the very crisis when the adoption of a real criterion is of the supremest consequence. Sad in the poverty of symbolic resources, miserably unreasoned and deficient in power, it urgently requires an entire reform of its prevailing theory. The student more especially, and the teacher (who is in each instruction a necessary artificer of prejudices), have need to pause, and find or verify that "*word,*" which, like the *κριτικὸς λόγος* of St. Paul[†], is to divide and penetrate all the problems of knowledge.

The history of the idea of pure motion is comparatively brief and simple. Herakleitos of Ephesus (460 B.C.), who first announced it, had clearly seen its vast importance and universal applicability; and, though termed by his successors The Dark (*Σκοτεινός*), he was undoubtedly understood by many of them. The Sokrates of Plato[‡] seems on the whole, to accept Herakleitos's doctrine[§] and frequently alludes to it; where he treats it humorously, the sport is rather at the expense of his auditors' intelligence than concerning his subject. Herakleitos pointed out the illusory nature of the seeming permanence or stationary condition of things: "every thing moves, and nothing remains," he said. The flow of a river is an apt illustration of the fleeting nature of the world; in which, indeed, the only reality is the act of transition, or the "becoming;" not inception and results, but process. In the instant of "becoming" he conceives the contrary determinations of being and not-being united,—a notion which Professor Ferrier has beautifully illustrated by the well-known geometrical explanation of two opposite forces combining in a continuous curvilinear path. The sophists, especially Protagoras, applied this doctrine, though not with the highest success, to justice and morals, chiefly in the interest of the freedom of the individual will. In the Aristotelian philosophy, the conditions of nature are summed up as *motion*, *space* (the possibility of motion), and *time* (the measure of motion); but, in Aristotle, the idea becomes less pure, is circumscribed with limits in the detail, and loses in elasticity and vigour. The same is true of the Epicureans; but it formed the latent basis of their philosophy, as likewise of the Skepticism of every age.

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[*] Phil. Mag. S. 4. vol. xxxvii. p. 461.

[†] Hebrews, chap. iv. verses 12, 13.

[‡] In the *Theætetus*.

[§] Ferrier (Lectures on Greek Philosophy, vol. i. pp. 145, 146) writes of Herakleitos, "here, if anywhere, is the embryo of the solution of the enigma of the universe. I am convinced that the unity of contraries is the law of

In much more modern times, the idea of motion was most distinctly grasped by Hobbes, as may be seen by referring to his 'Humane Nature,' 2nd edition (1650), from which work I have taken the following statements. "That the Subject wherein

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all things—that all life, all nature, all thought, all reason centres in the oneness or conciliation of Being and not-Being. A firm grasp of this doctrine, a clear insight into its truth, and a vigorous enforcement of it and its consequences, would lead to the construction of a truer philosophy than that which is at present so much in vogue. That philosophy is founded entirely on the denial of the unity of contrary determinations in the same subject. It takes two opposite conceptions, and holding them apart it shows that reason is baffled in its attempts adequately to conceive either of them. It is in this way that Sir W. Hamilton and Mr. Mansel achieved what they conceive to be a great triumph in proclaiming, or, as they think, in proving the impotency of human reason. But what if the conceptions thus set in opposition to each other are not conceptions at all, but are mere moments or elements of conception?" Compare with this the following passage from Herbert Spencer (First Principles, 2nd edit. p. 277). "The law we seek, therefore, must be the law of *the continuous redistribution of matter and motion*. Absolute rest and permanence do not exist. . . . . And the question to be answered is—What dynamic principle, true of the metamorphosis as a whole and in its details, expresses these ever-changing relations? . . . . . a Philosophy rightly so-called can come into existence only by solving the problem." And again, p. 285, "While the general history of every aggregate is definable as a change from a diffused imperceptible state to a concentrated perceptible state and again to a diffused imperceptible state, every detail of the history is definable as a part of either the one change or the other. This, then, must be that universal law. . . . ."

Colour and Image are inherent, is *not* the *Object* or thing seen. That there is nothing *without us* (really) which we call an Image or Colour. That the said Image or Colour is but an *apparition* unto us of the *motion*, agitation or alteration which the *Object* worketh in the *Brain*, or spirits, or some internal substance of the head. That as in Vision, so also in conceptions that arise from the *other Senses*, the subject of their inherence is not the *Object*, but the Sentient." (p. 9.)

Hobbes also says that the reflex stroke from the brain to the impressing object constitutes our *sense* of colour, form, sound, etc. What is really outside is "*motions*, by which these seemings are caused." (p. 18.)

"*Conceptions* and *apparitions* are nothing *really*, but motion in some internal substance of the head." (p. 69.)

"All *evidence* is *conception* . . . . and all conception is imagination, and proceedeth from *Sense*." (p. 135.)

Hobbes does not seem to have anywhere definitely stated the final conclusion deducible from the above propositions. But they involve a sorites, the beginning of which is the mental act, the end of which is motion: motion is therefore exclusively our being. The argument may be exhibited as follows. I only know of events by sensations, which can only be represented as motion; the organs or parts of my body whereby I know these are themselves events, both to me and other intelligences. All knowledge of events is therefore knowledge of motion. In "events" are included the emotions, will, and intellect of other persons; consequently my own. A moving body is merely directed or relative motion. This theory must apply at least to all conscious knowledge*.

The last philosopher who can be said to have advocated the idea of pure motion was Hegel. In his dialectical method, which always advances "from notion to notion through negation"—in his doctrine of being, as always (pendulum-like) vanishing into nothing and back again—in these and other principal features of Hegel's system, we observe, as its author acknowledged, the restoration of the Herakleitic principle. It would be a mistake, however, to suppose that Hegel, in endeavouring to trace out with much minuteness the universal prevalence of his dialectic, agrees at all points with his Grecian predecessor; but, on the whole, his fundamental idea is most convincingly the same.

The philosophic aspect of the idea of pure motion, as portrayed in the preceding paragraphs, indicates the prevalence of that ideal influence from very early historic times until now; and shows, moreover, how natural it is and must be to man's mind to acquire. Great thoughts, however, do not remain for ever without fruit, even when held unconsciously by ignorant people. Secret and imperceptible, they grow. Hence it becomes important to ascertain whether there are any decided practical evidences of the idea of pure motion in modern science.

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* Further expositions of Hobbes's view will be found in his work 'The Leviathan' (1651), pp. 3, 352, 369, 374, 375.

The science of the naturalist had always proved dark and difficult whenever an attempt was made to transcend facts and verge upon principles. Its valuable functions were those of a warehouseman and clerk—to collect and register; its speculations hovered over the facts rather than rested upon them. The light but pungent satire of Reybaud must be admitted to have had its point. "Celui-ci, me disait-il, appartient corps et âme aux entomozoaires; il a eu la chance de découvrir une quinzième articulation dans un insecte, et des antennes que personne n'avait soupçonnées avant lui . . . . Il passera à la postérité avec son hymenoptère, sans compter une espèce de scolopendre qui lui a de grandes obligations. Supprimez cet homme de la communauté humaine, et voilà des scolopendres qui n'occupent pas, dans l'échelle des êtres, le rang qui leur appartient. Lui seul a pu en faire huit genres, douze sous-genres, sans compter les variétés." Wide as was the naturalist's scope, his chief duty was the discovery, description, and arrangement of species—species believed, for the most part, to have been distinctly created, and to be each one of them an instance of a break in nature. But the subtle principle of motion, altogether opposed to stationary points, or discontinuity, can now be seen to have been ever asserting itself, asserting itself in more or less fanciful theories of archetypes, of the emanations of beings from few sources, of a fundamental identity of origin for animals and plants*. At length, with seeming but not veritable suddenness, the beautiful and harmonious hypothesis of Darwin and Wallace arose, and ere long was found to comprehend within its grasp the varied forms of the world's life. For many years to come it will be the finest task of the naturalist to work at the verification of that hypothesis, though we have even now the present value of such verification. Darwin's theory is familiar to every one; the principle of it may be gathered from the following statements of its originator†:—"A French author, in opposition to the whole tenor of this volume, assumes that, according to my view, species undergo great and abrupt changes, and then he triumphantly asks how this is possible, seeing that such modified forms would be crossed by the many which have remained unchanged."

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* It remains for the philosophic chemist and physicist to bridge over the artificial barrier between "living and non-living matter," and to prove that life is the common property of all sensuous objects.

† On the Origin of Species by means of Natural Selection (1866), pp. 146, 232, 246.

"Why should not nature take a sudden leap from structure to structure? On the theory of natural selection we can clearly understand why she should not; for natural selection acts only by taking advantage of slight successive variations; she can never take a sudden leap, but must advance by short and sure though slow steps."

"On the theory of natural selection we can clearly understand the full meaning of that old canon in natural history, 'natura non facit saltum.' This canon, if we look only to the present inhabitants of the world, is not strictly correct; but if we include all those of past times, whether known or not yet known, it must by my theory be strictly true."

Here, then, we have, stated in the clearest language, explicit evidence that the great principle acquired by the naturalist, and now governing the entire extent of his observations, is continuity, a derivative form of the idea of motion.

I turn to the science of algebra. It, too, has gone through a "new" phase, and now exists under conditions which are most remarkable, though these conditions have been developed in the most gradual and orderly manner from the time of Newton, as by him from his predecessors. It, too, is morphological; through it every branch of modern mathematics has again become young. The interoperation of algebraic forms corresponds to those conflicting conditions of nature by which species are produced and maintained. But what sun was it that gave such light to the mathematician? The same that shone on that other continent of the naturalist. "Time was when all the parts of the subject were dissevered*, when algebra, geometry, and arithmetic either lived apart or kept up cold relations of acquaintance confined to occasional calls upon one another; but that is now at an end; they are drawn together and are constantly becoming more and more intimately related and connected by a thousand fresh ties; and we may confidently look forward to a time when they shall form but one body with one soul. Geometry formerly was the chief borrower from arithmetic and algebra; but it has since repaid its obligations with abundant usury; and if I were asked to name, in one word, the pole-star round which the mathematical firmament revolves, the central idea which pervades as a hidden spirit the whole corpus of mathematical doctrine, I should point to Continuity as contained in our notions of space, and say, it is this, it is this!"

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* Sylvester, British Association's Report (1869), Transactions of Sections, p. 7.

In the antagonism between continuity in mathematics and alleged absolute limits in chemistry, we see the reason why so few chemists are mathematicians, and so few mathematicians chemists.

Geology has, in recent years, shaken itself free from the fetters of the cataclysmal school, and finds in existing forces, continuously exerted, an adequate explanation of her special phenomena.

Chemistry still looks with half-averted face upon all dynamical doctrines. But her great centres of historic conflict are intelligible only by their aid. Acid, Alkali, Base, and Salt are not capable of definition as particular things*; the principle of continuity alone renders them clear. Chemical Substance is homogeneous, not discontinuous substance; Chemical Functions are modes of motion†. The Atomic Theory, triumphant still, is more suspected than before; but it is indeed a better servant to pure dynamics; for it places before the mind, daily and most distinctly, the fatal consequences of the assumption that quantity consists of parts‡. Grave and mature chemists now investigate the position of a particular atom in an aromatic compound, and find it at the side, in the middle, or near some other portion of an open or closed chain. In the mean time we hear nothing of the chemical process. Such are our dreams; we think them so regular, orderly, and reasonable. And so they are, until the morning; *then* we shall be the first to laugh at ourselves. For my own part, the perusal of modern chemical literature fills me with an abiding sense of sorrow and shame.

In the practical side of philosophy the idèa of motion, in various derived forms, is also found ascendant. It is for the most part to philosophers that the intellectual bias of modern nations is due; and the characteristics of contemporary society, having been so derived, are in the main dynamical. The sentiments, the affections, the passions of mankind have been flooded with liberty and power. It has been found advisable, and even necessary, to remove many of the older educational restraints, and to qualify at least the social distinctions we preserve. The science of ethics has expired, with all necessity for formal sanctions. The notions of right and wrong can now be derived historically, and are no longer mysterious. While Idealism has, on the one hand, restored to its supreme and lawful rank the individual Self, it should be the business of the schools to make men know that; in the mean time, the wide demand for liberty of the will is not only audible but imperative. But it is evident that, as soon as the ethical sanction is defined as liberty or freedom, no sanction any longer exists; act and motive are identical. This strange phenomenon of the destruction of a science has been witnessed by the present generation; it is as rare in human history as the extinction of a star.

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* Phil. Mag. S. 4. vol. xxxvii. p. 461.

† Ibid. S. 4. vol. xl. p. 259.

‡ Ibid. S. 4. vol. xlii. p. 112.

With regard to other departments of practical philosophy, I need merely state the names of politics, religion, and sociology, and leave it to the candid reader to consider whether their nearer history has been more characterized by freedom or definition. To his judgment I also relinquish the questions whether liberty, toleration, the scientific spirit, and the growth of all our noblest interests, are not more likely to be fostered by cultivating the great standard I have discussed than by the dogma of an absolute limit. Where can we find "contentment, but in proceeding"*? or peace, but in action? or hope, but in the drift of nature?

Thus, then, having reflected upon the principles of our daily actions and the more recondite sources of our scientific life, we see rising from them all, like a spirit out of darkness, the perfect majesty of motion. This is the centre, this is the vast and ever-receding circumference of fulfilled desire. All beauty, all life, all thoughtful power, are contained in it and are it. Like a river it has glided on, silent, devious, unperturbed. From a Grecian source toward the untravelled sea the stately ships of science take freight and spread their sails upon its bosom; the gilded craft of music, of poetry, and of rhetoric disport there; and the tiny barks of incipient civilization have but that single channel. It has drained, refreshed, and fertilized every continent of thought and feeling. At a distance from its margin are chiefly found the hideous desert and the dry rocks of old dogmatic conflict; but nearer to the shore come soft oases; and its banks are fertile vales, content, and sweet and tranquil. All remote inspirations, all scattered fragments of knowledge, are but dew or clouds exhaled at first from its surface, or wafted from its waves.

Ah, Shade of Herakleitos! leave his urn, and weep no more; for the teachings of that mighty mind are not only understood, but have been verified, and we can now coordinate them from new forms. Henceforth it remains but to apply his great idea, now seen to be the widest generalization from experience—to turn round, and, journeying backward into detail, take that as both map and compass on the road. Henceforth, for us who have chosen the criterion, all that is good and desirable is motion, all that is evil and to be dreaded is a limit. Let no one strive to reconcile them.

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[*] Hobbes.

12 Pemberton Terrace,
St. John's Park, N.

LI. *Notices respecting New Books.*

An Elementary Treatise on the Differential Calculus, containing the Theory of Plane Curves, with numerous Examples. By Benjamin Williamson, *A.M., Fellow and Tutor, Trinity College, Dublin.* Second Edition, Revised and Enlarged. London: Longmans, Green, and Co. 1873. (Pp. 367.)

WE noticed the first edition of this book, shortly after its publication, about a year and a half ago (April 1872). There is therefore no need to say much on the present occasion. The revision to which the work has been subjected consists partly in a change of arrangement of some of the articles (*e. g.* the chapter on Lagrange's Theorem, which was Chap. 20 in the first edition, is Chap. 7 in the second), partly in the addition of articles here and there throughout the volume (*e. g.* the articles on Linear Transformation, Nos. 292–294 added to the chapter on Change of the Independent Variable), partly in the insertion of additional Examples. The total number of changes is considerable, and the result of the whole is to increase the volume by 24 pages; still the changes do not make what can be regarded as a substantial alteration in the work. It only remains, therefore, to congratulate the author on the rapid sale of the first edition, and to express a hope that his very useful book will continue to find many readers.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 326.]

May 1, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On the Effect of Pressure on the Character of the Spectra of Gases." By C. H. Stearn and G. H. Lee.

The variations in the spectra of gases which accompany changes of density have been studied by Plücker and Hittorf, Frankland and Lockyer, Wüllner and others.

It appears to us that one cause to which these changes may be due has been overlooked, and that many of the observed variations are entirely independent of the density of the gas. If a Leyden jar be placed in the circuit, and the current from an induction-coil be passed through a Plücker's tube containing nitrogen with the traces of hydrogen generally present, the following well-known phenomena are observed.

When the gas is near atmospheric pressure, the line-spectrum of nitrogen is brilliant, and the F line of hydrogen is broad and nebulous. As the pressure is reduced, the lines of nitrogen gradually fade out, and the band-spectrum appears, while at the same time the F line of hydrogen becomes narrow and well defined. If fresh gas be admitted, the line-spectrum reappears, accompanied by a widening of the F line.

That these changes are not dependent on the density of the gas, appears from the following experiment:—

A sealed tube containing nitrogen, with traces of hydrogen at a pressure of about 2 millims., was placed before the spectroscope. A second tube was connected with the air-pump, and the current passed through both tubes, a Leyden jar being placed in the circuit.

When the pressure in the second tube was high, the line-spectrum of nitrogen appeared brilliantly in the sealed tube, and the F line was broad and nebulous; as the exhaustion proceeded these lines faded out, and the F line became narrow, in precisely the same manner as if the sealed tube had been in process of exhaustion. The explanation appears to be that the production of the line-spectrum of nitrogen, and the expansion of the F line of hydrogen, depend entirely on the intensity of the charge communicated to the Leyden jar. When the pressure of the gas between the electrodes is high, the discharge does not take place until the jar is fully charged; but as the exhaustion proceeds a less and less charge is communicated to the jar, and the discharge at last is virtually not more than that of the simple current.

The same effect may be produced by interposing a break in the circuit, the length of which may be increased as the pressure in the tube is reduced. Plücker and Hittorf appear to have used a break, as in their paper in the Philosophical Transactions, Nov. 1864, they speak of the expansion of lines obtained by increasing the charge of the jar by an interposed stratum of air. They do not, however, appear to have noticed that the reduction of pressure in the tube was only equivalent to a diminution of the charge of the jar, and that to this cause many of the changes of spectra which accompany the reduced pressure ought to be ascribed.

We are continuing our experiments on the effect of temperature on the spectrum, but prefer to reserve this portion of the subject for the present.

May 8.—Francis Sibson, M.D., Vice-President, in the Chair.

The following communication was read:—

"Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—No. II. By J. Norman Lockyer, F.R.S.

The observations in this paper are a continuation of those referred to in the previous communication bearing the same title. They deal (1) with the spectra of chemical compounds, and (2) with the spectra of mechanical mixtures.

I. *Chemical Compounds.*

Several series of salts were observed; these series may be divided into two:—1st, those in which the atomic weights varied in each series; 2nd, those in which the associated elements varied in each series. The following salts were mapped:—

PbF_2, $PbCl_2$, $PbBr_2$, PbI_2; SrF_2, $SrCl_2$, $SrBr_2$, SrI_2; BaF_2, $BaCl_2$, $BaBr_2$, BaI_2; MgF_2, $MgCl_2$, $MgBr_2$, MgI_2; NaF, $NaCl$, $NaBr$, NaI.

The conditions of the experiments are described. The same aluminium cups, described in the first paper, were used; and the poles were arranged in such a manner that they could at will be surrounded with any gas or vapour. Hydrogen was used in some of these experiments; it was purified in the usual manner by drying and freeing from traces of sulphuretted hydrogen; it was then passed over clean-cut pieces of sodium, and admitted to the poles. An induction-spark from 5 one-pint Grove cells was used, the *circuit being without the Leyden jar.*

The lead compounds behaved (in air) as follows:—

The fluoride gave the eleven longest lines of the metal; but four were very faint.

The chloride gave nine lines; one of these was very short.

The bromide gave six lines; but one was a mere dot on the pole.

The iodide gave four lines distinctly and two as dots, one of which was scarcely visible.

It is pointed out that the decrease in length and number of lines follows the increase in the atomic weight of the non-metallic element, the lines dying out in the order of their length.

Barium was next experimented on, the same series of salts being used. A marked departure from the results obtained in the case of the lead compounds was observed, especially in the case of the fluoride, its spectrum being much the simplest; in fact it consisted of only 4 lines. Strontium behaved like barium; and so did magnesium fluoride. This anomalous behaviour was found to be most probably due to the exceedingly refractory nature of these fluorides, all of them being quite infusible, and non-volatile in any spark that was used.

Sodic fluoride, sodic chloride, sodic bromide, and sodic iodide exhibited a behaviour exactly the reverse of that of lead; *i. e.* the iodide showed most of the metallic spectrum.

The difference between flame-spectra and those produced by a weak electric discharge are then discussed. Beads of the chlorides &c. were heated in a Bunsen gas-flame. BaI_2 gave a "structure" spectrum (since proved to be due to the oxide) and the line at wave-length 5534·5, by very far the longest metallic line of barium. The bromide behaved like the iodide; and so did the chloride, except that its spectrum was more brilliant. Baric fluoride gave scarcely a trace of a spectrum, the oxide structure being scarcely visible, and 5534·5 very faint indeed. The strontium salts follow those of barium—4607·5, the longest strontium line, appearing in conjunction with an oxide spectrum. The strontic fluoride, however, refused to give any spectrum whatever. These results are compared with those obtained with the weak spark; and it is shown that the difference is one of degree: *e. g.* baric bromide gives 25 lines in the spark; these are the longest lines. In the flame it gives but one line; but this is the longest of all the barium lines, and indeed very far exceeds all the others in length. When the flame-spectra are compared with those produced by the low-tension spark, the spectra of the metals in the combination are in the former case invariably more simple than in the latter, *so that only the very longest line or lines are left.*

Some experiments made by Mr. R. J. Friswell to determine the cause of the similarity of the spectra of the various salts of the same metal observed in air are then given, the conclusion being that the spectrum observed is really that of the oxide.

Kirchhoff and Bunsen's, Mitscherlich's, and Clifton and Roscoe's prior conclusions on the points investigated are stated at length; and it is shown that the observations recorded, taken in conjunction with the determination of the long and short lines of metallic vapours, are in favour of the views advanced by Mitscherlich, Clifton, and Roscoe. For while the spectra of the iodides, bromides, &c. of any element in air are the same, as stated by Kirchhoff and Bunsen, the fact that this is *not* the spectrum of the metal is established by the other fact, *that only the very longest lines of the metal are present, increased dissociation bringing in the other metallic lines in the order of their length.*

The spectra have been mapped with the salts in hydrogen: here the spectra are different, as stated by Mitscherlich; and *the metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the case of the least-volatile one.*

The following are the conclusions arrived at:—

1. A compound body has as definite a spectrum as a simple one; but while the spectrum of the latter consists of lines, the number and thickness of some of which increase with molecular approach, the spectrum of a compound consists in the main of channelled spaces and bands, which increase in like manner. In short, the molecules of a simple body and of a compound one are affected in the same manner by their approach or recess, so far as their spectra are concerned; *in other words, both spectra have their long and short lines or bands.* In each case the greatest simplicity of the spectrum depends upon the greatest separation of molecules, and the greatest complexity (a continuous spectrum) upon their nearest approach.

2. The heat required to act upon a compound, so as to render its spectrum visible, dissociates the compound according to its volatility: the number of true metallic lines which thus appear is a measure of the dissociation; and doubtless as the metal lines increase in number the compound bands thin out.

Mitscherlich's observations, that the metalloids show the same structural spectra as the compound bodies, is then referred to, and the question is asked whether the molecules of a metalloid do not in structure lie between those of elements on the one hand and of compounds on the other.

These considerations are applied to solar and stellar spectra. The general appearance of the solar spectrum shows that in all probability there are no compounds in the sun.

Secchi's maps of a large number of stellar spectra are referred to as now indicating beyond all doubt the existence of compound vapours in the atmosphere of some stars; and it is suggested that the phenomena of variable stars may be due to a delicate state of equilibrium in the temperature of a star, which now produces the great absorption of the compound and now that of the elemental molecules.

II. *Mechanical Mixtures.*

The second part of the paper deals with the mechanical mixtures. Maps of the spectra of alloys of the following percentages are given:—

Sn and Cd	percentages of	Cd	10·0, 5·0, 1·0, 0·15.
Pb and Zn	" "	Zn	10·0, 5·0, 1·0, 0·1.
Pb and Mg	" "	Mg	10·0, 1·0, 0·1, 0·01.

It is pointed out that the lines disappear from the spectrum as the percentage becomes less, the shortest lines disappearing first —and that although we have here the foreshadowing of a quantitative spectrum-analysis, the method is so rough as to be inapplicable.

It is then stated that further researches on a method which promises much greater accuracy are in progress.

The bearing of these results on our knowledge of the reversing layer of the sun's atmosphere is then discussed.

LIII. *Intelligence and Miscellaneous Articles.*

ON THE CONDENSATION OF GASES AND LIQUIDS BY WOOD-CHARCOAL. THERMIC PHENOMENA PRODUCED ON THE CONTACT OF LIQUIDS AND CHARCOAL. LIQUEFACTION OF THE CONDENSED GASES. BY M. MELSENS.

ABSORPTION of chlorine by wood-charcoal may go on until it represents a weight of chlorine equal to that of the charcoal; consequently the condensing force of the latter may serve to realize the liquefaction of the non-permanent gases.

Charcoal put into a tube similar to Faraday's Λ-shaped tube is saturated with chlorine. The two extremities of this siphon tube being then sealed at the lamp, if the long branch of the tube be heated in a water-bath of boiling water, and the short branch be dipped into a freezing-mixture, a considerable quantity of chlorine leaves the charcoal and resumes the gaseous state; and under the influence of the pressure developed, this gas liquefies in the cooled short branch.

I have in this way obtained several cubic centimetres of pure liquid chlorine. On taking the tube out of the bath, the liquid chlorine commences spontaneous ebullition, and again condenses on the charcoal, while the short branch becomes covered with a frost.

This succession of phenomena can be reproduced, so to say, indefinitely; and the experiments, easy to perform at public lectures, permit the audience to observe the various phases.

Although I can only consider my experiments a trial, I have extended them to the liquefaction of several gases absorbed by the charcoal when cold and disengaged by a temperature not rising above 100° C.:—chlorine, ammonia, sulphurous, hydrosulphuric, and hydrobromic acids, chloride of ethyle, and cyanogen. The liquefaction of each of these gases can be demonstrated in lectures when explaining the history of those bodies.

Reflecting on the feeble thermic effects ascertained by Pouillet when pulverulent mineral matters are soaked with water, oil, alcohol, or acetic ether, and on the somewhat greater effects exhibited when the same liquids are absorbed by organized substances, I asked myself if we could not succeed in ascertaining pronounced thermic effects by placing in contact with cellules of charcoal liquids which do not act upon it—water, alcohol, ordinary ether, sulphide of carbon, and bromine.

The experiments exceeded my expectation. For example, with 1 part of charcoal and from 7 to 9 parts of liquid bromine, the rise of temperature exceeds 30° C., operating on only from 5 to 10 grammes of charcoal.

With charcoal well freed from gas, heated and cooled *in vacuo*, the heating due to the imbibition of bromine would doubtless be much more considerable.

The volatile liquids condensed in the pores of the charcoal (bromine, cyanhydric acid, sulphide of carbon, ordinary ether, and alcohol) are not expelled, or only partially, by a temperature of 100° C. at the ordinary pressure. I made the experiment with a Faraday tube, operating as described for the liquefaction of the gases. A tube filled with charcoal saturated with alcohol does not permit any to distil at 100°.

[The tubes were exhibited to the Academy; and with them the principal experiments (the liquefaction of chlorine, cyanogen, &c.) have been repeated in the laboratory of the École Centrale.

The condensation of liquid bromine by charcoal, effected upon a few grammes, gave rise to a brisk rise of temperature, the mixture passing in a few minutes from 20° to 45°.]—*Comptes Rendus de l'Académie des Sciences*, October 6, 1873.

NOTE ON THE POSSIBLE EXISTENCE OF A LUNAR ATMOSPHERE.

BY E. NEISON, ESQ.

Owing to the many difficulties with regard to the constitution of the lunar surface involved in an assumption as to the absolute non-existence of a lunar atmosphere, it would appear of far greater probability that some such atmosphere, however limited, exists. Not only, as Dr. De La Rue has remarked, is it difficult to conceive any chemical formation of matter without an atmosphere, but it is also difficult to even find matter exhibiting the features and properties of that constituting the lunar surface, which under the known conditions would not either yield an atmosphere, or require for formation the presence of substances that would.

The absolute absence of any atmosphere has never yet been demonstrated, but only the fact that it does not exceed certain limits, generally supposed much more restricted than is actually the case. In consequence it is usually granted that some atmosphere might exist; it is also assumed that it must be of most extreme tenuity; and the subject is dismissed as a matter of indifference, without inquiring whither the admission might carry us, so far as relates to this atmosphere's power of fulfilling the same purposes as our own terrestrial one.

But it would be of interest to ascertain how far this possible lunar atmosphere might not effect for the lunar surface those changes &c. that our own does for the terrestrial surface, and whether in fact it might not amply suffice for maintenance of at least some form of vegetable life. For the present, however, this must be deferred.

The only point restricting the extent of a lunar atmosphere of the nature supposed appears to be its refractive power, more especially as shown by the occultation of stars by the moon. Irrespective of the circumstance that these do not invariably answer conclusively in the negative, it does not appear to be generally recognized that we may have an atmosphere whose maximum power of refraction would not be equal to one second of arc, and yet be of very considerable amount. For, of however great tenuity in comparison with our dense terrestrial atmosphere, it would be in reality present in large quantity—to be estimated in fact, with regard to each square mile of surface, by very many thousands of tons.

There can be but little doubt but such an atmosphere would exert a very considerable influence on the lunar surface, render possible the existence of many substances that appear to constitute a great portion of that surface, and explain many selenographical observations of great interest that at present appear to point to some such solution, and thus support the hypothesis of the existence of a definite lunar atmosphere.—*Monthly Notices of the Royal Astronomical Society*, June 1873.

A CONTRIBUTION TO THE HISTORY OF THE HORIZONTAL PENDULUM. BY PROF. ŠAFAŘÍK.

As it is for the most part only some time after date that I get a sight of scientific journals that do not treat of my special department (chemistry), I first became acquainted recently, through the

Philosophical Magazine, with M. Zöllner's great memoir "On the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies." In § 24 of this (like every thing that comes from M. Zöllner) ingenious and thoughtful treatise, there is a detailed account of a new instrument, which in November 1869 had been brought by M. Zöllner to the notice of the Saxon Royal Society of Sciences, in a memoir "On a new Method for measuring Attracting and Repelling Forces"—which had remained unknown to me. He proposes to name the new instrument "horizontal pendulum," mentioning also that, in 1863, M. Perrot had proposed for the same purposes, and described in the *Comptes Rendus* of the Paris Academy, an instrument resting on the same principles.

It is of high interest, and will certainly interest M. Zöllner to learn, that, more than a generation since, in Germany his instrument was not only described and figured, but also applied to experiments, although no particulars of the results are given.

Singularly, the matter goes back to a man whose name has no good sound in connexion with the exact sciences, and from whom it certainly would be least expected—Gruithuisen of Munich. I feel so much the more obligation to render him this late justice, as seven years since, in a longer memoir published in the *Böhmische Museumzeitschrift*, vol. xxxix., "On the present State of Lunar Investigations," I took the fanciful philosopher of Munich somewhat sharply to task—in fact, designated him as selenoprotophantast. I feel now myself, in somewhat riper years, that this asperity towards one far advanced in age, and so much my senior, who, with all his extravagancies, always proceeded in good faith, was perhaps not in place; and I hereby gladly retract, not the substance of my words, but the too great acrimony of their form.

The very first Part of Gruithuisen's *Analekten für Erd- und Himmelskunde* (Munich, 1828, 80 pages 8vo) opens with a truly original and remarkable paper of 45 pages, by the editor, "On the Proposal *to dig a hole through the earth*; whether the condition of the air at great depths can be ascertained in any other way, by the excavation of a tunnel transversely through a mountain-range or an arm of the sea, by the *catachthonic observatory*, its mathematical and optical instruments, and also by the elkysmometer." (The words in italics are so in the original.)

At a time when (of course mostly through the fault of Gruithuisen himself and those like him) all so-called physical investigation of the heavens had fallen into such discredit that, in consequence of a natural reaction, professional astronomers rigorously insisted on acknowledging as the substance of Astronomy merely places and times, therefore merely motions and their variations—that (to mention only one instance) even the discovery of the dark ring of Saturn by Galle at Berlin (in the presence of Mädler!) and Vico at Rome, in 1838, could be suppressed or at least remain unnoticed (incredible but true)—at such a time Gruithuisen's memoir could not but seem preposterous; at the present time, when celestial physics are developing so promisingly, much therein contained will be read only with surprise. I can here intimate only that Gruithuisen proposes, partly, to sink perpendicular shafts of several thousand feet depth, and partly to drive horizontal galleries in the direction of a chord (under the Alps! to a length of 15 miles) through the ground, and, together with practical locomotive purposes, to apply them to physical-astronomical investigations. *L. c.* p. 21, we read, "But it is moreover *incalculable what the astronomer in such a tunnel, provided with a shaft as dry as possible, for observations deserving attention, could accomplish.* Common people would set up a terrible roar of laughter, if told that, under the mountains, at such depths an excellent observatory could be built, in which to make observations of a peculiar sort, which would furnish us with data in the highest degree desirable and to be expected, and a quantity of useful data at present unknown, which must supply to both practical and theoretical astronomy new aids for still greater geometrical accuracy and a great number of new results. To this subterranean observatory I will give the name of *catachthonic observatory* or *catachthonium*." Now I believe that at the time when the above was printed others besides merely common people would have set up a terrible roar of laughter, if they had seen Gruithuisen's dissertation.

The chief instruments of the catachthonium were, according to Gruithuisen, of two kinds:—first, large, accurately turned rings at the mouths of the shafts, in order on them as on ring micrometers, from a distance of from 100 to 2000 feet, to observe the transits of the stars by day also (in consequence of the presumed visibility of the stars from deep pits), and (p. 22) "to obtain *immediately clear geocentric observations perfectly free from refraction.*" On this it says, at p. 28, "What even these few instruments could accomplish and settle in relation to *the proper motions of many fixed stars, the solstice, precession, nutation, aberration, the course of the moon, and the like*, may be expected to be so much the more, *as the position of the most simple instruments must be the most invariable possible, because here the variations of the temperature are almost* $=0$; *so that the necessary clocks do not require even a compensation-pendulum*; and, besides, there is absolutely nothing present which could be subject to so much as a slight variation of temperature; *wherefore the place of such an observatory cannot be supplied by any observatory above ground, however efficient.*" Here we have Lamont and Carrington's subterranean observatories anticipated a generation previously.

The second principal instrument of the catachthonium was to consist of fine plummets suspended on wires from 150 to 1500 feet in length, in order thereon (according to p. 32) to make "observable *the motion of the earth in its orbit*," "and perhaps even the difference in the annual velocity of this motion." Namely, at pp. 30 and 31 it is shown that, during each rotation of the earth, the rotational velocity of a point in the equator is once added to the orbital velocity, and once subtracted from it, and thereby are produced variations in the horizontal component of the earth's gravitation, which depend on the double difference of the two velocities. With a plumbline of only 10 feet length, Gruithuisen made some observations which (*l. c.* p. 33) had been related in his work entitled "Lieblingsobjecte auf dem Felde der Naturforschung" (Munich, 1817), pp. 69 *et seq.*, 26, 77, 128. I have not been able to procure a sight of this work. In the *Analekten*, *l. c.* pp. 33, 34, it says of these observations:—"In my very first experiments it proved that this instrument (which I named *elkysmometer*) exhibits effects *which depend not on accidental causes, but on the actions of the gravity and motion of the earth and the increasing nearness of other large heavenly bodies*, which latter would announce themselves through the *plummets* so positively and distinctly, even if we had no ebb and flood. The eastern deviation of the elkysmometer-thread was most striking from 8 to 9 o'clock in the morning . . . . . It was also unmistakable that the moon exerted an attraction on the elkysmometer, especially in the morning, when she was just between the sun and the earth." Also the elkysmometer indicated to him "earthquakes, even from other parts of the world" (*l. c.* p. 34); and on p. 37 the superiority of long plumblines to short ones is demonstrated in detail; indeed a table is given at the end for the reduction of very small sines to arcs for the sake of observations on very long elkysmometers.

With the rough arrangements described, there is no doubt that Gruithuisen's results depended on accidental external disturbances, and perhaps in part on illusion, as a short calculation is sufficient to show that, here, plummets would scarcely ever lead to any result. Only the name proposed by him (ἔλκυσμα = traction, ἕλκω = I draw), though it is not correctly formed and should have been "helkometer," deserves to be adopted.

But the most remarkable thing comes last. In his *Neuen Analekten für Erd- und Himmelskunde*, Band i. Heft 1, published at Munich in 1832 (finished, according to p. 72, "am 27 Juli 1832"), pp. 39 & 40, there is a memoir entitled, "Ritter Bessel's Experiments on the Force with which the Earth attracts bodies of various natures, and of the Editor's Elkysmometer and Hengeller's Swing Balance."

After a report, occupying only 19 lines, on Bessel's pendulum-experiments with gold, silver, lead, iron, zinc, brass, marble, clay, quartz, water, meteoric iron, and meteoric stone, all of which gave, to less than $\frac{1}{60000}$, the same length for the simple seconds-pendulum, it says verbatim*:—

"By these experiments one of my most anxious wishes is fulfilled. Already twenty years since (in 1812), I suspended on wires of several fathoms length bodies of various natures, in order to try whether the opposite positions of the moon towards them would effect any deviation from the vertical line. But as the place was not faultless for such experiments, I held that the results of the experiments were not worth publishing. A pendulum of such a length I called an elkysmometer when a scale to be read with a telescope was attached to the wire beneath. I thought to have perceived thereon the daily alternation of forward and backward motion of the earth's surface in relation to the earth's motion in its orbit, and so also with all certainty very distant earthquakes, &c. I wish that some one may find an opportunity to make these observations in a pit, with due accuracy. Reichenbach has proposed for it a very large level [compare A. Wagner's observations of earthquakes in South Europe on the level of the large transit-instrument at Pulkowa]; and I believe that the swing balance made by one of my pupils (of the name of Engeller), adapted on a large scale might, as a preliminary arrangement do excellent service:—

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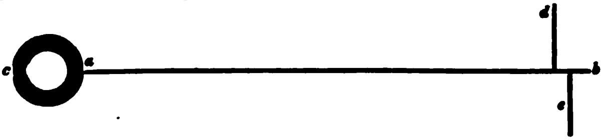
* The figure is a faithful fac-simile of the original.



"It consists of a horizontal lever *a b* of brass, on which is fixed at one end a brass ball *c* as a weight; *d* is a fine wire, by which the lever is suspended; instead of the counterpoise, the other arm of the lever is fastened to the floor by the wire *e*; and the instrument becomes the more delicate the nearer the wire *d* comes to the wire *e*. The ball *c* can oscillate only horizontally, and is visibly (according to Hengeller's experiments) attracted by a cannon-ball. It would be very meritorious to institute observations on this instrument.—G."

This is therefore Zöllner's horizontal pendulum complete, scale-reading by telescope and all, though perhaps not with reading-off by mirror; and after the above statements there is certainly no doubt that M. Zöllner's bold idea, of demonstrating the variations of the gravity of the earth and of cosmic attractions by terrestrial observations at one and the same place, had already in 1817, therefore full 52 years previously, been expressed and proved experimentally by Gruithuisen in Munich—and further that the horizontal pendulum, proposed for this purpose by Zöllner, had been constructed and put to the test of experiment, at the latest, in 1832, thus at least 37 years before Zöllner, by a Munich student, a pupil of Gruithuisen, of the name of Hengeller, although unfortunately nothing further about the observations is communicated than that they proved the usefulness of the instrument for the end proposed. Thus, then, this important thought, like so many similar, did not come to light at once and complete, but emerged in a less perfect form a long time previously in isolated original minds, and, because the time was not ripe for it, passed away unnoticed. Hengeller must have been a genius; and since he studied between 1828 and 1832 at Munich, where Gruithuisen was Professor, it might perhaps still be possible to learn something more about his personality. —*Sitzung der math.-naturw. Classe der k. böhmischen Gesellsch. d. Wissenschaften*, November 15, 1872.

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LIV. *On Diffraction-Spectrum Photography.* *By* HENRY DRAPER, *M.D., Professor of Analytical Chemistry and Physiology in the University of New York*[*].

[Illustrated by a Photograph (PLATE IV.) printed by the Alberttype process.]

THERE are, as is well known, two methods by which spectra may be obtained:—1st, by the action of a prism; 2nd, by a system of closely ruled lines. In the latter case it is convenient to speak of the contrivance employed as a grating, and of the spectrum as an interference- or diffraction-spectrum. A casual inspection shows that there is a great difference between the spectra produced by these two methods; and close investigation proves that the diffraction-spectrum is by far the more suitable for accurate scientific work. For this reason it has seemed desirable to make a trustworthy map of those parts of the diffraction-spectrum which can be photographed on collodion, and to attach to it a scale for reading the wave-lengths of the rays.

The Plate (Pl. IV.) accompanying this memoir is from collodion photographs made by myself, transferred to a thick plate of glass, the latter process being known as the Alberttype. For the entire success of this transfer I am indebted to my friend Mr. E. Bierstadt, the owner of the patent in America. The glass is then used in a printing-press in the same manner as a lithographic stone. The picture is absolutely unretouched. It represents, therefore, the work of the sun itself, and is not a drawing either made or corrected by hand.

It consists of two portions—first, the upper, which gives all the lines of the spectrum from near G to O, or from wave-length 4350 ten-millionths of a millimetre to 3440. Above that is placed a scale which is a copy of Ångström's, from just below G to H_2, with the same-sized divisions carried out from H_2 to O. The second, or lower, is a magnified portion of the same negative, having H_1 and H_2 about its middle, and extending from wave-length 4205 to 3736.

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[*] Communicated by the Author, to whom we are also much indebted for the impressions of the beautiful photograph.

It follows, therefore, that the lines in the solar spectrum are correctly represented in their relative positions. The only errors are those which may have arisen from mal-adjustment of the scale. The precautions that were taken to avoid such errors will be described. With a certain correction, to be mentioned hereafter, it may also be stated that the relative shadings and intensities are preserved.

The value of such a map depends on the fact that it not only represents parts of the spectrum which are with difficulty perceived by the eye (though they may be seen by the methods of Stokes and Sekulic), but also that even in the visible parts there is obtained a far more correct delineation in those portions which can be photographed. In the finest maps drawn by hand, such as those in the celebrated "Spectre Normal du Soleil" of Angström, the relative intensity and shading of the lines can be but partially represented by the artist, and a most laborious and painstaking series of observations and calculations on the part of the physicist is necessary to secure approximately correct positions of the multitudes of Fraunhofer lines. Between wave-lengths 3925 and 4205 Angström shows 118 lines, while my original negative has at least 293.

For such reasons many attempts have been made to procure good photographs of the diffraction-spectrum. The earliest were by my father, J.W. Draper; his results were printed in 1843–44 in a work entitled "On the Forces which produce the Organization of Plants." This memoir was accompanied by plates drawn from his daguerreotypes; and the wave-lengths, which he first suggested as the proper indices for designating the Fraunhofer lines, were used as a scale.

Since that time the most important experiments in this direction have been by Mascart and Cornu. These eminent physicists, however, have resorted to the plan of taking portions of the spectrum on a small scale, and subsequently making enlarged drawings therefrom. This course introduces the defects of hand work, and the artistic difficulties of copying intensity and shading, as well as the omission of fine lines.

In the photographs of the spectrum which I have taken, I have tried to get as large a portion as I could at once, and on as large a scale as possible. I have usually obtained images from below G (wave-length 4307) to above O (wave-length 3440), of about 12 inches (·305 metre long). I have succeeded, however, in photographing from near *b* (wave-length 5167) to T (wave-length 3032), by resorting to a ruled speculum plane and a concave speculum mirror; but the photographic and optical difficulties in securing an enlarged spectrum of that length are great*.

Of course in such a research as this an essential is a finely and evenly ruled plane of glass or other material. Those which I have used were made by a machine devised and constructed by Mr. L. M. Rutherford, whose beautiful lunar and prismatic spectrum-photographs are so well known to the scientific world. The plate generally employed is of glass ruled with 6481 lines to the inch; the ruled part is $1\frac{8}{100}$ inch (·027 metre) long and $\frac{64}{100}$ (·016 metre) wide. It is unquestionably much more nearly perfect than similar gratings made by Nobert and others; for the character of the photographs and the uniformity of the orders on either side of the normal, together with its behaviour under a searching examination, show that it leaves little to be desired. As it is on glass and gives a bright transmitted spectrum, I have constructed the remainder of the optical apparatus of glass achromatized according to the plan used by J. W. Draper in 1843, except that I have not silvered the ruling, and therefore have used the refracted and not the reflected beam. The slit is $\frac{8}{10}$ of an inch (·02 metre) long and $\frac{1}{110}$ of an inch (·00023 metre) wide; the jaws are of steel; and there is not only a micrometer-screw for separating them, but also one for setting them at an angle. Occasionally I have taken photographs with the jaws $\frac{1}{90}$ of an inch (·00028 metre) apart at the top, and $\frac{1}{130}$ (·00019 metre) at the bottom, so as to obtain different intensity in the two edges of the spectrum.

Most of the photographs have been of the spectrum of the third order, which has certain conspicuous advantages. In the first place it is dilated to such an extent as to give a long image and yet one not too faint to be copied by a reasonable exposure of the sensitive plate; and in the second place the spectrum of the second order overlaps it in such a way that D falls nearly upon H, and *b* upon O. These coincidences are serviceable in determining the true wave-lengths of all the rays.

The only point of special interest in connexion with the pho-

* Since writing the above I have succeeded in photographing the lines of the visible spectrum from *b* downward; and the picture comprises not only the regions including E, D, C, B, *a*, and A, but also the ultra-red rays. The great groups α, β, γ below A, discovered by my father in 1843, are distinctly reproduced.

tographic part of the operation is the device for avoiding the unequal action on the sensitive plate of different rays of the spectrum. It has been commonly supposed until the recent memoirs of J. W. Draper, that there are in the spectrum three different types of force in three different but overlapping regions. Heat was supposed to be principally found at the less refrangible end, light in the middle, and actinism at the more refrangible. But he showed that this error has partly arisen from using prismatic spectra, which condense the red end and dilate the violet, and do not present the rays in the true order of their wavelengths, and partly from the nature of our ordinary photographic substances. He proved that actinism, or the power of chemical decomposition, does not particularly belong to the violet end of the spectrum, but is found throughout its whole length. But bromide and iodide of silver as used in collodion photography are more easily decomposed by vibrations of certain lengths and periods than by others; and hence the excess of action seen at the violet end is a function of certain silver compounds, and not of the spectrum. Other substances, as carbonic acid, show maxima elsewhere, as in the yellow region. The solar beam is therefore not compounded of three forces, light, heat, and actinism, but it is a series of æthereal vibrations which give rise to one or other of these manifestations of force, depending on the surface upon which it falls.

In order to provide against this excees of action in certain parts of the spectrum, I introduced a system of diaphragms placed in the vicinity of the sensitive plate and removed at suitable times during the exposure. The region from wave-length 4000 to 4350 only requires about one tenth of the time demanded by that from 3440 to 3510. In the negative which produced the accompanying Plate the line O had 15 minutes, and G 2½ minutes; and the former is underexposed. These exposures seem at first sight unusually long for a wet collodion surface; but it must be remembered that the slit used was only $\frac{1}{110}$ of an inch wide, and that the diffraction-grating gives an almost complete circle of spectra round itself, amongst which this thin band of light is divided. A beam $\frac{1}{110}$ of an inch (·00023 metre) wide is spread out in this case into a streak about 78 feet (23·77 metres) long.

After the production of spectra that were in focus from end to end, it was next necessary to attach a scale to them by which the wave-lengths might be read. At first I tried by reducing Ångström's maps to the proper dimensions to accomplish this object; but the undertaking proved to be difficult, and was unsuccessful, because, though the original drawing on the stone was undoubtedly correct, the paper proof of it, which I had, had stretched unequally in printing; and on applying a photographic reduction to my spectra, coincidence could not be obtained. As, however, the subject of providing a scale for these diffraction-spectra is of prime importance in giving value and precision to the wave-lengths presented in this memoir, I propose to describe fully the method eventually employed in fitting a scale to the photograph.

The wave-lengths of the ultra-violet rays have never, so far as I know, been either determined or published except by J. W. Draper in 1844, Mascart in 1866, and Cornu in 1872. J. W. Draper's memoir has a steel engraving of some of the principal lines, from which the wave-lengths may be approximately read.

The large plate which accompanies Mascart's long and valuable memoir is of the prismatic spectrum; but he furnishes in addition the following Table of wave-lengths:—

L	3819·0
M	3728·8
N	3580·2
O	3440·1
P	3360·2
Q	3285·6
R	3177·5

The numbers do not entirely coincide in all cases with my photograph, as I will show further on.

The detailed results of M. Cornu have not appeared in any publication that has reached me.

I have used as a basis the numbers given by Ångström for the rays D_2, b_4, and G; and if there should be any small error in his determinations, my scale will require a proportionate correction, which can easily be effected. At first it seemed better to take G and H as fixed points; but the line H is so broad, and has so many component lines, that its position is uncertain, and, moreover, being almost at the limit of visibility in Ångström's apparatus, it was more open to errors of measurement. These reasons led me to take advantage of the fact that the second spectrum overlaps the third, the ray D of the second being near H of the third, and *b* of the second near O of the third. It is obvious that we have thus the means of ascertaining the wave-lengths of three parts—one at each end, and one in the middle of my photograph. As the rays D and *b* cannot impress themselves on collodion by any length of exposure that it is convevenient to give, and as in my method of working the ultra-violet rays could not be seen simultaneously with them, it was necessary to resort to the following device. I placed in front of the sensitive plate and close to it two fine steel points, one of which was carefully adjusted to the position of D_2 of the second order, and the other to b_4 of the second order. When, therefore, after a suitable exposure to the ultra-violet spectrum of the third order the collodion picture was developed, there were two sharply defined images of the steel points superposed on the spectrum. The point which had been coincident with D_2 of the second order was then found to have cast its shadow on H_2 of the third order; and the point at b_4 of the second order had impressed itself near O of the third order.

By a simple calculation it was thus rendered evident that a given ray in the compound line H_2 was of the wave-length 3930·1 ten-millionths of a millimetre, and that another line near O had the wave-length 3444·6. By looking at the photograph the reader will see that 3930 falls upon a fine division in H_2, which is beautifully shown in both the spectrum with the scale and the enlarged proof below. Of course the ray G of the third order, the wave-length of which is known, had impressed itself photographically on the collodion.

Having thus ascertained the wave-lengths of three fixed points in the photograph, the next step was to apply a scale reading to a single ten-millionth of a millimetre, and, if possible, fractions thereof. After many abortive attempts to use that part of Ångström's map which lies between G and H, and to attach thereto an additional length of scale sufficient to extend to the end of the ultra-violet region, I was compelled to resort to a linear dividing-engine, and rule a scale which was about twice the length of the photographic reduction shown in the accompanying Plate. Of course this necessitated drawing in by hand the same systems of lines and lettering as are shown on Ångström's chart; and this I did as carefully and faithfully as I could.

It only remained to reduce this divided scale to the proper size to fit the spectrum-photograph; after many trials it was accomplished.

It is proper in this place to make a criticism on my scale and to point out a small error, which may be due, however, to an incorrect determination of the wave-lengths that I have used as fixed points. Taking the distance from G (wave-length 4307) in the photograph to the fixed line 3930 in H_2 and dividing it into 377 parts, and then prolonging these divisions towards O, it was found that the third fixed point was not attained, but that there was an error of about two divisions. But if the position of D_2 in Angström's determinations be incorrect to the extent of one ten-millionth of a millimetre, or if this small error should be partly attributed to D_2 and partly to G, my scale will be correct. Future measures of the wave-lengths of these rays and of b_4 can alone settle this delicate point; for the determinations of Mascart and Ångström and Thalen differ nearly to the extent mentioned above. The same remark is true of Ångström compared with Ditscheiner, while the difference between Ångström and Van der Willigen is more than three times the amount necessary to remove my discrepancy. In any case the photograph is correct, as it is the work of the sun, and is only open to errors arising from imperfect flatness in the field of a fine lens, and that field only subtending an angle of about 4°. The angular aperture of the lens viewed from the sensitive plate is 20′. I trust, therefore, that the photograph may be of permanent value to physicists; for any one can affix another scale if this be erroneous.

An examination of the photographed spectrum shows many points of interest, some of which are best seen in the spectrum with the scale above, and some in the portion enlarged below. The latter is magnified about twice, and comprises the region from wave-length 3736 to 4205. I have also made photographs on the same scale as Ångström's map, but have not as yet printed them. The capital letters which are attached to the region above H are according to the nomenclature of Mascart, although the wave-lengths assigned by him to those letters do not coincide exactly in all cases with the lines in my photograph; for instance, the line L, which he regards as single, is in reality triple, and does not correspond to 3819 but to 3821; M is correctly designated by 3728, but it is double; N is really at 3583 and not at 3580. It has been suggested that it would be proper to return to the old nomenclature of Becquerel and J. W. Draper, who simultaneously discovered these lines in 1842–43; but the designation of position by wave-length in reality renders the letters unnecessary.

The spectrum above H, when compared with the region from G to H, is marked by the presence of bolder groups of lines; and most conspicuous are those between 3820–3860, 3705–3760, 3620–3650, 3568–3590, 3490–3530. The first of these groups is strikingly shown in the enlarged photograph. I am not as yet able to offer an opinion as to the chemical elements producing these groups; for almost all the photographs of the ultraviolet spectra of metalline vapours that I have thus far made were produced by a quartz train, and have not yet been reduced to wave-lengths; indeed that is a separate field of inquiry, and could not be comprised in a memoir of this length. I have also tried to utilize the photographic spectra of the late Professor W. A. Miller, published in the Transactions of the Royal Society for 1862; but, for some reason (probably insufficient intensity of the condensed induction-spark), his pictures do not bring out the peculiarities of the various metals in the striking manner that is both necessary and attainable. The diffraction-spectra of metalline vapours that I have made are not yet ready for use.

The probability is that each of these groups will be found to be due to several elements, as is plainly seen in the group H. This compound line, which is commonly spoken of as being caused by calcium, iron, and aluminium, is in reality much more complex; for there can readily be counted in it more than fifty lines in the original negative; and a careful inspection of the accompanying paper picture shows a large proportion of them. This observation leads us to a more general statement. *The exact composition of even a part of the spectrum of a metal will not be known until we have obtained photographs of it on a large scale.* The coincidences which were so thoroughly examined by Mr. Huggins (Trans. Roy. Soc. Dec. 1863) will only disappear when we can, in addition to the position of a line, have a clear idea of its size, strength, and degree of sharpness or nebulosity. The eye is not able to see all the fine lines; or even if it does, the observer cannot map them with precision, nor in their relative strength and breadth. For example, in Angström's justly celebrated chart, of which the G–H portion is copied in this Plate; and in the construction of which the greatest pains were taken by him, many regions are defective to a certain extent. The region from 4101 to 4118 is without lines; yet the photograph shows in the enlarged copy seventeen that can easily be counted, and the original negative shows more yet. The reader, of course, understands that a paper print of a collodion picture is never as good as the original, the coarseness of grain in the paper, want of contact in transferring, &c. effect such a result. Moreover the Alberttype process depends on a certain fine granulation which is given to the bichromated gelatine; and this forbids the use of a magnifier upon these paper proofs. It is only just, however, to Mr. Bierstadt to state that, without his personal supervision, such sharp and fine-grained proofs could not have been obtained, and that no other printing-press process that I know of could have accomplished this work at all. As an illustration of the difficulty of depicting the relative intensity of lines, we may examine 3998, which in Angström's chart is shown of equal intensity with 4004, while in reality it is much fainter, and instead of being single is triple, as is well shown in the enlarged spectrum.

When, however, we compare Ångström's chart with the photograph, it requires, as the above remarks show, a critical examination to detect defects, and we have a striking confirmation of the surprising accuracy of the Swedish philosopher.

So also in comparing Mascart's excellent map of the prismatic spectrum with the photograph, the difficulty of depicting all the even in the Alberttype copy of my photograph 25 can be counted, and in the original negative many more. From H to L he shows 70 lines; in my Plate 138 can be observed besides many unresolved bands.

In the earlier part of this memoir it was stated that the relative intensities of the lines in the spectrum were correctly represented if a certain allowance was made. If an unshielded collodion plate were presented to the image of the spectrum, there would be produced a stain very dense from G to H, fainter above H, and still fainter below G. But this stain would not represent the actinic force of the sun, it would merely be an index of the decomposability of a mixture of iodide and bromide of silver. I have for this reason adopted the idea of J. W. Draper, that force is equally distributed through the spectrum, and have tried to produce a photograph of equal intensity throughout. This has been accomplished, as I have before stated, by suitable diaphragms. But whether this view be correct or not, lines which are not far distant from one another are presented virtually without any interference by diaphragms, and must therefore be correct both as to shading and intensity.

Besides the points above mentioned, there are many theoretical considerations suggested by the photograph which it does not seem expedient to enter upon fully at present. Among such is the possibility of arriving at an estimate of the sun's temperature by interpreting the apparent bands, such as those near G and H, by the aid of Lockyer's researches on the temperature of dissociation of compounds. No one has yet ascertained whether there are or are not unresolvable bands in the solar spectrum. If they do exist, the compounds to which they belong, and the necessary temperature for dissociation, remain to be determined.

It would seem also to be possible to find out whether, as asserted by Zöllner, there is a liquid envelope round the sun, by a search for more diffused bands in its photographed spectrum.

In the hope that this photograph may prove to be of value to scientific men for further investigations upon the sun and the elements, I have caused a number of extra copies to be printed, and shall be glad to present them to any one who can make use of them.

University, Washington Square,
New York.

LV. *On* Hamilton's *Dynamic Principle in Thermodynamics.*
By C. Szily*.

In a memoir published not long since†, I have maintained that the equation which expresses the second proposition of the mechanical theory of Heat is in complete agreement with the dynamical equation which is named after Hamilton, and that, accordingly, in reply to the question "To which equation in dynamics can the second proposition of thermodynamics be reduced?" we must point directly to Hamilton's equation.

I was moved to the publication of my considerations on this subject chiefly by the circumstance that neither Boltzmann nor Clausius, in their memoirs‡, has once mentioned Hamilton's principle; and they are the only writers who up to that time had occupied themselves with the above question. Both seem to consider that, in order to bring in accordance, it was necessary to deduce an equation yet unknown in dynamics; both remark that the equations deduced by them exhibit a certain affinity with the principle of least action; but neither of these two philosophers has taken any notice of Hamilton's principle and its close relations to the equations in question.

Hereupon M. Clausius published a memoir § which refers exclusively to mine above mentioned; and at the very commencement he therein readily acknowledges that the connexion sought becomes much more striking when one compares the proposition in question, not (as he and Boltzmann had done) with the principle of least action, but with Hamilton's. At the same time Clausius suggests the cause which prevented him from turning his attention to Hamilton's principle when treating upon the above-mentioned connexion. Namely, in many text-books of dynamics (for instance, Jacobi's *Vorlesungen über Dynamik*) Hamilton's equation is cited in a form which differs essentially from the original, in consequence of the peculiar variation. Now, if Jacobi's form only be kept in view instead of the primitive form, it is indeed true that it is not very conceivable how any connexion capable of being clearly indicated can exist between that equation and the second proposition of the mechanical theory of heat.

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* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlix. pp. 74–86.

† "Hamilton's Principle and the Second Proposition of the Mechanical Theory of Heat," Phil. Mag. S. 4. vol. xliii. p. 339. Pogg. *Ann.* vol. cxlv. p. 295.

‡ Boltzmann, "Ueber die mechanische Bedeutung des zweiten Hauptsatzes der Wärmetheorie," *Sitzungsberichte der Wiener Akademie*, vol. liii. p. 210. R. Clausius, "Ueber die Zurückführung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mechanische Principien," Pogg. *Ann.* vol. cxlii. p. 433; Phil. Mag. S. 4. vol. xlii. p. 161.

§ Pogg. *Ann.* vol. cxlvi. p. 585. "On the Connexion of the Second Proposition of the Mechanical Theory of Heat with Hamilton's Principle," Phil. Mag. S. 4. vol. xliv. p. 365.

Clausius then quotes the original form of Hamilton's equation used by me, and proves that Boltzmann's equation (23*a*), rightly interpreted, perfectly corresponds with Hamilton's, is indeed identical with it. But what he concedes in relation to Boltzmann's equation, that he decidedly questions in reference to his own; nay, he declares without hesitation the correspondence impossible. Thus the same question is again raised which has been discussed in the *Annalen*, between Clausius and Boltzmann*, on the occasion of the claiming of priority by the latter, namely:—Does there actually exist a difference, and, if this is the case, what is the difference, between the equation of Clausius and that of Boltzmann? I believe that I am now entitled—nay, after the reply of Clausius, am bound to enter into the discussion of the question, at least in regard to Hamilton's equation.

I will first discuss the equation which Clausius claims as his own.

For the more convenient elucidation of the matter, we will first of all, as Clausius does, confine ourselves to the consideration of the periodic motion of a single point. Given, therefore, a material point which with the original motion describes a closed path, or moves between two given points. The only forces acting on the movable point shall be such as possess a force-function (or, as Clausius is accustomed to express it, an ergal)—that is, forces of which the components can be represented by the partial differential quotients, taken negatively, of a function of the coordinates of the point. Let us now suppose that this motion undergoes an infinitesimal alteration. With the changed motion let the point likewise describe a closed path, or move between two points, which latter may be either identical with those previously given, or, if not so, fulfil the condition that the quantity

$$\frac{dx}{dt}\delta x+\frac{dy}{dt}\delta y+\frac{dz}{dt}\delta z$$

has the same value at the terminal point of the motion as at the initial point. Then let i denote the period of the motion, T the *vis viva*, U the force-function; and let us agree to indicate the mean value of the variables by putting a horizontal stroke over them (for example, $\bar{x}=\frac{1}{i}\int xdt$): then the equation to which Clausius in his answer appeals, and which he marked (18) in his first memoir on this subject*, is the following:—

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* Pogg. *Ann.* vol. cxliii. p. 211: "Zur Priorität der Auffindung der Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und dem Principe der kleinsten Wirkung; von L. Boltzmann." Ibid. vol. cxliv. p. 265: "Bemerkungen zu der Prioritätsreclamation des Hrn. Boltzmann; von. R. Clausius."

$$\overline{\frac{dU}{dx}\cdot\delta x+\frac{dU}{dy}\cdot\delta y+\frac{dU}{dz}\cdot\delta z}=\delta\bar{T}+2\bar{T}\,\delta\log i. \quad . \quad . \qquad (1)$$

Now this is the equation to which Clausius refers as his own, and which, he quite correctly remarks, has a more general applicability than Hamilton's. It is also indeed impossible to find complete accordance between this and Hamilton's equation. I must, however, observe that I have not reckoned *this* equation as Clausius's and designated it as such; neither can I now so designate it; for this equation was known five years since, as it was given and demonstrated in the first volume of Sir William Thomson's work, so widely diffused and frequently cited amongst physicists, the 'Treatise on Natural Philosophy,' Oxford, 1867 (translated into German by G. Wertheim: Brunswick, 1871). If, namely, the equation (9) in the section "Dynamical Laws and Principles"† be written with Clausius's notation, it exhibits the following form:—

$$\delta(2i\bar{T})=i\left[\delta\bar{T}+\overline{\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z}\right]. \quad . \quad . \qquad (2)$$

Now it is clear that this equation is identical with equation (1) above cited.

Moreover, for a second and not less important reason I could not, when speaking of the equation of Clausius, mean the above-mentioned equation of Thomson: namely, Thomson's equation is of such generality that, without a special hypothesis, it cannot be brought into consonance with the second proposition of the mechanical theory of heat. For if we could apply this equation in its entire generality to that motion which we call heat, then (as we shall see further on) the second proposition of the mechanical theory of heat would not be true. We can, however, most simply bring Thomson's equation into harmony with the second proposition of thermodynamics, if we make a term vanish from the dynamic equation, putting it $=0$. And because it was

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* Pogg. *Ann.* vol. cxlii. p. 442; Phil. Mag. S. 4. vol. xlii. p. 167. This is equation (2) of his memoir "Ueber die Anwendung einer von mir aufgestellten mechanischen Gleichung" &c., *Nachr. der kgl. Ges. d. Wiss. zu Göttingen*, 1871, p. 248; *Math. Ann.* *von* Clebsch u. Neumann, vol. iv. p. 232; Phil. Mag. S. 4. vol. xlii. p. 321.

† In order fully to set forth the importance of this equation, Thomson adds:—"This, it may be observed, is a perfectly general kinematical expression, unrestricted by any terminal or kinetic conditions."

Clausius who showed which term must be omitted from that equation, and under what assumption this can be attained, I think that the complete truth of the historical development is attained when we, as I have done, designate as Clausius's the special equation* resulting from the assumption above alluded to, viz.

$$\overline{\delta U}=\overline{\delta T}+2\overline{T}\delta \log i. \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

Now, what is the difference between this and Hamilton's equation?

Before entering further into the clearing-up of this question, let us inquire generally what kind of conditions must be fulfilled in order to bring Thomson's equation into agreement with the second proposition of the mechanical theory of heat. For the sake of a more convenient survey, I will write Thomson's equation in a somewhat different form. If E denote the total energy, therefore the sum of $\overline{T}$ and $\overline{U}$, we can, after a slight transformation, write equation (2) as follows:—

$$\delta E=\frac{\delta(2i\overline{T})}{i}+\overline{\delta U}-\overline{\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z}, \quad . \qquad (4)$$

or, if we restore the definite integral in the last term, from which the mean value denoted by the horizontal stroke has arisen, we obtain

$$\delta E=\frac{\delta(2i\overline{T})}{i}+\frac{1}{i}\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)\right]dt. \qquad (5)$$

If, finally, we divide the entire equation by $\overline{T}$, it becomes

$$\frac{\delta E}{\overline{T}}=\delta 2\log(i\overline{T})+\frac{1}{i\overline{T}}\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\right)\delta z\right]dt.$$

It is plain that this equation, which is still identical with Thomson's, can only be brought into the form of the second proposition when it is possible to put the last term equal to the total variation of any invariable function of the coordinates—that is, when

$$\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)\right]dt=i\overline{T}\,\delta F. \quad . \qquad (6)$$

Supposing this value introduced into the last equation, and the symbols of the differentials substituted for those of the variations, then

$$\frac{dE}{\overline{T}}=d2\log(i\overline{T})+dF.$$

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* This is Clausius's equation (21) in Pogg. *Ann.* vol. cxlii. p. 442; Phil. Mag. S. 4. vol. xlii. p. 170.

If we integrate this equation throughout an entire cyclical process, taking at the same time into consideration that $i\overline{T}$ as well as F (according to the assumption) has at the beginning and at the end of the process the same value, then is

$$\int\frac{dE}{T}=0.$$

Thomson's equation is therefore only then in complete accordance with the second proposition of the theory of heat, when the equation of condition (equation 6) is satisfied.

Let us now see in what manner this condition is fulfilled, on the one hand, in Hamilton's, and, on the other, in Clausius's equation.

Hamilton founded his equation on the assumption* that the form of the force-function does not change with the change of the motion, that hence δU depends solely on the variation of the space-coordinates—that is, that

$$\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)=0. \quad . \quad . \quad (7)$$

In consequence of this,

$$\delta F=0,$$

and equation (5) takes the following form:—

$$\delta E=\frac{\delta(2i\overline{T})}{i}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This (8) is Hamilton's equation.

Now, in what consists the assumption made by Clausius? He admits that even the form of the force-function may change; he only lays down this condition†, that the variation which results from the change of form of the force-function, taken at its mean value, is equal to 0, and therefore

$$\overline{\delta U}-\overline{\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z}=0,$$

that is,

$$\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)\right]dt=0. \quad . \quad (9)$$

Therefore according to this assumption also

$$\delta F=0.$$

Consequently equation (1) takes the form of equation (3), viz.

$$\delta\overline{U}=\delta\overline{T}+2\overline{T}\delta\log i.$$

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* Phil. Trans. 1834, p. 249, *sub* (1).

† Pogg. *Ann.* vol. cxlii. pp. 442–446, *sub* (18)–(22); Phil. Mag. S. 4. vol. xlii. pp. 167–170.

This is Clausius's equation.

Now I affirmed that *this* equation, and not Thomson's, is identical with Hamilton's. This assertion of mine I still maintain in its full extent; and I believe that after the preceding statements Professor Clausius will urge no objection to it.

Still, although Clausius's mathematical contemplations on this subject have not led to an altogether new equation, but have for their result merely Thomson's and Hamilton's equations somewhat transformed, it would be a great mistake to suppose that those highly important disquisitions had remained unfruitful. As Boltzmann on his part, so I also most willingly acknowledge that the portion which chiefly refers to the change of form of the force-function was absolutely necessary—nay, that only with their aid could Hamilton's equation have been, with any result, introduced into the mechanical theory of heat. Hamilton has proved his equation only for the case in which the form of the force-function undergoes no change when the motion is changed; but from the researches of Clausius it follows that *the same* equation (Hamilton's) can be made use of also when the form of the force-function changes, provided that the mean value of the resulting variation be $=0$. Such an extension of the validity of Hamilton's equation was at all events requisite, because in the thermodynamic theory changes of state of bodies come into consideration which are not dependent merely on space-alterations, but also on the changed form of the force-function.

Let us now examine how far Hamilton's equation must be modified to make it applicable to the case in which we have to do not merely with a single point, but with the change of motion of an entire system of points. And indeed let us just take a system the individual points of which do not travel their paths with equal *vires vivæ* or in equal times. Professor Clausius remarks, quite correctly, that in this case Hamilton's equation is not immediately applicable, and consequently special investigations were necessary.

When, however, we consider the matter a little more closely, we find that in the form (8) of Hamilton's equation no other modification is required than that which, relative to the energy, naturally results from the notion of a heterogeneous system of points; that is, we have only to prefix the sign of summation to the right- and left-hand terms, and to extend this summation to all the points of the system. Thus, whilst Hamilton's equation expresses the variation of the energy *for one point* as follows,

$$\delta E = \frac{\delta(2i\overline{T})}{i},$$

we obtain the variation of the energy relative to an *entire system* *of points* in a perfectly analogous form by the equation

$$\Sigma\delta E=\Sigma\frac{\delta(2i\bar{T})}{i}. \qquad (10)$$

But this modification is truly so slight, and follows so naturally from the notion of the system, that we may regard equation (10) as the necessary consequence of Hamilton's equation. As, however, this equation does not occur in Hamilton, and he limited the employment of the summation-symbol to the case of the points all accomplishing their paths in the same time, it will perhaps not be unadvisable to effect a special demonstration, after Hamilton's method, of equation (10). It is moreover to be remarked that this equation is first found in Boltzmann, and that the demonstration here given deviates essentially from his only at the commencement, and afterwards only with respect to the arrangement.

Let m be the mass of any point whatever of a system, x, y, z its rectangular coordinates, v the velocity, T the *vis viva*, U the force-function, E the energy, and i the period of a revolution. T and U have different values at different places in the path; but their sum (that is, E) will remain constant so long as the point describes the same path. Hence this quantity is independent of the time. Let the motion of the point undergo a change: instead of its former closed path and periodic motion, let it follow a new closed path infinitesimally different from the previous one. In this new path the mass of the point alone remains as before; all the rest of the quantities are in general changed. Let the variations springing from the change of path be denoted by δ. It follows, from the conception of the energy, that

$$\delta E=\delta T+\delta U.$$

If we multiply this equation by the time-element and integrate it from 0 to i, and at the same time take into account that both E and δE are independent of the time, then

$$\delta E=\frac{1}{i}\int_0^i(\delta T+\delta U)dt.$$

Let us form analogous expressions for the other points of the system, and sum them:

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i(\delta T+\delta U)dt,$$

which can also be written thus:—

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i\left(\delta T+\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)dt$$
$$+\Sigma\frac{1}{i}\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)\right]dt.$$

Supposing now that the last term on the right-hand side (that is, the sum of the mean values of the variations resulting from the change of form of the force-function) is $=0$, there remains

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i\left(\delta T+\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)dt.$$

Substituting

$$\delta T=mv\,\delta v$$

and

$$-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)$$

$$=m\left(\frac{d^2x}{dt^2}\delta x+\frac{d^2y}{dt^2}\delta y+\frac{d^2z}{dt^2}\delta z\right)=m\frac{dv}{dt}\delta s,$$

where δs signifies the variation in the length of the path, we obtain

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i m\left(v\delta v-\frac{dv}{dt}\delta s\right)\delta t.$$

Now

$$\int_0^i\frac{dv}{dt}\delta s\,.\,dt=v_1\delta s_1-v_0\delta s_0-\int_0^i v\,\frac{d(\delta s)}{dt}\,dt,$$

where the indices 0 and 1 in the integrated portions denote the initial and final values of the quantities. But as the path is closed and the motion periodic,

$$\delta s_1=\delta s_0 \text{ and } v_1=v_0.$$

Accordingly

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i m\left(v\delta v+v\,\frac{d(\delta s)}{dt}\right)dt.$$

Taking into consideration that

$$vdt=ds,$$

by a slight simplification we get

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i m\delta(v^2dt).$$

If we bring the constant mass under the variation-symbol and reverse the succession of the integration and variation, we have

$$\Sigma\delta E=\Sigma\frac{1}{i}\,\delta\int_0^i 2T dt,$$

or, designating the mean value by a horizontal stroke,

$$\Sigma\delta E=\Sigma\,\frac{\delta(2i\overline{T})}{i}.$$

Now this is the equation which we wished to demonstrate.

I will remark finally that, when the paths of the points are not closed, the transition from closed paths to those not closed can be effected in precisely the same manner, whether we write the Hamilton-Boltzmann equation in its primitive form or in the logarithmic form as modified by Clausius. In truth the form of the equation does not occasion, nor does it prevent, in this case the difficulties mentioned by Professor Clausius at the end of his memoir.

For an easier view of the question, let us compare the dates of the development of that dynamic equation to which the second proposition of thermodynamics is reducible. Hamilton set up the equation in the year 1834, but presupposed in the demonstration that the system of points is homogeneous, and the form of the force-function invariable. In 1868 Boltzmann applies this equation to a heterogeneous system, and, for the first time, brings it into the well-known form of the second proposition of the mechanical theory of heat. In 1871 Clausius proves that the same equation is also valid when the form of the force-function changes, but the mean value of the resulting variation $=0$. Consequently, within these thirty-eight years, only the validity, the applicability, of the equation has been extended, without the equation itself undergoing any essential alteration, even in form. I repeat the concluding words of my previous memoir:—"What in thermodynamics we call the second proposition, is in dynamics no other than Hamilton's principle, the identical principle which has already found manifold applications in several branches of mathematical physics."

LVI. *On the Fundamental Modes of a Vibrating System.*

By Lord Rayleigh, *M.A., F.R.S.*[*]

THE motion of a conservative system about a configuration of stable equilibrium may be analyzed into a series of normal component vibrations, each of which is entirely independent of the others. When one of the components exists alone, the motion is simple harmonic, the period of the vibration and the phase at any given moment of time being the same for all parts of the system. In such a case the system is said to vibrate in a fundamental mode. In order to represent the most general kind of motion, the whole series of normal components must be imagined to exist together, the amplitude and phase of each being arbitrary.

The number of the normal components, which also expresses the degree of freedom enjoyed by the system, may be either finite or infinite, though, strictly speaking, every natural system belongs to the latter class. It is only by the introduction of limitations, such as attributing to various parts absolute rigidity, that the position of the whole is reduced to dependence on a finite number of coordinates. But even after every permissible simplification has been made, there remains a large and important class of systems whose configuration cannot be defined without the specification of an infinite number of coordinates, or, which comes to the same, of a function of one or more independent variables. Under this head must be included all strings, bars, membranes, plates, &c. which are treated as capable of continuous deformation.

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[*] Communicated by the Author.

To each fundamental mode corresponds what may be called a normal function. To determine these in the case of any particular system is a problem which may tax, and will usually overtax, all the power of analytical expression which the mathematician possesses; but whether expressed in terms of simple functions or not, the normal functions must be thoroughly discussed, not to say tabulated, before the solution of the problem can be considered complete.

The normal functions appear analytically as the solution of a differential equation containing a constant at this stage undetermined; and the first step is the formation of this characteristic equation. The usual method proceeds by the consideration of the forces actually acting on an element in virtue of its connexion with the rest of the system. For example, the element of a flexible string is acted on by the tensions at its two extremities; and the equation of motion expresses the fact that the actual acceleration of the element is proportional to the resultant of the tensions when resolved in the transverse direction. The characteristic equation is obtained on the introduction of the assumption that the whole motion is simple harmonic.

The second method, which was (I believe) first employed by Green, depends on the use of what we now call the potential energy; and my present object is to point out its advantages. For this purpose I will take, as neither too easy nor too difficult, the problem of the transverse vibrations of a thin uniform rod whose natural condition is straight.

The potential energy V is for each element of length proportional to the square of the curvature; and thus, if y denote the transverse displacement of the element whose distance from one end of the rod is x, we have

$$V=\tfrac{1}{2}B\int\left(\frac{d^2y}{dx^2}\right)^2 dx, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the integration must extend over the length of the rod, from 0 to l.

If ρ be the longitudinal density, we have by the Principle of Virtual Velocities as the variational equation of motion,

$$\delta V+\int \rho \ddot{y}\delta y=0\,; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where δy refers to a hypothetical variation of y, which is subject only to the condition of not violating the connexion of the system. In the formation of (2) we have, as is usual, neglected the reaction of the elements of the bar against the acceleration of rotation.

In order to deduce the ordinary form of the differential equation and the terminal conditions, we require to transform the expression δV. By the usual method we find

$$\delta V=B\int_0^l \frac{d^2y}{dx^2}\,\frac{d^2\delta y}{dx^2}\,dx$$

$$=B\left\{\frac{d^2y}{dx^2}\,\frac{d\delta y}{dx}\right\}-B\left\{\frac{d^3y}{dx^3}\,\delta y\right\}+B\int_0^l \frac{d^4y}{dx^4}\,\delta y\,dx\,;$$

and therefore, as in the Calculus of Variations,

$$B\frac{d^4y}{dx^4}+\rho\ddot{y}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The integrated terms give us the conditions to be satisfied at the extremities, namely

$$\frac{d^2y}{dx^2}\,\delta\frac{dy}{dx}=0, \quad \frac{d^3y}{dx^3}\,\delta y=0. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

There are three cases to be considered. At a clamped end δy and $\delta\frac{dy}{dx}$ vanish, and the equations (4) are satisfied without any further supposition. At a free end δy and $\delta\frac{dy}{dx}$ are arbitrary, and hence

$$\frac{d^2y}{dx^2}=0, \quad \frac{d^3y}{dx^3}=0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

are the conditions to be satisfied in such a case. The third case occurs when the end is constrained to be a node, but the direction of the rod is left free. Since $\delta\frac{dy}{dx}$ is arbitrary, the conditions are

$$y=0, \quad \frac{d^2y}{dx^2}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The general equation (3) and the terminal conditions are the same as would be found without the use of the potential energy by the ordinary method.

In order to determine the normal functions, we assume that $y=u\cos pt$, where u is a function of x. We thus obtain

$$B\frac{d^4u}{dx^4}-\rho p^2u=0\,; \quad . \quad . \quad . \quad . \quad . \quad (7)$$

from which u is found as the sum of four distinct functions of x (which it is not necessary to write down), each multiplied by a coefficient, which is up to this point arbitrary. The four equations expressing the terminal conditions determine the *ratios* of the four coefficients, and cannot be harmonized without ascribing special value to p. The condition expressing the compatibility of the four equations limits p to certain definite values which appear as the roots of a transcendental equation. Corresponding to each admissible value of p there is thus a normal function in which every thing is definite except a constant multiplier.

Since the most general motion may be compounded of the normal component vibrations, it follows that the most general value of y may be expressed in the series

$$y=\phi_1u_1+\phi_2u_2+\ldots\,; \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where u_1, u_2, &c. are the normal functions, and ϕ_1, ϕ_2, &c. arbitrary coefficients. The determination of ϕ_1 &c. is effected by means of the characteristic property of the normal functions, that the product of any two of them vanishes when integrated over the length of the rod. From this it follows that

$$\int_0^l yu_rdx=\phi_r\int_0^l u_r^2dx. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The fact that the normal functions are *conjugate* (which is the name given to functions possessing the property above stated) may be proved from the form of the functions, or better from the differential equation and terminal conditions which define them. The last course is that which has been followed by the distinguished mathematicians who have treated of the present and similar questions.

If u and v be two normal functions corresponding to p and p', we have from (7),

$$\frac{\rho}{B}\left(p'^2-p^2\right)\int_0^l uvdx=\int_0^l\left(u\frac{d^4v}{dx^4}-v\frac{d^4u}{dx^4}\right)dx$$

$$=u\frac{d^3v}{dx^3}-v\frac{d^3u}{dx^3}+\frac{dv}{dx}\frac{d^2u}{dx^2}-\frac{du}{dx}\frac{d^2v}{dx^2}, \quad . \quad . \quad . \quad (10)$$

as may be found by integrating by parts. The integrated terms are to be taken between the limits, and vanish in each case, whether the end be clamped, free, or supported. Hence

$$(p'^2-p^2)\int_0^l uvdx=0, \quad . \quad . \quad . \quad . \quad (11)$$

from which we infer that any two normal functions corresponding to different periods are conjugate.

This process may perhaps seem as simple as could be expected; but a little consideration will show that in the derivation of (11) we have in fact retraced the steps by which the ordinary differential equation was itself proved, *and that the true foundation of* (11) *is the variational equation from which we originally started.*

In the equation referred to, namely

$$\delta V+\int\rho\ddot{y}\delta ydx=0,$$

δV is a symmetrical function of y and δy, as may be seen either from its form in the present problem, or by the general theorem proved below. Suppose that y refers to the motion corresponding to a normal function u, so that $\ddot{y}+p^2y=0$, while δy is proportional to another normal function v; then

$$\delta V=p^2\int_0^l \rho uvdx.$$

Again, if we suppose, as we are equally entitled to do, that y varies as v and δy as u,

$$\delta V=p'^2\int_0^l \rho uvdx;$$

and thus, as before,

$$(p'^2-p^2)\int_0^l \rho uvdx=0.$$

If p and p' are different,

$$\delta V=\int_0^l uvdx=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The symmetrical character of δV is a simple consequence of the fact that V is a homogeneous quadratic function of the coordinates. If we suppose that

$$V=\tfrac{1}{2}\{11\}\psi_1^2+\ldots+\{12\}\psi_1\psi_2+\ldots,$$

and let ψ_1 become $\psi_1+\Delta\psi_1$ &c., we find

$$\Delta V=\{11\}\psi_1\Delta\psi_1+\ldots+\{12\}(\psi_1\Delta\psi_2+\psi_2\Delta\psi_1)+\ldots$$
$$+\tfrac{1}{2}\{11\}(\Delta\psi_1)^2+\ldots+\{12\}\Delta\psi_1\Delta\psi_2+\ldots,$$

or, passing to the limit,

$$\delta V=\{11\}\psi_1\delta\psi_1+\ldots+\{12\}(\psi_1\delta\psi_2+\psi_2\delta\psi_1)+\ldots,$$

which is a *symmetrical* function of ψ_1 &c. and $\delta\psi_1$ &c.

The reader will now perceive that the proof of the conjugate property will be scarcely altered if, instead of the bar, we substitute a membrane or plate, whose thickness or material need not be uniform. Denoting the transverse displacement of any point by w, we have as the variational equation,

$$\delta V + \iint \rho \ddot{w} \delta w \, dx \, dy = 0;$$

from which precisely, as before, we deduce

$$(p'^2 - p^2) \iint uv \, dx \, dy = 0.$$

It is, I hope, now clear that great advantage results from the direct employment of the variational equation. The reason of the advantage appears to lie in the fact that every thing required to be known is here embraced in one equation, while the ordinary[*] differential equation needs to be supplemented by the boundary conditions, which are indeed of the same mechanical importance as itself.

The same method may be applied to the general system. Lagrange's equation of motion for a vibrating system may be written

$$\frac{d}{dt}\left(\frac{dT}{d\dot{\psi}_1}\right) \delta\psi_1 + \ldots + \delta V = 0,$$

where T is the kinetic energy. If the actual motion denoted by ψ_1 &c. be a normal component vibration, each coordinate varies as $\cos pt$, and the general equation may be written

$$-p^2 \delta T_1 + \delta V = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where T_1 is the same function of ψ_1, ψ_2, &c. that T is of $\dot{\psi}_1$ &c. By the same reasoning as before we infer that if ψ_1 &c. refer to one normal component and $\delta\psi_1$ &c. to another having a different period,

$$\delta T_1 = \delta V = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

LVII. *On a new Spectroscope. By* Professor Ch. V. ZENGER[*].

IN the spectroscopes usually employed the maximum of dispersion is attained by the combination of several prisms; but this entails a very great loss of light by absorption and reflection at the numerous surfaces of the prisms. It is obvious that feeble intensity of light, or, even when the light is strong, feeble physiological action of the red and violet rays of the spectrum much interferes with exactness and distinctness of vision, especially in the spectral analysis of starlight.

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[*] *Ordinary* is here used in opposition to *variational*, not to *partial*.

[†] Communicated by the Author.

Any contrivance by which this loss could be avoided and yet the same amount of dispersion be secured would be very advantageous for spectrum-observations on feeble, and even on strong light.

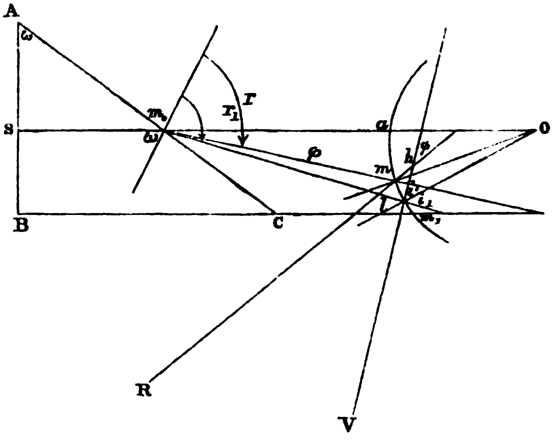
For this purpose I employ only a single prism of the least-absorbent material and the greatest dispersion attainable. It seemed to me that a prism of pure transparent ice-spar would best fulfil this condition, as its index of refraction is:—

$$\begin{aligned} n_b &= 1{\cdot}6531, \\ n_h &= 1{\cdot}6833, \\ \hline \Delta n &= 0{\cdot}0302; \end{aligned}$$

so that its dispersion is greater than that of crown glass, and but little less than that of flint glass, while it absorbs very little of the extreme red and violet rays.

To explain the action of the new spectroscope, let A B C (fig. 1) be the section of a rectangular prism of spar; the solar ray Sm_0 falling at right angles on the surface A B, the angle of incidence ω is equal to the angle of the prism. After refraction the red ray falls in the direction $m_0 m$ on the surface of a cylindrical mirror and is reflected from it in the direction m R; and the violet ray will be reflected, at a greater angle, in the direction m_1 V.

Fig. 1.



The diagram shows at once that the effect of the cylindrical surface $a\,m\,m_1$ is to produce an increase of the angle of dispersion $m\,m_0\,m_1 = \phi$ to R h V $= \psi$. It is easy to account for the increase, if the angle of the refracting prism, the distance between the prism and the mirror, and the radius of curvature of the mirror be given. In the first place, the angle of dispersion produced by the prism itself will be

$$\phi = r_h - r_b,$$

equal to the difference of the angle of refraction for the red and the violet rays:,

$$\sin r_h = n_h \,.\, \sin \omega ;$$

$$\sin r_b = n_b \,.\, \sin \omega .$$

If the angle of the prism is not too large, the sines may be replaced by the angles:

$$r_h = n_h \omega ;$$

$$r_b = n_b \omega ;$$

$$r_h - r_b = (n_h - n_b)\,\omega,$$

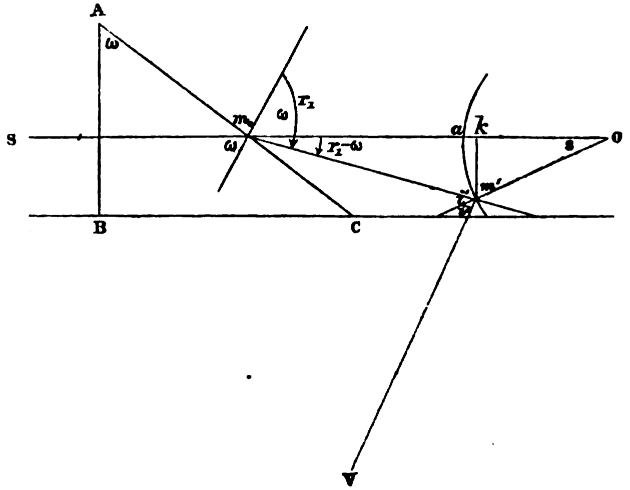
$$\phi = (n_h - n_b)\,\omega = \omega \,.\, \Delta n. \quad . \quad . \quad . \quad (1)$$

The angle of incidence (i) at a given distance between the mirror and of the prism (fig. 2) is

$$i = r - \omega + \rho,$$

ρ being the angle of aperture of the mirror.

Fig. 2.



In the tringle $m_0 m' o$,

$$m_0 o : m' o = \sin i : \sin (r - \omega);$$

or approximately

$$m_0 o : m' o = i : r - \omega ; \quad r - \omega = (n - 1)\,\omega .$$

$m_0o=am_0+ao=a+p$, if a be the distance of the prism from the mirror, and p its radius of curvature; we then obtain:—

$$\frac{a+p}{p}=\frac{i}{(n-1)\omega}, \text{ or } i=\left(\frac{a}{r}+1\right)(n-1)\omega. \quad . \quad (2)$$

For the extreme red and violet rays we get:—

$$i_h=\left(\frac{a}{p}+1\right)(n_h-1)\omega, \quad i_h-i_b=\left(\frac{a}{p}+1\right)(n_h-n_b)\omega,$$

$$i_b=\left(\frac{a}{p}+1\right)(n_b-1)\omega, \quad i_h-i_b=\left(\frac{a}{p}+1\right)\Delta n\,.\,\omega. \quad . \quad (3)$$

The triangle $h\,l\,m_1$ contains the angle ψ of the red and violet rays after the reflection from the mirror; and it measures the increase of dispersion produced by the reflection from the cylindrical surface.

If, therefore, ψ is known, the ratio between the angles ϕ and ψ gives the real increase of dispersion produced in that way.

The angle hlm_1, as an exterior angle of the triangle lm_0m, is $hlm_1=\phi+2i$; and $2i$ being the exterior angle of the triangle hlm_1, we obtain:—

$$\psi+\phi+2i=2i_1,$$

$$\psi+\phi=2(i_1-i),$$

$$i_1-i=\frac{\psi+\phi}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Combining (3) and (4), we get:—

$$\frac{\psi+\phi}{2}=\left(\frac{a}{p}+1\right).\,\omega\,.\,\Delta n,$$

$$\phi=\omega\,.\,\Delta n,$$

$$\psi+\phi=2\left(\frac{a}{p}+1\right)\omega\,.\,\Delta n,$$

$$\psi=\left(\frac{2a}{p}+1\right)\omega\,.\,\Delta n; \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{\psi}{\phi}=\left(\frac{2a}{p}+1\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

To receive the spectrum on the surface of the mirror the angle ρ must be sufficiently great; it is given by the equation

$$\rho=i-(r-\omega); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\rho=i-(n-1)\omega,$$

or

$$\rho=\left(\frac{a}{p}+1\right)(n-1)\omega-(n-1)\omega;$$

$$\rho=\frac{a}{p}(n-1)\omega.$$

If f be the focal length of the mirror, we have

$$\frac{p}{2}=f, \text{ and } \frac{2}{p}=\frac{1}{f},$$

and we finally obtain:—the angle of dispersion

$$\phi=\omega\,.\,\Delta n; \quad \ldots\ldots\ldots \quad \text{(I.)}$$

the angle of reflection

$$i=\left(\frac{a}{2f}+1\right)(n-1)\omega; \quad \ldots\ldots \quad \text{(II.)}$$

the difference of the angles of reflection for the extreme red and violet rays

$$\Delta i=\left(\frac{a}{2f}+1\right)\omega\,.\,\Delta n, \quad \ldots\ldots \quad \text{(III.)}$$

the increase of dispersion being

$$\frac{\psi}{\phi}=(af+1). \quad \ldots\ldots\ldots \quad \text{(IV.)}$$

The angle of aperture required for the reflecting surface of the mirror becomes

$$S=\frac{a}{2f}(n-1)\omega. \quad \ldots\ldots\ldots \quad \text{(V.)}$$

The equations (I.) to (V.) give all the dimensions for the construction of the spectroscope itself.

Suppose the prism to be of crown glass, flint glass, sulphide of carbon, or ice-spar, we obtain:—

	Crown glass.	CS_2.	Flint glass.	Ice-spar.
n_h . .	1·5447	1·7025	1·6711	1·6833
n_b. . .	1·5243	1·6182	1·6277	1·6531
Δn . .	0·0204	0·0843	0·0434	0·0302

If the increase of the angle of dispersion be ϕ, or $\psi=\phi=21$, the focal length of the mirror $f=2$ centims., $p=4$ centims., then the distance is $a=40$ centims.; $\frac{a}{f}=20$; and the other values may be calculated as follows:—

	Crown glass.	CS_2.	Flint glass.	Ice-spar.
ϕ . .	$0·0204\omega$	$0·0843\omega$	$0·0434\omega$	$0·0302\omega$
ψ . .	$0·4284\omega$	$1·7703\omega$	$0·9114\omega$	$0·6342\omega$
Δi . .	$0·2244\omega$	$0·9273\omega$	$0·4774\omega$	$0·3322\omega$
S . .	$5·243\omega$	$7·025\omega$	$6·711\omega$	$6·531\omega$

It is obvious that ice-spar has the advantage of not absorbing large portions of the extreme red and violet rays, not refracting so much as flint glass or bisulphide of carbon, while yet its dispersion is nearly as much as that of flint glass; it requires therefore not so large an aperture of the mirror as flint glass or bisulphide of carbon.

Supposing the angle of the prism to be 10°, we obtain:—

$$\begin{aligned} \phi &= 0^{\circ}{\cdot}302, \\ \psi &= 6{\cdot}342, \\ \Delta i &= 3{\cdot}322, \\ \mathrm{S} &= 65{\cdot}31. \end{aligned}$$

For 20°:—

$$\begin{aligned} \phi &= 0^{\circ}{\cdot}604, \\ \psi &= 12{\cdot}684, \\ \Delta i &= 6{\cdot}644, \\ \mathrm{S} &= 130{\cdot}62. \end{aligned}$$

For more than 30° the reflecting mirror cannot give the whole spectrum at once, unless it has an aperture of 195°·93; and then it would reflect the rays on both sides of the prism, instead of only one as shown in the diagram.

In the case of the angle ω being 10°, the refracting angle of the prism should be nearly 211°, or three prisms must be used of 60°–70° to get the same dispersive power in a common spectroscope.

If the angle be 20°, six prisms of 60°–70° nearly should be used to get the same effect, and so on.

If the angle of incidence be not equal to zero, the dispersion increases to, very approximately,

$$\phi=\left(1+\frac{n^2\sin^2 i}{2}\right)\omega\Delta n,$$

and the preceding equations become:—

$$\phi=\left(1+\frac{n^2\sin^2 i}{2}\right).\,\omega\,.\,\Delta n, \quad \ldots\ldots \quad \text{(I.)}$$

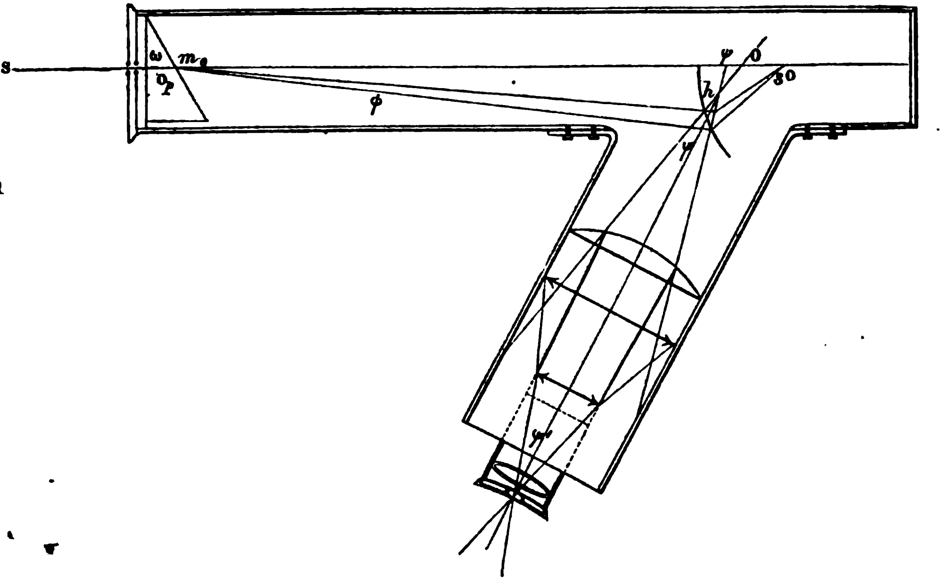
$$\psi=\left(\frac{a}{f}+1\right)\left(1+\frac{n^2\sin^2 i}{2}\right).\,\omega\,.\,\Delta n, \quad . \quad \text{(II.)}$$

$$\Delta i=\left(\frac{a}{2f}+1\right)\left(1+\frac{n^2\sin^2 i}{2}\right)\omega\,.\,\Delta n. \quad . \quad \text{(III.)}$$

There is a practical difficulty in getting exactly cylindrical surfaces; but it is possible to use spherical surfaces instead, if only a narrow zone of the middle part of a convex or concave silvered glass mirror be used. All the formulæ quoted above may be applied to it, and the dispersion augmented without fear of marring the exactness and definition of vision.

The new spectroscope, therefore, consists of a brass tube with a slit at one end and closed at the other end (fig. 3). The slit

Fig. 3.



must be narrow and short, and the prism very near to it, turning by a pinion (*p*) in its case. The mirror, inclined a certain angle ρ to the axis of the tube, reflects from the other, closed end the spectrum into a brass tube lateral to the former, containing a small telescopic apparatus to receive sharp images of the spectrum reflected by the convex spherical mirror, and to magnify them to ten times the diameter. The mirror is adjusted in such a way as to bring only a narrow zone of its middle part into operation, the rest of the surface being protected by a cover with a narrow slit in the direction of the rays emerging from the prism, and parallel to the reflecting zone of the mirror.

To increase the effect of the mirror, it can be brought nearer to the closed end of the tube, and the side tube moved in the same direction with it. Yet it seems more advisable to fix both at such a distance as will give the greatest separation of the spectral lines attainable without a great loss of light and a diminution of the exactness of the image.

London, October 17, 1873.

LVIII. *Reply to* Professor Challis's *further Remarks*[*] *"On the Received Principles of Hydrodynamics." By* ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*[†].

IN his recent paper Professor Challis reiterates, in a slightly different form, his argument derived from the equation $p=\text{funct.}(\rho, v)$, without taking any notice of my refutation of it.

Putting u, v, w for the resolved parts of the velocity, and x, y, z for the coordinates of a particle at the time t, he assumes that the motion in three dimensions of any fluid may be represented by the five following equations; viz.

$$\left.\begin{array}{l} p=f_1(x,y,z,t), \quad \rho=f_2(x,y,z,t), \quad u=f_3(x,y,z,t), \\ \quad v=f_4(x,y,z,t), \quad w=f_5(x,y,z,t)\ddagger. \end{array}\right\}.(\alpha)$$

Eliminating x, y, z, t from equations (α), Professor Challis obtains the equation $\mathrm{F}(p, \rho, u, v, w)=0$, which he assumes to be capable of being "satisfied by an *arbitrary* relation between the quantities —that is, by one which is independent of the particular problem." "The arbitrary condition," says Professor Challis, "has the effect of *defining* the fluid; and evidently the number of different kinds of fluids is unlimited."

I must here point out the very extraordinary character of the admission made by Professor Challis when he asserts that the number of fluids is unlimited, each fluid being defined by the relation between p, ρ, u, v, w prevailing in it—his sole object, so far as I understand it, being to prove that one only of this unlimited number of fluids, and that a fluid in which the highly

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[*] See Phil. Mag. for October last.

[†] Communicated by the Author.

[‡] From this view I must express my dissent, believing that the equations (α) are insufficient to determine the motion in the case in question. Not wishing upon the present occasion, however, to enter upon a fresh field of controversy, I shall content myself with observing that this assumption of Professor Challis contradicts his subsequent assumption that the resultant of the elimination of x, y, z, t from equations (α) will be a perfectly arbitrary function of p, ρ, u, v, w. For in the case of equilibrium the five equations (α) reduce to the pair

$$p=\{f_1(x,y,z,t)\}_{t=0}, \quad \rho=\{f_2(x,y,z,t)\}_{t=0}; \quad . \quad . \quad . \quad . \quad (\beta)$$

the elimination between which of z gives

$$p=\text{funct.}(\rho, x, y);$$

which is irreconcilable with Boyle's law in the case of equilibrium, unless we suppose that x and y both disappear along with z in the elimination. But this would require the functions on the right-hand sides of equations (β) to be of a special form; from which it follows that the functions f_1, f_2 must be of a special form, and therefore the result of elimination from them of x, y, t cannot be perfectly arbitrary.

special relation $p=a^2\rho$ obtains has the slightest claim on our attention.

Waiving this consideration, however, I join issue with Professor Challis on the point of our being able to assign to the relation $F(p, \rho, u, v, w)=0$ a definite form which is independent of the particular problem. By assigning a definite form to the arbitrary function $F(p, \rho, u, v, w)=0$, it is not true, as assumed by Professor Challis, that we simply *define* the fluid. We do a great deal more. We define the fluid, and at the same time impose conditions on the motion existing in it.

In proof of this I must again direct attention to the analytical argument of my last paper, which Professor Challis does not appear to have examined.

Confining ourselves to motion in one direction—suppose that we have a cylindrical tube filled with any particular fluid (say air of the mean density of the atmosphere); and suppose that at a given time t we have a disturbance extending over a limited portion of the column. Let the velocity and density throughout the disturbance at the time t follow any law whatsoever consistent with continuity; then I have shown in the paper in question that the pressure prevailing throughout the disturbance at the same time may follow any law whatever consistent with continuity*; from which it is evident that Professor Challis's argument, in proof "that it is allowable to make for fluid in motion the hypothesis that the pressure varies as the density always and at all points," must be fallacious.

I observe that Professor Challis mistakes the points on which I mainly rely for my "objections to the received principles of hydrodynamics."

My main argument is this:—Taking, in the case of motion in one direction,

$$p=f_1(xt), \quad \rho=f_2(xt), \quad v=f_3(xt).$$

I hence derive the equation

$$p=\text{funct.}\,(\rho, v);$$

whence it follows that the equation of motion

$$v=\frac{d^2y}{dt^2}+\frac{1}{D}\frac{dp}{dx}$$

will be satisfied by the three relations

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* It must be borne in mind that the condition of continuity requires that at the limits of the disturbance the velocity shall be zero, the density $=D$, the atmospheric mean density, and the pressure $=a^2D$.

$$p = -\frac{\alpha^2}{\rho} + \phi\left(v + \frac{\alpha}{\rho}\right),$$

$$v + \frac{\alpha}{\rho} = \psi_1\left\{x - \frac{\phi'(\omega) - \alpha}{\mathrm{D}} . t\right\},$$

$$\frac{1}{\rho} + \int \frac{d\omega}{\phi'(\omega) - 2\alpha} = \psi_2\left\{x - \frac{\alpha}{\mathrm{D}} . t\right\}.$$

A result more rigorously deduced or more analytically complete is not to be found in the whole range of mathematical physics; and my main objection to the received theory of hydrodynamics is that its upholders must arbitrarily set aside the above relations, being such as have been described, substituting in their room the equation $p = a^2\rho$ with such emasculated results as have been or may be* derived from it—an equation, be it remembered, which is unsupported by a single fact, which is opposed to every sound principle, and as to which all that can be said in its favour is that, in the infancy of the theory, the founders of the theory seized upon it in desperation, amidst the overwhelming difficulties by which they were beset, as the only law of pressure prevailing under any circumstances that up to that time had ever been so much as suggested.

But although I do not rest my main objection to the received principles of hydrodynamics upon the two points to which Professor Challis refers, I regard the views I have put forth in relation to those cases as of importance as showing in a popular manner, conclusively and at a glance as it were, the wholly untenable character of the existing theory of fluid pressure.

As to the first of these cases, Professor Challis writes:—"Mr. Moon founds an argument on the immediate juxtaposition of two densities one of which is double of the other; in other terms, he admits that $\frac{1}{\rho}\frac{dp}{dx}$ may have an infinite value."

I do not know on what ground Professor Challis attributes to me this admission; for, although on entirely different principles†, I am as satisfied as he can be as to the impossibility of variation of pressure taking place *per saltum*. Of course if we admit the equation $p=a^2\rho$, variation *per saltum* of density and variation *per saltum* of pressure necessarily go together; but in a discussion as to the truth of that equation it is evident that its validity cannot be assumed.

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* It is well known that the only solution of the equation of motion under these circumstances which has ever been demonstrated is that derived by Poisson, containing a single arbitrary function, and involving a relation between the velocity and density which it is quite impossible can always or even generally subsist. In a recent Number of the Philosophical Magazine I have shown, as I believe irrefragably, that this solution of Poisson's is the most general which, consistently with the relation $p = a^2\rho$, the equation of motion admits of.

† Professor Challis regards equality of pressure in all directions as a law upon which "the whole of analytical hydrodynamics depends." It is true that the founders of the theory of fluid motion in three dimensions, in their

But though believing variation of pressure *per saltum* to be impossible, I see no more difficulty in supposing a variation of density *per saltum* to occur in a fluid than in supposing two fluids of different densities to be in contact, or that two solids of different densities may be pressed together. Till, therefore, Professor Challis has shown variation of density *per saltum* to be impossible in a fluid, I shall consider myself entitled to insist on all the consequences which, according to the received theory, would flow from its occurrence.

As to the second case, where a weight is suddenly placed upon a piston supported by a vertical column of air, Professor Challis admits that the dilemma I have suggested "is fairly inferred and demands explanation." If it be true that the dilemma is fairly inferred, I think that most persons will regard it as incapable of explanation, and as necessarily involving a modification of the existing theory.

Professor Challis's explanation consists in supposing that "as soon as the weight is added, there will be *a tendency to an excess of pressure on the upper side* [of the stratum immediately underneath the piston] due to the disturbance of pressure in the solid piston."

If the words in italics mean any thing, they mean that there will be an increased pressure of the piston on the air, at the same time that, according to the received theory of pressure in elastic fluids, no increase can have taken place in the pressure of the air upon the piston; which is manifestly a contradiction of the law of the equality of action and reaction. Whatever disturbance of pressure may exist in the other parts of the solid piston, none such can occur at its base consistently with the relation $p=a^2\rho$, until the lapse of an interval of time after the deposition of the additional weight upon the piston.

At the same time I think that a true and complete theory of

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early struggles in connexion with the subject, did have recourse to the assumption of the truth of this law; but, so far is it from being an adequate foundation for a general theory of fluid motion, its retention is simple strangulation to the theory, and accounts for the limited and unpromising progeny which trace their origin to it. The true foundation of the theory of fluid motion is the principle of continuity (continuity of motion, that is, as well as continuity of mass), from which it results, as can readily be shown, that any element of the fluid mass will comport itself exactly in the same manner as an element of a rigid body would do under the same circumstances, and may therefore be dealt with accordingly.

the motion which would take place under the circumstances supposed might be evolved by having recourse to considerations similar to those which Professor Challis has suggested—viz. by considering first what would happen if the column of air were a perfectly rigid bar, next by considering the effect of a slight deviation from perfect rigidity, and finally by inferring from that analogy what would take place in the actual case. I hope hereafter to have an opportunity of developing what has been thus briefly sketched.

Avranches, October 11, 1873.

LIX. *On a new Relation between Heat and Static Electricity. By* A. W. Bickerton, *F.C.S., Associate of the Royal School of Mines, Lecturer on Experimental Science, Winchester College, and the Hartley Institution, Southampton*[*].

ON reading the able paper on the above subject by Dr. Guthrie in the October Number of the Philosophical Magazine, it occurred to me that the relationship might be most satisfactorily and easily explained by the assumption that currents of air passing over an electrified body carry off their electricity. The necessity for the assumption of a coercive force existing between these two physical forces (as suggested in the 'Proceedings of the Royal Society') is thus removed.

The following experiments seem to me to prove in a most decisive manner the correctness of my hypothesis.

Cold air, or cold gas of any kind, even at considerable pressure, when discharged against a charged Peltier's electrometer is incapable of taking away its electricity; but I found that with a stream of hot air the electroscope was rapidly discharged.

In one of the Professor's experiments a heated platinum wire is placed above an electrified body, the body being instantly discharged. I thought it possible that the induced electricity on the heated wire might charge the air in its vicinity with electricity; this charged air passing by attraction down to the electrified body would neutralize it. To ascertain if such a current of air existed, I placed the platinum spiral in the upper part of a gas-jar, a thermometer being passed through the cork so that its bulb was a short distance below the spiral. A brass knob connected with an electric machine was passed up into the jar so as to be close to the thermometer, about 2 inches below the spiral. The knob was also in connexion with a small quadrant electroscope. The machine was worked; and when the spiral was heated, a marked diminution in the indications of the electroscope was observed. After the apparatus had again attained the normal temperature, the current was sent continuously, the rise of the thermometer being noted every minute*, and the brass ball being electified and left unelectrified in alternate minutes. The thermometer indicated on the average a rise of one degree when the ball was not electrified and five degrees when it was electrified—thus proving the existence of a downward current of hot air when the ball was electrified.

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[*] Communicated by the Author.

Each time the machine was worked the sudden rush of air from the white-hot spiral caused its temperature to be appreciably lowered. On filling the vessel with smoke, the upward convection-currents of heated air showed themselves plainly. On working the machine, the sudden downward rush of smoke from the spiral to the ball was very striking. The smoke disappeared with surprising rapidity when the ball was electrified; it was doubtless burnt in passing over the heated spiral. Repeated experiments proved that the smoke was consumed very much more rapidly when the ball was electrified than when not electrified.

If heated air be the cause of the discharge, the heated wire will possess no power of discharge through rock-salt. This is actually the case; the ignited wire may be placed ever so close to a charged Peltier's electrometer without discharge, if a plate of rock-salt be interposed between them; but remove the salt, and discharge is instantaneous.

An induction-coil giving a spark $\frac{1}{20}$ inch will give a spark $\frac{1}{4}$ inch if the electrodes are at a high temperature. A 1-inch spark becomes $1\frac{1}{4}$ inch under the same conditions.

As it might be said that the lengthening of the spark of the coil was due to rarefaction of the air, the following experiment was made. Platinum wires were fused into the ends of a short piece of combustion-tube. The spark from the small coil passed through a distance of $\frac{3}{16}$ of an inch when the glass, except the fused ends, was strongly heated; the air of course was here almost at the same density. As it is possible to explain the apparent conductivity of glass observed by Dr. Guthrie on the above supposition of air-currents, I sent the spark from the small coil through the heated end of a closed glass tube. I also found that the current from four Grove's cells would pass through $1\frac{1}{2}$ inch of heated glass and deflect a galvanometer.

This power of conductivity is attained at a red heat, and increases enormously with slight increase of temperature. Thus, if a small sphere of glass have two platinum electrodes fused into the two sides and it be gradually heated, the galvanometer does not move for some time; but after the first movement is observed, the needle quickly passes up to 90°.

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* A clock striking minutes with the hand making a complete revolution in the same time was used in all time experiments.

If we assume that hot air possesses the power of carrying off + electricity more readily than —, the whole of Dr. Guthrie's apparently contradictory experiments of the different effects of + and — electricity may be easily explained. Thus a heated body cannot be charged with + electricity, because the heated air carries it off. A body near a heated conductor cannot be charged with —, because it induces + electricity in the heated body; this electrifies the heated air, and the electricity is then carried to the electrified — body and neutralizes it.

Faraday proved that air at ordinary temperatures carried off — electricity most easily.

Hence the experiments of Faraday, Dr. Guthrie, and myself, taken in conjunction, prove the two following principles:—

1st. That at low temperatures — electricity is taken away by air most easily; at certain temperatures both electricities are carried off with equal facility, and at high temperatures + electricity most easily.

2nd. That high-tension electricity can be conveyed away at low temperatures; and as the tension gets lower and lower, it requires the air to be of higher and higher temperatures to carry it off.

To illustrate the second principle, I placed a needle on the knob of a fully charged Peltier's electrometer and left the point within $\frac{1}{100}$ of an inch of an earth-connected brass ball; after two hours the electroscope had only lost half a degree; and on passing a current of hot air, or putting a heated point near it, the electricity was instantly discharged.

The lengthening of the electric arc after the current is once established may be due to these principles, especially from the well-known fact that the part of the + carbon where induction is strongest wears away most rapidly; while the slight wear of the other carbon is as much down the sides as at the point, and hence it remains pointed. It is further confirmed by the fact that if the electric current pass between two electrodes of different oxidizable metals, the space about the light becomes filled with particles of the oxide of the + pole.

LX. *A Discourse on Molecules. By* J. CLERK MAXWELL, *M.A., F.R.S., Professor of Experimental Physics in the University of Cambridge*[*].

AN atom is a body which cannot be cut in two. A molecule is the smallest possible portion of a particular substance. No one has ever seen or handled a single molecule. Molecular science, therefore, is one of those branches of study which deal with things invisible and imperceptible by our senses, and which cannot be subjected to direct experiment.

The mind of man has perplexed itself with many hard questions. Is space infinite, and if so, in what sense? Is the material world infinite in extent, and are all places within that extent equally full of matter? Do atoms exist, or is matter infinitely divisible?

The discussion of questions of this kind has been going on ever since men began to reason; and to each of us, as soon as we obtain the use of our faculties, the same old questions arise as fresh as ever. They form as essential a part of the science of the nineteenth century of our era as of that of the fifth century before it.

We do not know much about the science organization of Thrace twenty-two centuries ago, or of the machinery then employed for diffusing an interest in physical research. There were men, however, in those days who devoted their lives to the pursuit of knowledge with an ardour worthy of the most distinguished members of the British Association; and the lectures in which Democritus explained the atomic theory to his fellow-citizens of Abdera realized, not in golden opinions only, but in golden talents, a sum hardly equalled even in America.

To another very eminent philosopher, Anaxagoras, best known to the world as the teacher of Socrates, we are indebted for the most important service to the atomic theory which, after its statement by Democritus, remained to be done. Anaxagoras, in fact, stated a theory which so exactly contradicts the atomic theory of Democritus, that the truth or falsehood of the one theory implies the falsehood or truth of the other. The question of the existence or non-existence of atoms cannot be presented to us this evening with greater clearness than in the alternative theories of these two philosophers.

Take any portion of matter, say a drop of water, and observe its properties. Like every other portion of matter we have ever seen, it is divisible. Divide it in two, each portion appears to retain all the properties of the original drop, and among others that of being divisible. The parts are similar to the whole in every respect except in absolute size.

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[*] Read before the British Association at Bradford, September 22, 1873.

Now go on repeating the process of division till the separate portions of water are so small that we can no longer perceive or handle them. Still we have no doubt that the subdivision might be carried further if our senses were more acute and our instruments more delicate. Thus far all are agreed; but now the question arises, Can this subdivision be repeated for ever?

According to Democritus and the atomic school, we must answer in the negative. After a certain number of subdivisions the drop would be divided into a number of parts each of which is incapable of further subdivision. We should thus in imagination arrive at the atom, which, as its name literally signifies, cannot be cut in two. This is the atomic doctrine of Democritus, Epicurus, and Lucretius, and, I may add, of your lecturer.

According to Anaxagoras, on the other hand, the parts into which the drop is divided are in all respects similar to the whole drop, the mere size of a body counting for nothing as regards the nature of its substance. Hence, if the whole drop is divisible, so are its parts down to the minutest subdivisions, and that without end.

The essence of the doctrine of Anaxagoras is that parts of a body are in all respects similar to the whole. It was therefore called the doctrine of Homoiomereia. Anaxagoras did not, of course, assert this of the parts of organized bodies such as men and animals; but he maintained that those inorganic substances which appear to us homogeneous are really so, and that the universal experience of mankind testifies that every material body without exception is divisible.

The doctrine of atoms and that of homogeneity are thus in direct contradiction.

But we must now go on to molecules. Molecule is a modern word. It does not occur in Johnson's Dictionary. The id it embodies are those belonging to modern chemistry.

A drop of water (to return to our former example) may be divided into a certain number, and no more, of portions similar to each other. Each of these the modern chemist calls a molecule of water. But it is by no means an atom, for it contains two different substances, oxygen and hydrogen; and by a certain process the molecule may be actually divided into two parts, one consisting of oxygen and the other of hydrogen. According to the received doctrine, in each molecule of water there are two molecules of hydrogen and one of oxygen. Whether these are or are not ultimate atoms I shall not attempt to decide.

We now see what a molecule is, as distinguished from an atom.

A molecule of a substance is a small body such that if, on the one hand, a number of similar molecules were assembled together, they would form a mass of that substance; while, on the other hand, if any portion of this molecule were removed, it would no longer be able, along with an assemblage of other molecules similarly treated, to make up a mass of the original substance.

Every substance, simple or compound, has its own molecule. If this molecule be divided, its parts are molecules of a different substance or substances from that of which the whole is a molecule. An atom, if there is such a thing, must be a molecule of an elementary substance. Since, therefore, every molecule is not an atom, but every atom is a molecule, I shall use the word molecule as the more general term.

I have no intention of taking up your time by expounding the doctrines of modern chemistry with respect to the molecules of different substances. It is not the special but the universal interest of molecular science which encourages me to address you. It is not because we happen to be chemists or physicists or specialists of any kind that we are attracted towards this centre of all material existence, but because we all belong to a race endowed with faculties which urge us on to search deep and ever deeper into the nature of things.

We find that now, as in the days of the earliest physical speculations, all physical researches appear to converge towards the same point, and every inquirer, as he looks forward into the dim region towards which the path of discovery is leading him, sees, each according to his sight, the vision of the same quest.

One may see the atom as a material point, invested and surrounded by potential forces. Another sees no garment of force, but only the bare and utter hardness of mere impenetrability.

But though many a speculator, as he has seen the vision recede before him into the innermost sanctuary of the inconceivably little, has had to confess that the quest was not for him; and though philosophers in every age have been exhorting each other to direct their minds to some more useful and attainable aim, each generation, from the earliest dawn of science to the present time, has contributed a due proportion of its ablest intellects to the quest of the ultimate atom.

Our business this evening is to describe some researches in molecular science, and in particular to place before you any definite information which has been obtained respecting the molecules themselves. The old atomic theory, as described by Lucretius and revived in modern times, asserts that the molecules of all bodies are in motion, even when the body itself appears to be at rest. These motions of molecules are, in the case of solid bodies, confined within so narrow a range that even with our best microscopes we cannot detect that they alter their places at all. In liquids and gases, however, the molecules are not confined within any definite limits, but work their way through the whole mass, even when that mass is not disturbed by any visible motion.

This process of diffusion, as it is called, which goes on in gases and liquids and even in some solids, can be subjected to experiment, and forms one of the most convincing proofs of the motion of molecules.

Now the recent progress of molecular science began with the study of the mechanical effect of the impact of these moving molecules when they strike against any solid body. Of course these flying molecules must beat against whatever is placed among them; and the constant succession of these strokes is, according to our theory, the sole cause of what is called the pressure of air and other gases.

This appears to have been first suspected by Daniel Bernoulli; but he had not the means which we now have of verifying the theory. The same theory was afterwards brought forward independently by Lesage, of Geneva, who, however, devoted most of his labour to the explanation of gravitation by the impact of atoms. Then Herapath, in his 'Mathematical Physics,' published in 1847, made a much more extensive application of the theory to gases; and Dr. Joule, whose absence from our Meeting we must all regret, calculated the actual velocity of the molecules of hydrogen.

The further development of the theory is generally supposed to have begun with a paper by Krönig, which does not, however, so far as I can see, contain any improvement on what had gone before. It seems, however, to have drawn the attention of Professor Clausius to the subject; and to him we owe a very large part of what has been since accomplished.

We all know that air or any other gas placed in a vessel presses against the sides of the vessel, and against the surface of any body placed within it. On the kinetic theory this pressure is entirely due to the molecules striking against these surfaces, and thereby communicating to them a series of impulses which follow each other in such rapid succession that they produce an effect which cannot be distinguished from that of a continuous pressure.

If the velocity of the molecules is given and the number varied, then since each molecule on an average strikes the sides of the vessel the same number of times, and with an impulse of the same magnitude, each will contribute an equal share to the whole pressure. The pressure in a vessel of given size is therefore proportional to the number of molecules in it—that is, to the quantity of gas in it.

This is the complete dynamical explanation of the fact discovered by Robert Boyle—that the pressure of air is proportional to its density. It shows also that, of different portions of gas forced into a vessel, each produces its own part of the pressure independently of the rest, and this whether these portions be of the same gas or not.

Let us next suppose that the velocity of the molecules is increased. Each molecule will now strike the sides of the vessel a greater number of times in a second; but, besides this, the impulse of each blow will be increased in the same proportion, so that the part of the pressure due to each molecule will vary as the *square* of the velocity. Now the increase of velocity corresponds, on our theory, to a rise of temperature; and in this way we can explain the effect of warming the gas, and also the law discovered by Charles, that the proportional expansion of all gases between given temperatures is the same.

The dynamical theory also tells us what will happen if molecules of different masses are allowed to knock about together. The greater masses will go slower than the smaller ones, so that on an average every molecule, great or small, will have the same energy of motion.

The proof of this dynamical theorem, in which I claim the priority, has recently been greatly developed and improved by Dr. Ludwig Boltzmann. The most important consequence which flows from it is that a cubic centimetre of every gas at standard temperature and pressure contains the same number of molecules. This is the dynamical explanation of Gay-Lussac's law of the equivalent volumes of gases. But we must now descend to particulars, and calculate the actual velocity of a molecule of hydrogen.

A cubic centimetre of hydrogen, at the temperature of melting ice and at a pressure of one atmosphere, weighs 0·00008954 gramme. We have to find at what rate this small mass must move (whether altogether or in separate molecules makes no difference) so as to produce the observed pressure on the sides of the cubic centimetre. This is the calculation which was first made by Dr. Joule; and the result is 1859 metres per second. This is what we are accustomed to call a great velocity. It is greater than any velocity obtained in artillery practice. The velocity of other gases is less, as you will see by the Table; but in all cases it is very great as compared with that of bullets.

We have now to conceive the molecules of the air in this hall flying about in all directions at a rate of about seventy miles in a minute.

If all these molecules were flying in the same direction they would constitute a wind blowing at the rate of seventy miles a minute; and the only wind which approaches this velocity is that which proceeds from the mouth of a cannon. How, then, are you and I able to stand here? Only because the molecules happen to be flying in different directions, so that those which strike against our backs enable us to support the storm which is beating against our faces. Indeed, if this molecular bombardment were to cease even for an instant, our veins would swell, our breath would leave us, and we should literally expire.

But it is not only against us or against the walls of the hall that the molecules are striking. Consider the immense number of them, and the fact that they are flying in every possible direction, and you will see that they cannot avoid striking each other. Every time that two molecules come into collision the paths of both are changed and they go off in new directions. Thus each molecule is continually getting its course altered; so that, in spite of its great velocity, it may be a long time before it reaches any great distance from the point at which it set out.

I have here a bottle containing ammonia. Ammonia is a gas which you can recognize by its smell. Its molecules have a velocity of six hundred metres per second; so that if their course had not been interrupted by striking against the molecules of air in the hall, every one in the most distant gallery would have smelt ammonia before I was able to pronounce the name of the gas. But instead of this, each molecule of ammonia is so jostled about by the molecules of air that it is sometimes going one way and sometimes another, and, like a hare which is always doubling, though it goes a great pace, it makes very little progress. Nevertheless the smell of ammonia is now beginning to be perceptible at some distance from the bottle. The gas does diffuse itself through the air, though the process is a slow one; and if we could close up every opening of this hall so as to make it air-tight, and leave every thing to itself for some weeks, the ammonia would become uniformly mixed through every part of the air in the hall.

This property of gases, that they diffuse through each other, was first remarked by Priestley. Dalton showed that it takes place quite independently of any chemical action between the interdiffusing gases. Graham, whose researches were especially directed towards those phenomena which seem to throw light on molecular motions, made a careful study of diffusion, and obtained the first results from which the rate of diffusion could be calculated.

Still more recently the rates of diffusion of gases into each other have been measured with great precision by Professor Loschmidt, of Vienna.

He placed the two gases in two similar vertical tubes, the formation of currents. He then opened a sliding valve so as to make the two tubes into one; and after leaving the gases to themselves for an hour or so, he shut the valve, and determined how much of each gas had diffused into the other.

As most gases are invisible, I shall exhibit gaseous diffusion to you by means of two gases (ammonia and hydrochloric acid) which when they meet form a solid product. The ammonia, being the lighter gas, is placed above the hydrochloric acid with a stratum of air between; but you will soon see that the gases can diffuse through this stratum of air and produce a cloud of white smoke when they meet. During the whole of this process no currents or any other visible motion can be detected. Every part of the vessel appears as calm as a jar of undisturbed air.

But, according to our theory, the same kind of motion is going on in calm air as in the interdiffusing gases, the only difference being that we can trace the molecules from one place to another more easily when they are of a different nature from those through which they are diffusing.

If we wish to form a mental representation of what is going on among the molecules in calm air, we cannot do better than observe a swarm of bees, when every individual bee is flying furiously, first in one direction and then in another, while the swarm, as a whole, either remains at rest or sails slowly through the air.

In certain seasons swarms of bees are apt to fly off to a great distance, and the owners, in order to identify their property when they find them on other people's ground, sometimes throw handfuls of flour at the swarm. Now let us suppose that the flour thrown at the flying swarm has whitened those bees only which happened to be in the lower half of the swarm, leaving those in the upper half free from flour. If the bees still go on flying hither and thither in an irregular manner, the floury bees will be found in continually increasing proportions in the upper part of the swarm till they have become equally diffused through every part of it. But the reason of this diffusion is not because the bees were marked with flour, but because they are flying about. The only effect of the marking is to enable us to identify certain bees.

We have no means of marking a select number of molecules of air, so as to trace them after they have become diffused among others, but we may communicate to them some property by which we may obtain evidence of their diffusion.

For instance, if a horizontal stratum of air is moving horizontally, molecules diffusing out of this stratum into those above and below will carry their horizontal motion with them, and so tend to communicate motion to the neighbouring strata, while molecules diffusing out of the neighbouring strata into the moving one will tend to bring it to rest. The action between the strata is somewhat like that of two rough surfaces, one of which slides over the other, rubbing on it. Friction is the name given to this action between solid bodies; in the case of fluids it is called internal friction, or viscosity.

It is, in fact, only another kind of diffusion—a lateral diffusion of momentum; and its amount can be calculated from data derived from observations of the first kind of diffusion, that of matter. The comparative values of the viscosity of different gases were determined by Graham in his researches on the transpiration of gases through long narrow tubes; and their absolute values have been deduced from experiments on the oscillation of disks by Oscar Meyer and myself.

Another way of tracing the diffusion of molecules through calm air is to heat the upper stratum of the air in a vessel, and to observe the rate at which this heat is communicated to the lower strata. This, in fact, is a third kind of diffusion—that of energy; and the rate at which it must take place was calculated from data derived from experiments on viscosity before any direct experiments on the conduction of heat had been made. Professor Stefan, of Vienna, has recently, by a very delicate method, succeeded in determining the conductivity of air; and he finds it, as he tells us, in striking agreement with the value predicted by the theory.

All these three kinds of diffusion (the diffusion of matter, of momentum, and of energy) are carried on by the motion of the molecules. The greater the velocity of the molecules and the further they travel before their paths are altered by collision with other molecules, the more rapid will be the diffusion. Now we know already the velocity of the molecules; and therefore, by experiments on diffusion, we can determine how far, on an average, a molecule travels without striking another. Professor Clausius, of Bonn, who first gave us precise ideas about the motion of agitation of molecules, calls this distance the mean path of a molecule. I have calculated, from Professor Loschmidt's diffusion-experiments, the mean path of the molecules of four well-known gases. The average distance travelled by a molecule between one collision and another is given in the Table. It is a very small distance, quite imperceptible to us even with our best microscopes. Roughly speaking, it is about the tenth part of the length of a wave of light, which you know is a very small quantity. Of course the time spent on so short a path by such swift molecules must be very small. I have calculated the number of collisions which each must undergo in a second. They are given in the Table, and are reckoned by thousands of millions. No wonder that the travelling power of the swiftest molecule is but small when its course is completely changed thousands of millions of times in a second.

The three kinds of diffusion also take place in liquids; but the relation between the rates at which they take place is not so simple as in the case of gases. The dynamical theory of liquids is not so well understood as that of gases; but the principal difference between a gas and a liquid seems to be that in a gas each molecule spends the greater part of its time in describing its free path, and is for a very small portion of its time engaged in encounters with other molecules; whereas in a liquid the molecule has hardly any free path, and is always in a state of close encounter with other molecules.

Hence in a liquid the diffusion of motion from one molecule to another takes place much more rapidly than the diffusion of the molecules themselves, for the same reason that it is more expeditious in a dense crowd to pass on a letter from hand to hand than to give it to a special messenger to work his way through the crowd. I have here a jar, the lower part of which contains a solution of copper sulphate, while the upper part contains pure water. It has been standing here since Friday, and you see how little progress the blue liquid has made in diffusing itself through the water above. The rate of diffusion of a solution of sugar has been carefully observed by Voit. Comparing his results with those of Loschmidt on gases, we find that about as much diffusion takes place in a second in gases as requires a day in liquids.

The rate of diffusion of momentum is also slower in liquids than in gases, but by no means in the same proportion. The same amount of motion takes about ten times as long to subside in water as in air, as you will see by what takes place when I stir these two jars, one containing water and the other air. There is still less difference between the rates at which a rise of temperature is propagated through a liquid and through a gas.

In solids the molecules are still in motion, but their motions are confined within very narrow limits. Hence the diffusion of matter does not take place in solid bodies, though that of motion and heat takes place very freely. Nevertheless certain liquids can diffuse through colloid solids, such as jelly and gum; and hydrogen can make its way through iron and palladium.

We have no time to do more than mention that most wonderful molecular motion which is called electrolysis. Here is an electric current passing through acidulated water, and causing oxygen to appear at one electrode and hydrogen at the other.

In the space between the water is perfectly calm; and yet two opposite currents of oxygen and of hydrogen must be passing through it. The physical theory of this process has been studied by Clausius, who has given reasons for asserting that in ordinary water the molecules are not only moving, but every now and then striking each other with such violence that the oxygen and hydrogen of the molecules part company and dance about through the crowd, seeking partners which have become dissociated in the same way. In ordinary water these exchanges produce, on the whole, no observable effect; but no sooner does the electromotive force begin to act than it exerts its guiding influence on the unattached molecules, and bends the course of each toward its proper electrode till the moment when, meeting with an unappropriated molecule of the opposite kind, it enters again into a more or less permanent union with it till it is again dissociated by another shock. Electrolysis, therefore, is a kind of diffusion assisted by electromotive force.

Another branch of molecular science is that which relates to the exchange of molecules between a liquid and a gas. It includes the theory of evaporation and condensation, in which the gas in question is the vapour of the liquid, and also the theory of the absorption of a gas by a liquid of a different substance. The researches of Dr. Andrews on the relations between the liquid and the gaseous state have shown us that though the statements in our elementary text-books may be so neatly expressed as to appear almost self-evident, their true interpretation may involve some principle so profound that, till the right man has laid hold of it, no one ever suspects that any thing is left to be discovered.

These, then, are some of the fields from which the data of molecular science are gathered. We may divide the ultimate results into three ranks, according to the completeness of our knowledge of them. To the first rank belong the relative masses of the molecules of different gases, and their velocities in metres per second. These data are obtained from experiments on the pressure and density of gases, and are known to a high degree of precision.

In the second rank we must place the relative size of the molecules of different gases, the length of their mean paths, and the number of collisions in a second. These quantities are deduced from experiments on the three kinds of diffusion. Their received values must be regarded as rough approximations till the methods of experimenting are greatly improved.

There is another set of quantities, which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate, as in the second, but is only of molecules in a cubic centimetre. We know the relative masses of different molecules with great accuracy; and we know their relative diameters approximately. From these we can deduce the relative densities of the molecules themselves. So far we are on firm ground.

The great resistance of liquids to compression makes it probable that their molecules must be at about the same distance from each other as that at which two molecules of the same substance in the gaseous form act on each other during an encounter. This conjecture has been put to the test by Lorenz Meyer, who has compared the densities of different liquids with the calculated relative densities of the molecules of their vapours, and has found a remarkable correspondence between them.

Now Loschmidt has deduced from the dynamical theory the following remarkable proportion:—As the volume of a gas is to the combined volume of all the molecules contained in it, so is the mean path of a molecule to one eighth of the diameter of a molecule.

Assuming that the volume of the substance, when reduced to the liquid form, is not much greater than the combined volume of the molecules, we obtain from this proportion the diameter of a molecule. In this way Loschmidt, in 1865, made the first estimate of the diameter of a molecule. Independently of him and of each other, Mr. Stoney in 1868, and Sir W. Thomson in 1870, published results of a similar kind, those of Thomson being deduced not only in this way, but from considerations derived from the thickness of soap-bubbles and from the electric properties of metals.

According to the Table, which I have calculated from Loschmidt's data, the size of the molecules of hydrogen is such that about two millions of them in a row would occupy a millimetre, and a million million million millions of them would weigh between four and five grammes.

In a cubic centimetre of any gas at standard pressure and temperature there are about nineteen million million million molecules. All these numbers of the third rank are, I need not tell you, to be regarded as at present conjectural. In order to warrant us in putting any confidence in numbers obtained in this way, we should have to compare together a greater number of independent data than we have as yet obtained, and to show that they lead to consistent results.

Thus far we have been considering molecular science as an inquiry into natural phenomena. But though the professed aim of all scientific work is to unravel the secrets of nature, it has another effect, not less valuable, on the mind of the worker.

It leaves him in possession of methods which nothing but scientific work could have led him to invent; and it places him in a position from which many regions of nature, besides that which he has been studying, appear under a new aspect.

The study of molecules has developed a method of its own, and it has also opened up new views of nature.

When Lucretius wishes us to form a mental representation of the motion of atoms, he tells us to look at a sunbeam shining through a darkened room (the same instrument of research by which Dr. Tyndall makes visible to us the dust we breathe), and to observe the motes which chase each other in all directions through it. This motion of the visible motes, he tells us, is but a result of the far more complicated motion of the invisible atoms which knock the motes about. In his dream of nature, as Tennyson tells us, he

> "Saw the flaring atom-streams
> And torrents of her myriad universe,
> Ruining along the illimitable inane,
> Fly on to clash together again, and make
> Another and another frame of things
> For ever."

And it is no wonder that he should have attempted to burst the bonds of Fate by making his atoms deviate from their courses at quite uncertain times and places, thus attributing to them a kind of irrational free will, which on his materialistic theory is the only explanation of that power of voluntary action of which we ourselves are conscious.

As long as we have to deal with only two molecules, and have all the data given us, we can calculate the result of their encounter; but when we have to deal with millions of molecules, each of which has millions of encounters in a second, the complexity of the problem seems to shut out all hope of a legitimate solution.

The modern atomists have therefore adopted a method which is, I believe, new in the department of mathematical physics, though it has long been in use in the section of statistics. When the working members of Section F get hold of a report of the census or any other document containing the numerical data of economic and social science, they begin by distributing the whole population into groups, according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the history of each separately; so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work.

This of course is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen is therefore to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical, and the other the statistical method.

The equations of dynamics completely express the laws of the historical method as applied to matter; but the application of these equations implies a perfect knowledge of all the data. But the smallest portion of matter which we can subject to experiment consists of millions of molecules, not one of which ever becomes individually sensible to us. We cannot, therefore, ascertain the actual motion of any one of these molecules; so that we are obliged to abandon the strict historical method and to adopt the statistical method of dealing with large groups of molecules.

The data of the statistical method as applied to molecular science are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind we meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics.

Thus molecular science teaches us that our experiments can never give us any thing more than statistical information, and that no law deduced from them can pretend to absolute precision. But when we pass from the contemplation of our experiments to that of the molecules themselves, we leave the world of chance and change, and enter a region where every thing is certain and immutable.

The molecules are conformed to a constant type with a precision which is not to be found in the sensible properties of the bodies which they constitute. In the first place, the mass of each individual molecule and all its other properties are absolutely unalterable. In the second place, the properties of all molecules of the same kind are absolutely identical.

Let us consider the properties of two kinds of molecules, those of oxygen and those of hydrogen.

We can procure specimens of oxygen from very different sources—from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different; and if during thousands of years difference of circumstances could produce difference of properties, these specimens of oxygen would show it.

In like manner we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water.

Now, if during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved.

But we have another and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements; and when these are excited, it emits rays, the wavelength of which is a measure of the time of vibration of the molecule.

By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained not only that molecules taken from every specimen of hydrogen in our laboratories have the same set of periods of vibration, but that light having the same set of periods of vibration is emitted from the sun and from the fixed stars.

We are thus assured that molecules of the same nature as those of our hydrogen exist in those distant regions, or at least did exist when the light by which we see them was emitted.

From a comparison of the dimensions of the buildings of the Egyptians with those of the Greeks, it appears that they have a common measure. Hence, even if no ancient author had recorded the fact that the two nations employed the same cubit as a standard of length, we might prove it from the buildings themselves. We should also be justified in asserting that at some time or other a material standard of length must have been carried from one country to the other, or that both countries had obtained their standards from a common source.

But in the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another; and yet this light, which is to us the sole evidence of the existence of these distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in presisely the same time.

Each molecule, therefore, throughout the universe bears impressed on it the stamp of a metric system as distinctly as does the metre of the Archives at Paris or the double royal cubit of the Temple of Karnac.

No theory of evolution can be formed to account for the similarity of molecules; for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction.

None of the processes of nature, since the time when nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural.

On the other hand, the exact equality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent.

Thus we have been led, along a strictly scientific path, very near to the point at which science must stop. Not that science is debarred from studying the internal mechanism of a molecule which she cannot take to pieces, any more than from investigating an organism which she cannot put together. But in tracing back the history of matter, science is arrested when she assures herself, on the one hand, that the molecule has been made, and on the other, that it has not been made by any of the processes we call natural.

Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created.

It is only when we contemplate, not matter in itself, but the form in which it actually exists, that our mind finds something on which it can lay hold.

That matter, as such, should have certain fundamental properties—that it should exist in space and be capable of motion, that its motion should be persistent, and so on, are truths which may, for any thing we know, be of the kind which metaphysicians call necessary. We may use our knowledge of such truths for purposes of deduction; but we have no data for speculating as to their origin.

But that there should be exactly so much matter and no more in every molecule of hydrogen is a fact of a very different order. We have here a particular distribution of matter—a *collocation* —to use the expression of Dr. Chambers, of things which we have no difficulty in imagining to have been arranged otherwise.

The form and dimensions of the orbits of the planets, for instance, are not determined by any law of nature, but depend upon a particular collocation of matter. The same is the case with respect to the size of the earth, from which the standard of what is called the metrical system has been derived. But these astronomical and terrestrial magnitudes are far inferior in scientific importance to that most fundamental of all standards which forms the base of the molecular system. Natural causes, as we know, are at work which tend to modify, if they do not at length destroy, all the arrangements and dimensions of the earth and the whole solar system. But though in the course of ages catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn. They continue this day as they were created—perfect in number and measure and weight; and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him who in the beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist.

TABLE of Molecular Data.

		Hydrogen.	Oxygen.	Carbonic oxide.	Carbonic acid.
Rank I.	Mass of molecule (hydrogen = 1)	1	16	14	22
	Velocity, mean (square metres per second), at 0° C.	1859	465	497	396
Rank II.	Mean path, tenth-metres......	965	560	482	379
	Collisions in a second (millions)	17750	7646	9489	9720
Rank III.	Diameter, tenth-metres.........	5·8	7·6	8·3	9·3
	Mass, twenty-fifth grammes...	46	736	644	1012

TABLE of Diffusion.

$\frac{(\text{Centimetre})^2}{\text{Second}}$ measure.

	Calculated.	Observed.	
H & O	0·7086	0·7214	Diffusion of matter observed by Loschmidt.
H & CO	0·6519	0·6422	
H & CO^2	0·5575	0·5558	
O & CO	0·1807	0·1802	
O & CO^2	0·1427	0·1409	
CO & CO^2	0·1386	0·1406	
H	1·2990	1·49	Diffusion of momentum (Graham and Meyer).
O	0·1884	0·213	
CO	0·1748	0·212	
CO^2	0·1087	0·117	
Air	....	0·256	Diffusion of temperature observed by Stefan.
Copper	....	1·077	
Iron	....	0·183	
Cane-sugar in water	....	0·00000365	Voit.
(or in a day	....	0·3144)	
Salt in water	....	0·00000116	Fick.

LXI. *On the Differential Galvanometer.*

By OLIVER HEAVISIDE, *Newcastle-on-Tyne**.

THE great similarity between the systems of resistance-measuring by means of the differential galvanometer and Wheatstone's bridge, the latter having been probably suggested by the former, must have struck every one who has had any thing to do with them. In each case do we make one resistance a fourth proportional to three others, and, knowing the three, deduce the fourth. As in the bridge for every resistance to be measured there is a certain arrangement of the three other sides which gives the most sensitive balance, so with the differential galvanometer there must be a best arrangement for any particular case, which it is the object of this paper to point out.

The expression for the strength of the current through the galvanometer in Wheatstone's bridge is

$$E=\frac{v\cdot\dfrac{ad-bc}{a+b+c+d}}{\left\{\dfrac{(a+b)(c+d)}{a+b+c+d}+e\right\}\cdot\left\{\dfrac{(a+c)(b+d)}{a+b+c+d}+f\right\}}\quad . \qquad (1)$$

(where v is the electromotive force of the battery, E the current through e, and the resistances as in the diagram) when at a balance and therefore $ad-bc$ a vanishing quantity.

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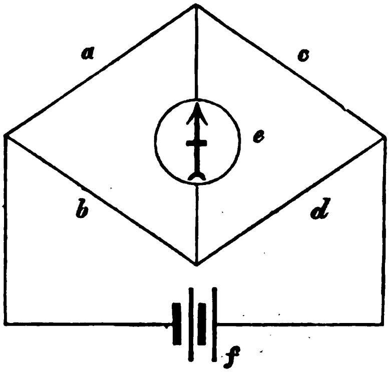
* Communicated by the Author.



To deduce from this the expression for the force acting on the needle in the differential galvanometer, let a and b be the two coils. Then, in the first place, by Kirchhoff's rule,

$$Aa-Bb=Ee,$$

where A, B, E are the currents in a, b, and e respectively; and next, that as e is absent in the differential-galvanometer arrangement, we must make e infinite. Therefore, multiplying (1) by e and making $e=\infty$, we obtain

$$Aa-Bb=\frac{v\,.\,\dfrac{ad-bc}{a+b+c+d}}{\dfrac{(a+c)(b+d)}{a+b+c+d}+f} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (2)$$

for the differential galvanometer. This can, of course, be obtained independently of any consideration of Wheatstone's bridge, but makes it evident that the best arrangement of the differential galvanometer with a given battery may be derived from the Wheatstone's-bridge formulæ by making e infinite in them.

These formulæ are as follows (Phil. Mag. February 1873). When c, d, e, and f are fixed,

$$\left.\begin{aligned} a&=\sqrt{\frac{c}{d}\,.\,\frac{cd+df+fc}{c+d+e}\,.\,e},\\ b&=\sqrt{\frac{d}{c}\,.\,\frac{cd+df+fc}{c+d+e}\,.\,e}; \end{aligned}\right\} \quad . \quad . \quad . \quad . \qquad (3)$$

and if c be not arbitrarily fixed,

$$\left.\begin{aligned} a&=\sqrt{ef},\\ b&=\sqrt{de\,.\,\frac{d+f}{d+e}},\\ c&=\sqrt{df\,.\,\frac{d+e}{d+f}}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

which is the most sensitive arrangement possible with a given galvanometer and battery. Finally, if the best resistances for the galvanometer and battery are also to be employed, we must make every branch of the bridge of the same resistance, viz. that of d, the resistance to be measured.

Making $e=\infty$ in (3) and (4), they become

$$\left.\begin{aligned} a&=\sqrt{\frac{c}{d}(cd+df+fc)},\\ b&=\sqrt{\frac{d}{c}(cd+df+fc)}, \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \quad (5)$$

and

$$\left.\begin{aligned} a&=\infty,\\ b&=\sqrt{d(d+f)},\\ c&=\infty, \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6)$$

which may also be obtained by differentiation from (2).

These formulæ, (5) and (6), would not be of any particular use if we had no means of varying at will the resistance of the coils a and b. This cannot be accomplished directly without considerable complication; but by means of shunts the same end may be reached. Thus, using (5), if the coils of our galvanometer have resistances greater than those best suited for the particular resistance to be measured, by means of shunts we may reduce their resistances to the required extent. And here a remarkable peculiarity presents itself. In general when a galvanometer is shunted, its resistance and sensibility are reduced and in the same proportion. Not so with the differential galvanometer; for its sensitiveness will be increased or reduced by shunts according as the normal resistances of its coils are greater or less than their best values in the particular case under consideration. The accuracy of (5) may be easily verified experimentally.

Proceeding to examine (6), we meet practical impossibilities, and can only carry it out by making the resistance of the coil a as great as possible by not shunting it. But as the values of b and c in (6) correspond to $a=\infty$, it will be necessary to find their values when $a=a$. Therefore, regarding both a and d as constant, and b and c variable, subject to the condition $ad-bc=0$, we shall find by differentiating (3) that

$$\left.\begin{aligned} c&=\sqrt{ad\,.\,\frac{a+f}{d+f}},\\ b&=\sqrt{ad\,.\,\frac{d+f}{a+f}} \end{aligned}\right\} \quad . \; . \; . \; . \; . \; . \quad (7)$$

gives the most sensitive arrangement for measuring a resistance d with a battery whose resistance is f.

It is an evident conclusion that differential galvanometers intended for measuring resistances comprised within wide limits, both high and low, should have coils of long fine wire, having necessarily a high resistance; for a galvanometer with coils of short and thick wire is only suitable for measuring small resistances; whereas if it have coils of fine wire it is suitable for both high and low resistances—for the latter by shunting.

LXII. *On certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat.* *By* W. F. Barrett, *F.C.S., Professor of Experimental Physics in the Royal College of Science, Dublin*[*].

IN the 'Proceedings of the Royal Society' for January 28, 1869, Mr. Gore published the important fact, that when an iron wire is heated to bright incandescence and then allowed to cool, *a momentary elongation*, or, as Mr. Gore believed, diminution of cohesion, of the wire occurs just after it has begun to contract by cooling. The main points in Mr. Gore's paper are as follows:—A thin iron wire fixed at one end to a binding-screw is attached at the other to an index which multiplies any motion of the wire; the wire is strained horizontally by a feeble spring; and matters are so arranged that the wire can be heated by an electric current or by a row of gas-jets. When heated, the wire expands and the spring pulls the index over. A sketch of the instrument is given in the Philosophical Magazine for July 1869. Mr. Gore states that *no* anomalous action is observed on *heating* the wire to bright incandescence; but when the heating is discontinued and cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk or kick, indicating a momentary elongation of the wire during the progress of its contraction. This effect is perfectly certain, and always occurs at this particular temperature. Mr. Gore states that iron wire of a certain thinness and a certain tension of the spring is necessary, and that the phenomenon is apparently *confined to cooling iron*, no such change being evident during the heating or cooling of wires drawn from the wide range of other metals he has examined. Further, Mr. Gore has investigated the production of induced currents during the cooling of magnetized iron bars, one portion of which had been heated to redness; and the result showed that the iron bar "suddenly increased in magnetic capacity during cooling at a particular temperature of moderate red heat."

Having occasion to show Mr. Gore's discovery in the course of a lecture delivered some eighteen months ago to the Dublin

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[*] Communicated by the Author.

Royal Society, Mr. Gore kindly furnished me with his own apparatus. By attaching to the movable cross piece a light mirror, from which a brilliant ray of light was reflected to a scale on a distant wall, the effect sought was not only vastly magnified, but one or two new facts also revealed themselves. (i) During the heating of the wire a slight and momentary retrogression of the beam was noticed at the temperature corresponding to the powerful jerk that occurred on cooling: some smaller tremblings of the beam were noticed at higher and lower temperatures; but these seemed due to irregular heating and cooling. (ii) It was evident that the anomalous deportment of the iron occurred approximately at the critical temperature when iron undergoes its principal magnetic change.

Mr. Gore having stated in a letter to me, written in May 1872, that he had no intention at present of making any more experiments in the direction of his discovery, and adding the subject was quite open to me, I felt at liberty to pursue the inquiry thus suggested. It was not, however, till this autumn that I could find the necessary leisure; and the following results were then obtained.

My best thanks are here due to Professor Guthrie for his hearty welcome to use his laboratory at South Kensington, where the experiments have been conducted.

I.

Employing twenty Grove cells, I have had no difficulty in obtaining this anomalous behaviour with *moderately thick* iron wires. These have the advantage of allowing the effect to be studied more leisurely, the phenomenon sought for occurring several seconds after the interruption of the current. The temperature at which the momentary jerk occurs seems to be lower in thick wires than in thin ones, the critical point being a moderately bright or cherry-red heat in thin iron wire, say No. 23, and a very dull red heat in thick wire, say No. 20; the latter wire is, in fact, in the stage just preceding obscurity when the effect occurs. The internal temperature of the thicker wires is no doubt masked by the cooling of their surface, whereas in thin wires the cooling throughout is extremely rapid, and moreover the transitions of temperature cannot be so well noted.

II.

With No. 21 hard iron wire I have had no difficulty in obtaining the jerk during heating. In this case the movement is in the reverse direction of that which occurs during cooling; that is to say, it indicates a momentary *retraction*, occurring, as closely as can be judged, at the same temperature at which the *elongation* takes place in cooling.

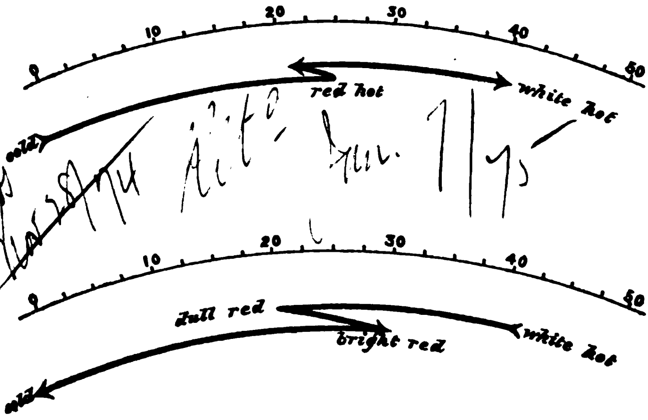
With a steel wire 25 centims. long and No. 22 B wire gauge the following observations were made—a battery of ten Grove cells being used, capable of raising this length of wire to a bright white heat. The index stood at 0 on the scale before contact was made, the wire being cold. After contact, as the wire became heated, the index regularly passed to 24; here it promptly retreated to 22, then steadily passed onwards to 34, the wire now glowing white-hot. Breaking contact, the index returned regularly to 20, then rose suddenly to 27, after which it continued its backward course till it finally rested at 2, the wire now being cold again. The action of the spring stretches the wire when hot, hence the index does not return to zero. Allowing for this stretching, the figures would be proportionally lower where the jerk occurs on cooling, viz. 18 to 25.

Here are two more out of many experiments with the same wire:—

1. Wire cold; contact made; index rose from 0 to 25, jerked back to 23, then rose to 33; wire bright red.
   Wire bright red; contact broken; index fell from 33 to 19, jerked forward to 25, then fell to 4; wire cold.
2. Wire cold; contact made; index rose from 0 to 25, jerked back to 23½, then rose to 32; wire bright red.
   Wire bright red; contact broken; index fell from 32 to 20, jerked forward to 24½, then fell to 4; wire cold.

The following diagram (fig. 1) illustrates the motion of the index on heating and cooling the wire.

Fig. 1.



Sent to Henry Meigs Nov 28/74 Rec'd Jan. 7/75

Releasing the tension of the spring, the forward motion on cooling is, as might be expected, much lessened, whilst the jerk back is scarcely affected. Increasing the tension of the spring, the forward jerk is correspondingly increased and the backward jerk diminishes, and can be made to disappear.

III.

Is this anomalous action, then, due to a momentary change in the cohesion of the wire? If so, at a certain point during the progress of *heating*, the molecules of iron have a sudden *accession* in elasticity, and at an approximately corresponding point during *cooling*, they incur a sudden *loss* in elasticity. If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling*. Fixing one end of the wire and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen; the sudden outward thrust on cooling is very conspicuous. Heating the wire by gas-flames, the same result is given.

All kinds of iron do not exhibit this behaviour; and some show it in a more or less marked degree. I have not been able to detect any change, in heating or cooling, in certain specimens of good soft iron wire; but in hard iron wire, and notably in steel wire, it is very apparent. The wire, moreover, requires to be raised to a very high temperature before the jerk is seen on cooling. I have not observed the momentary elongation on cooling when the wire has only been heated to a point *just beyond* that at which it would otherwise occur. The behaviour of iron wires of different degrees of purity and of widely different thicknesses are points I hope to examine in a subsequent inquiry. I may here also mention that the precise magnetic condition of the iron at the moment at which the jerk occurs, together with its electric resistance and its thermo-electric position†, are questions upon which I have already made some experiments, but not enough to justify the publication of any results at present.

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* A striking lecture experiment may be made by simply stretching some harpsichord-wire between two supports, and heating the wire to whiteness by a current. On allowing the wire to cool, it gradually straightens itself till just as it reaches the point of obscurity, when it suddenly drops for an instant. It is extraordinary that this action has not been frequently observed.

† Professor Tait's remarkable investigation on this point is alluded to subsequently.

IV.

On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation*. Necessarily no change of this kind can be observed on heating; but the reglowing of the wire on cooling is most uniform and conspicuous*. The wire must first be heated to whiteness; and then, being allowed to cool, just as it reaches a point of barely visible redness a sudden cherry-red glow takes place, passing as a wave of heat from one end of the wire to the other, or from both ends to the centre. The measured progress of this wave of temperature along the wire is extremely beautiful to observe. On first sending the current through the wire, the heating begins at one extremity and runs along to the other; on breaking contact, this reheating sweeps along the wire in the contrary direction. This peculiar movement, therefore, may be caused by the unequal thickness of the wire; though I do not think this is the explanation, as the reheating would then move in the same direction as the heating (namely, from the thinner to the thicker parts of the wire), and this is not the case. I hope shortly to investigate this further.

When the wire is heated by a row of gas-flames, the same results take place, although the heating by the battery is a far neater and more satisfactory way.

It is a real accession of temperature, a sudden increase in thermal as well as luminous radiation. This is evident from the following experiment. A wide glass tube (fig. 2) was fitted with corks at each end, so that the iron wire could be enclosed air-tight within the tube. At one end the cork was perforated to allow the insertion of a narrow glass tube bent at right angles, the lower end of which dipped into coloured water. On heating the wire to whiteness by the current, some of the enclosed air was expelled, and on breaking contact the liquid rushed up the tube, but midway suddenly stopped in its course, and was depressed some two inches. At this moment the assistant, who was watching the wire, gave notice the wire drooped and glowed again. There is no difficulty in repeating this experiment, nor in exhibiting its action to a large class. Wherever the momentary expansion of the wire is feeble or absent, there

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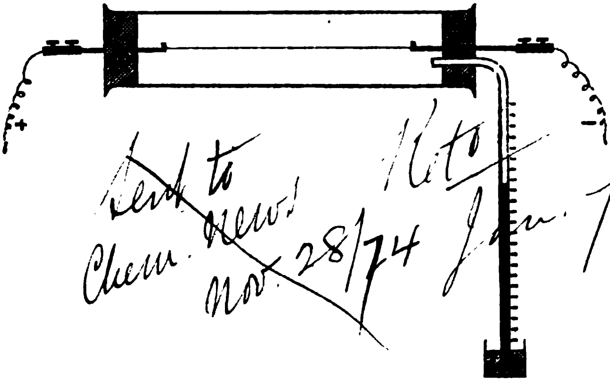
* Nevertheless during heating I thought I detected a momentary pause in the progress of the reddening of the wire, just after incandescence had been reached.

Fig. 2.



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likewise this *recalescence*, as it might perhaps be termed, is also feeble or absent. I am anxious to procure wires of nickel and cobalt; for, from the reasons set forth in another paper, the action might certainly be expected to occur in those metals as well as in iron, probably with nickel at a lower and with cobalt at a higher temperature than in the case of iron.

V.

Besides the molecular changes here detailed, there are indications of the existence of other disturbances during the heating and cooling of an iron wire, notably *the emission of a peculiar dry crackling sound,* like the crepitation that occurs on magnetizing and demagnetizing iron; and this, too, occurs at the critical temperature. But so many matters of interest have arisen during this investigation, that the present paper can only be regarded as the results of a preliminary inquiry.

VI.

The molecular disturbances to which iron is thus seen to be subject, at a particular temperature, are no doubt associated with an even wider range of phenomena than are here indicated. Professor Tait's experiments, which were read by me after most of the foregoing facts had been obtained, show that iron exhibits a most remarkable and anomalous *thermo-electric* deportment at a red heat. In his Rede Lecture* that eminent physicist points out that "the cause of this is that while, as Sir W. Thomson discovered, the specific heat of electricity in iron is negative at ordinary temperatures, it becomes *positive* at some temperature near low red heat, and remains positive till near the melting-point of iron, where it appears possible from some of my experiments that it may again change sign." And further on Professor Tait suggests the idea "that iron becomes, as it were, a different metal on being raised above a certain temperature. This may possibly have some connexion with the ferricum and ferrosum of the chemist, with the change of magnetic properties of iron, and of its electric resistance at high temperatures." Professor Tait adds the interesting fact that he has found an anomalous thermo-electric behaviour in *nickel* similar to that in iron, and, as one might venture to anticipate, at a much lower temperature.

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* Nature, June 12, 1873.

Thus two separate lines of inquiry have converged on the same point—namely, that a profound molecular disturbance takes place in iron at a low red heat. In connexion with future theories of magnetism, this fact is likely to be of considerable importance, inasmuch as it seems probable that this disturbance is confined to the magnetic metals, and that it occurs at or about the temperature when they leave or reenter this condition.

LXIII. *On the Relationship of the Magnetic Metals.*

By W. F. Barrett, *F.C.S.*[*]

THE remarkable similarity in the chemical and physical properties of the magnetic metals has no doubt often attracted attention; but I am not aware that any definite collation of these properties has ever been made. This I propose briefly to do in the following paper. The extraordinary homology these metals are thus seen to exhibit furnishes instructive evidence concerning the molecular state of a magnet.

By magnetic metals I mean those metals which possess magnetic properties under ordinary circumstances—namely, iron, nickel, and cobalt.

First we will compare their physical characteristics. The *specific gravity* of the thirty-eight known metals ranges from lithium 0·59, to platinum 21·5, a difference of nearly 21; whereas the specific gravity of iron is 7·8, nickel 8·3, and cobalt 8·5, an extreme difference of only 0·7. The *specific heat* of these three metals is also nearly identical; and their *atomic heat* is the same. Their *conductivity for sound* is almost absolutely the same; and so far as their heat and electric conductivity have been determined they are also alike. Their *dilatation by heat* is the same, and so also is the amount they lengthen by mechanical strain. They belong, I believe, to the same system of *crystallization*, namely the monometric, though too little is known

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[*] Communicated by the Author, having been read before the British Association at Bradford, September 1873.

LXIV. *Intelligence and Miscellaneous Articles.*

ON VARIOUS CASES OF INTERMISSION OF THE VOLTAIC CURRENT. BY A. CAZIN.

IN pursuing the researches which I have undertaken on the heat of electromagnets, I have had occasion to observe several cases of intermission of the voltaic current, which, I believe, have not yet been brought into notice.

Experiment 1.—A voltaic circuit is formed of 20 average Bunsen elements and a coil of 960 turns containing an iron tube 8 centims. in diameter and about 1 millim. thick. It can be closed or opened at will by means of a platinum point and a layer of mercury, which communicate respectively with each of the rheophores.

When the platinum is not in contact with the mercury, and they are put into communication with the armatures of a condenser with a glass plate (armed surface 3 square metres), a continued rustling is heard in the iron nucleus. The same effect is produced when, the condenser being omitted, a layer of alcohol is interposed between the mercury and the platinum point. The noise ceases when the alcohol is removed so that the platinum and mercury are separated by a layer of air, and also when the point is dipped into the mercury.

These facts indicate that the current passes through the glass in the first case, and through the alcohol in the second, and that *its passage is intermittent.* The nucleus of iron undergoes a rapid succession of alternate magnetizations and demagnetizations; and each demagnetization occasions a faint sound in the nucleus. The rapid succession of these noises constitutes the rustling which is heard.

When the iron nucleus resounds, a galvanometer indicates only a continuous current. In fact it cannot indicate any thing else when the intermissions are very close to one another.

I think that the cause of the intermission is the condensing action of the glass and the alcohol; when the two faces of the insulating body, which are in contact with the rheophores, have acquired a certain electric potential, a discharge takes place through the insulating layer; the magnetism of the nucleus increases during the charging of the condenser, and diminishes during its discharge; the noise is produced during the diminution of the magnetism; after each discharge a certain time elapses before the condenser is recharged; and the same phenomenon is reproduced indefinitely.

It is readily ascertained that the nucleus resounds during the diminution of its magnetism: we have only to immerse the platinum point in the mercury, and then withdraw it; at the moment when the spark issues at the point of interruption, a relatively intense sound is heard in the nucleus. It is solely the rupture of the cirleast in my apparatus.

M. de la Rive, in 1843, discovered that a current *interrupted by means of a rheotome* produces a sound in the iron of an electromagnet; but I think that the phenomenon I have just described has not been previously signalized.

Listening for a sound in the nucleus of an electromagnet may be regarded as a new process of investigation; we have seen that it reveals the intermission of the current in circumstances in which the known methods are insufficient. I will mention an instance which shows that this method accords with the others when they can be employed simultaneously.

Experiment 2.—When the spark from the rupture of the preceding circuit is observed with the aid of the revolving disk, after the manner made known by me to the Academy on the 7th of April last, the spark appears *compound.* When it bursts in alcohol, and the platinum point and the mercury are connected with the armatures of the condenser, the revolving disk shows that the spark is composed of four or five successive bright strokes; the intervals between the strokes diminish from the first onwards. (I have already described the division of this spark by another process, *Bulletin de la Société Philomathique*, 13th May, 1865; and the journal *l'Institut*, 31st May, 1865.)

The sound produced by the rupture-spark presents a similar mode of division; it is the same with that which is heard in the condenser, and with that which takes place in the nucleus: these three sounds are *composed* exactly in the same manner.

The production of a sound in the condenser proves that there is a partial discharge through the insulating substance, although at no part does this appear to be pierced.

The sound of the condenser is augmented by increasing its surface, up to a certain limit which cannot be exceeded. At the same time the spark between the mercury and the platinum point is seen to diminish. These modifications indicate a change in the distribution of the electricity—which could be mathematically analyzed by regarding the alcohol of the interruptor as the insulating plate of a second condenser united to the first by the armatures of the same sign.

I think there is no essential difference between the intermission of the current which accompanies the breaking of the circuit in the circumstances I have just described, and that presented by my first experiment.

Experiment 3.—The platinum point of the mercury interruptor is screwed into a fixed nut, so that it can be raised or lowered. The mercury and the point communicate respectively with the armatures of a condenser of 1 square metre surface. The rest of the circuit is arranged as before.

The point being dipped into the mercury, is gradually raised until the spark springs through the alcohol. The point then remaining fixed, a succession of sparks is set up and persists for a long time. These sparks are lively and sonorous, and can readily be counted.

The level of the mercury visibly oscillates beneath the point. A possible cause of the oscillation is:—the spark being formed by the vapour of the mercury, the elastic force of this vapour depresses the level of the liquid; this returns to its former level, passes it in virtue of its acquired velocity, and rejoins the platinum point. Falling again, the mercury produces a fresh interruption, and the same phenomenon is repeated.

This purely mechanical cause cannot be the only one; for the circumstances favourable to this new mode of *automatic interruption* are those which accompany the decomposition of the rupture-spark into a small number of successive bright strokes. This correlation is recognized by changing the extent of the condenser, which alters the number of the divisions of the spark. Thus, on continually diminishing the surface of the condenser, the sparks are seen to follow one another more and more rapidly; and finally, when the condenser is omitted, there is only a crepitating voltaic arc. It is probable that the oscillation-period of the mercury comprises a definite number of intermissions in the discharge of the condenser, and that these two causes are in mutual dependence.

I believe that the discharge through air, under the form of a voltaic arc, and the discharge through glass, of which experiment 1 supplies an example, are comparable, and that the well-known crepitations of the voltaic arc are due to the same cause as the phenomena of which I have just spoken. All these facts may be brought into connexion by a single proposition: *the insertion of a suitable resistance in the voltaic circuit determines the intermission of the current.* The laws of the intermission will have to be studied with the interposition of a condenser, because the periods are long enough to be observed with facility. The laws found in this manner will afterwards be generalized, and must conduct us to the known laws of the currents which are regarded as continuous.

We cannot omit to derive from the whole of these considerations the important conclusion that *the current is a succession of modifications accomplished periodically in the circuit.—Comptes Rendus de l'Acad. des Sciences*, vol. lxxvii. pp. 1095–1098.

EXPERIMENTS ON EVAPORATION. BY M. STEFAN.

Atmometric experiments have hitherto led to no results expressible in the form of laws. The conditions under which they were made were not simple enough; yet they were sufficiently varied. The science of evaporation, especially of the diffusion of vapours, remained an unoccupied field.

to avoid the great lowering of the temperature at the surface, narrow tubes were chosen for evaporating-vessels, instead of the wide vessels hitherto usual.

1. *The velocity of the evaporation of a liquid from a tube is inversely poportional to the distance of the level of the liquid from the open end of the tube.* This law holds with rigorous exactness when the distance of the level a little exceeds 10 millims.

2. *The velocity of the evaporation is independent of the diameter of the tube.* This result was obtained from experiments with tubes the diameter of which varied from 0·3 to 8 millims.

3. The velocity of the evaporation increases with the temperature, so far as with this the vapour-pressure of the liquid rises. If p be the maximum of elasticity of the vapour corresponding to the temperature of the observation, P the atmospheric pressure under which the liquid evaporates, *the velocity of the evaporation is proportional to the logarithm of a fraction of which* P *is the numerator, and* P—p *the denominator.* If the pressure of the vapour becomes equal to that of the air, this logarithm becomes infinitely great, and signifies that under this condition the liquid boils.

Experiments were also made on evaporation in closed tubes.

If the open end of a tube, the other end of which is closed, be dipped in ether, bubbles form and issue continually from the tube, and, *at first, the times in which successively equal numbers of bubbles form are proportional to the odd numbers.*

If the immersed tube contains hydrogen instead of air, the same number of bubbles form in one fourth of the time. *Evaporation proceeds in hydrogen four times as rapidly as in air.*

The same result was also furnished by an experiment in apparatus in which a liquid can be brought to evaporation in an open tube in various gases. It consists of a T-shaped glass tube; into its vertical arm enters the tube containing the liquid to be evaporated; through the horizontal cross-piece a continual current of the gas is conducted.

If a tube provided with a cock be dipped with the cock open in ether, the level of the liquid within the tube will sink below that outside; and, *at first, the depths to which the interior level sinks below the exterior in definite times are as the square-roots of those times.*—*Sitzung der math.-naturw. Classe der Kaiserl. Akad. d. Wissensch. in Wien*, Oct. 23, 1873.

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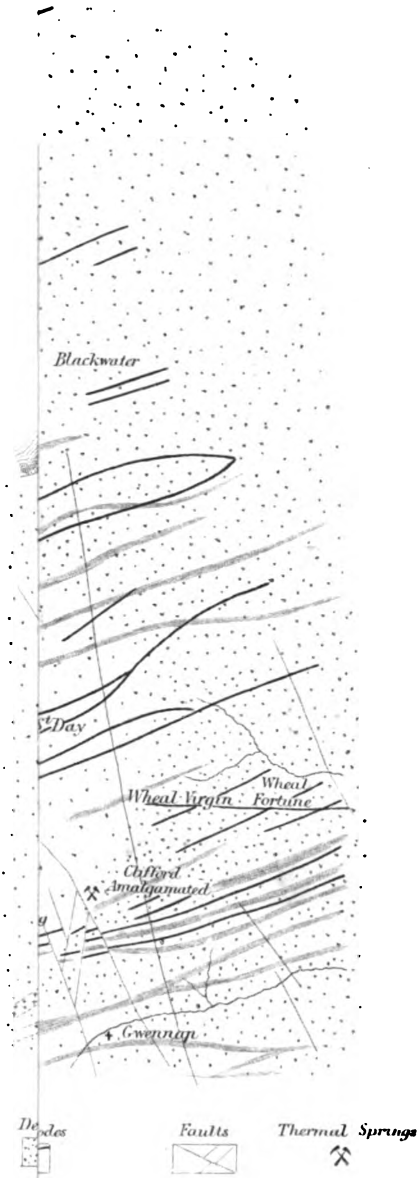

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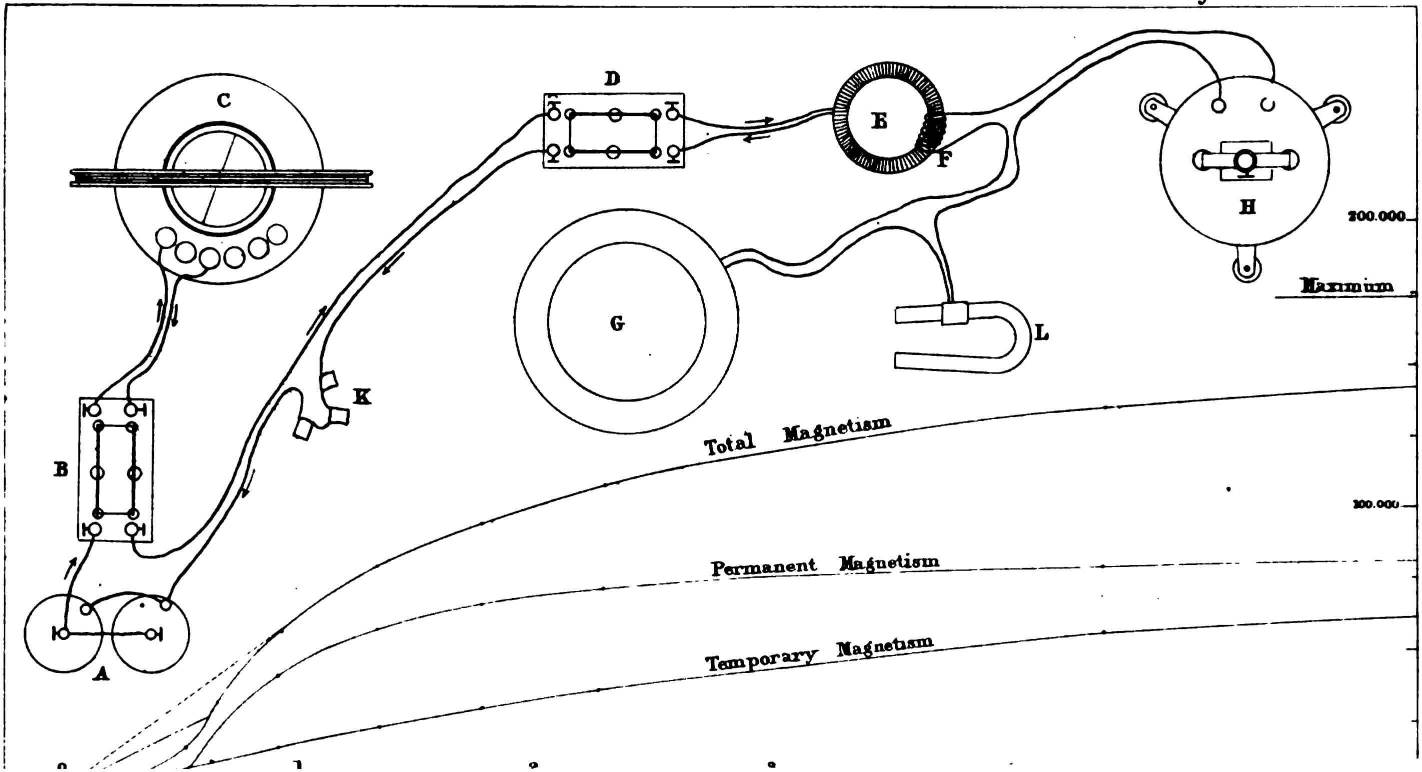
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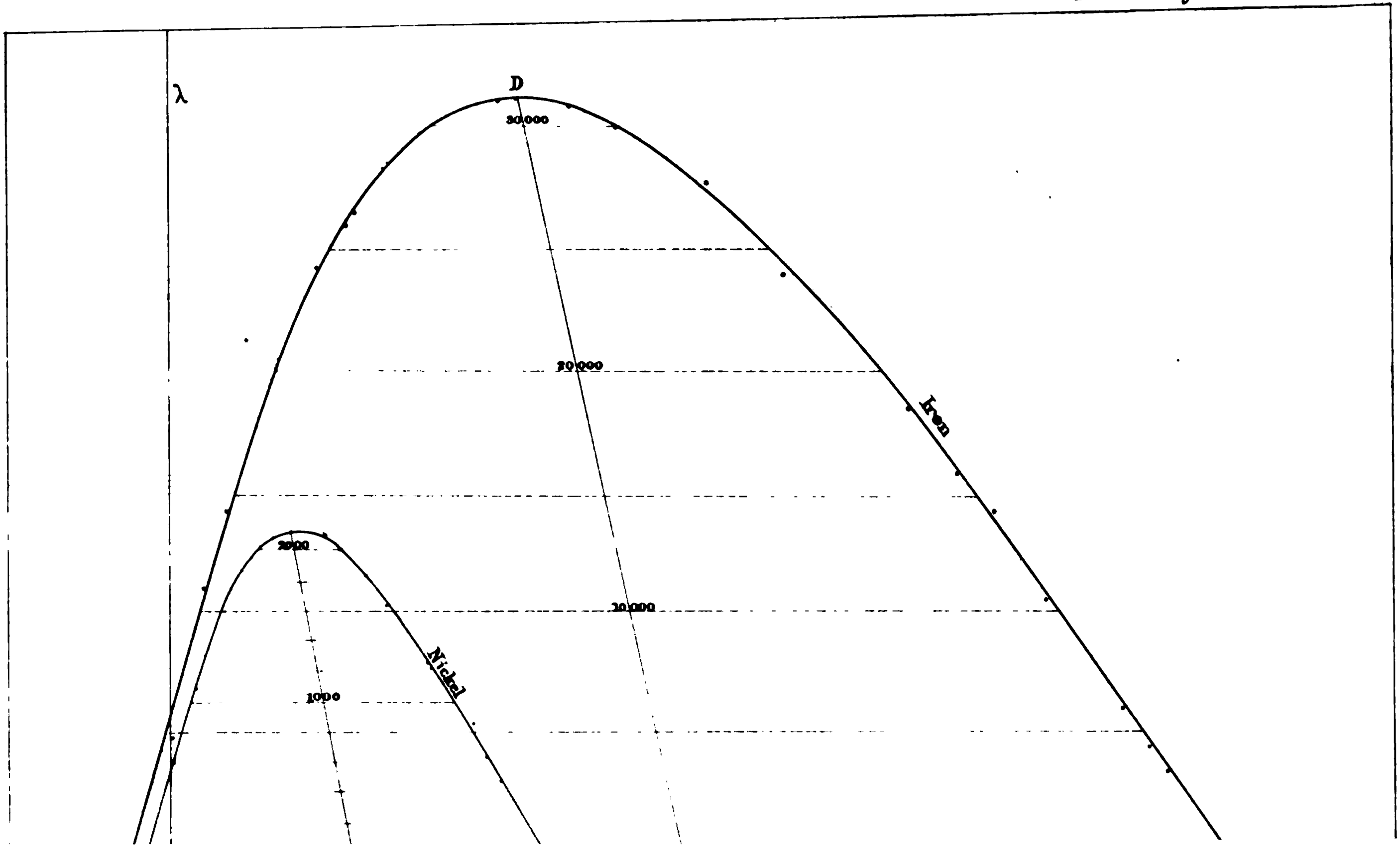
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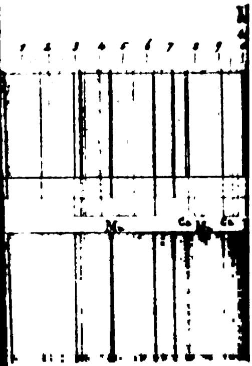
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